The Collected Works of Peter Razzell

An Anthology

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The History of Myddle

by RICHARD GOUGH

CALIBAN BOOKS

PREFACE

Professor Hoskins has written in the facsimile original edition of *The Antiquities and Memoirs of Myddle:*

> "Gough's *History of Myddle* . . . sounds like the narrowest kind of parish-pump history one could possibly imagine, of interest only to devoted local historians in Shropshire. It is in fact a unique book. It gives us a picture of seventeenth-century England in all its wonderful and varied detail such as no other book that I know even remotely approaches. If History is, as has once been said, the men and women of the past talking and we overhearing their conversations, then Gough's history of his native parish, written between the years 1700 and 1706, is History . . . A whole countryside, an entire society, comes alive in our minds, in a way that no historian, however skilled, can possibly evoke . . . this remarkable book is . . . one of the most entertaining books ever written in English, unique in our literature."[1]

Given the outstanding quality of Gough's work, why is the book not more widely known amongst historians and the general public? The answer lies probably in the nature of the original edition — not only is some of its content antiquarian in nature, but so is much of its style and lay-out. The aim of the present edition is to eliminate material of purely antiquarian interest, and to re-arrange presentation and style of the original — in particular spelling, which has been modernised throughout — so as to make it much more accessible to the modern reader. I have retained all biographical material, as it is the biographies which give the book its central fascination. No alterations have been made to Gough's language, for that is a part of the delight of his writ-

ing. Readers who wish to know more about the original are fortunate in having it readily available in the facsimile edition.

In my introduction I have sought to bring out the quality of Gough's writing — particularly the stories and anecdotes about his contemporaries — by quoting extensively from the text. I have discussed the book from the point of view of the social historian and historical sociologist, and have compiled a detailed subject index, so that anyone who wishes to know more about marriage, the family, the treatment of children, disease, violence, drunkenness, religion, love and a host of other topics in the seventeenth century, can turn to the index at the back of the book. But its main importance is Gough's unique history of a seventeenth century village community, bringing to life his contemporaries in such a vivid and entertaining fashion.

INTRODUCTION

Myddle is in Shropshire near the Welsh/English border, and had a population of about six hundred people at the end of the seventeenth century. It was situated in a woodland area and its economy was almost entirely agricultural, with a heavy emphasis on cattle-rearing; most of its population were small freeholders or tenant farmers, although by the time Gough wrote his book nearly a third of the men of the village had become labourers. Today Myddle is a quiet, peaceful place, a typical English country village. The idealisation of the countryside has led many to see this peacefulness as the dominant historical characteristic of village life, the title of one of Flora Thompson's books — *Still Glides the Stream* — perhaps epitomising this feeling. The romantic treatment of the English countryside has buttressed this image, and there is much in current ideology which points to a harmonious and serene traditional rural community, in order to condemn the perceived violence and disintegration of modern urban life. Gough's writing completely shatters this picture of a rural idyll, but in doing so, enriches our appreciation of the reality of our social history in a uniquely instructive way. Here is Gough on a sequence of events that occurred in Myddle and its neighbourhood:

> There was one Clarke, of Preston Gubballs, who had formerly been tenant to Sir Edward Kinaston, of a tenement in Welsh Hampton, and was indebted for arrears of rent, due to Sir Edward; whereupon he sued out a writ against this Clarke, and sent a bailiff to arrest him; and because Clarke had some lusty young men to his sons, therefore Sir Edward sent one of his servants to assist the bailiff, if need were. Clarke was cutting peat on Haremeare Mosse; Sir Edward's man stayed in the wood in Pimhill; the bailiff went towards Clarke, and being beaten back by Clarke's sons, Sir Edward's man came

> with his sword drawn, and swore he would make hay with them. But one of Clarke's sons, with a turf spade, which they call a peat iron (a very keen thing) struck Sir Edward's man on the head, and cloave out his brains. The bailiff fled; Clarke was rescued; and his son fled, and escaped. The coroner was sent for and by appointment of Sir Humphry Lea, the inhabitants of Myddle paid the coroner's fees. Clarke's son escaped the hand of justice, but not the judgment of God, for he that spilled man's blood, by man shall his blood be spilt, for when all things were quiet, and this thing seemed forgotten, Clarke's son came into this country again, and lived at Welsh Hampton, where a quarrel happening between him and one Hopkin, his next neighbour, about their garden hayment, as they stood quarrelling, each man in his own garden, Hopkin cast a stone at Clarke, which struck him so directly on the head, that it killed him. How Hopkin escaped the law, I have not heard; but vengeance suffered him not long to live, for a quarrel happened between him and one Lyth, a neighbour of his, as they were in an alehouse in Ellesmere, in the night-time, which quarrel ended in words, and Hopkin went towards home; and not long after Lyth went thence. The next morning Hopkin was found dead in Oatley Park, having been knocked on the head with the foot of a washing stock which stood at Ellesmere meare, which foot was found not far from him. Lyth was apprehended, and committed to prison on suspicion of the murder . . .

Three men were killed, two of the killers themselves being murdered in turn. The first homicide occurred because of a dispute over non-payment of rent, the second because of a garden quarrel, and the third as a result of an alehouse brawl. All the disputes were in themselves trivial, and what is remarkable is that three such killings should be linked one to the other in such a small community. These were not isolated incidents however, as Gough mentions a total of ten homicides in the course of his narrative, and although these did not all occur in Myddle itself, it is inconceivable that such a level of violence could occur in a modern rural community

or an urban area of equivalent size. But before I go on to discuss comparative homicide rates, I wish to illustrate the nature and type of violence in this seventeenth century rural area by further quoting from Gough's narrative. The following account of a murder of a young servant maid has tragic and comic qualities, and reminds us that Gough was both a contemporary of Pepys and lived near, both in space and time, to Shakespeare himself. The murderer's name was Hugh Elks, and

> he was an ill man — for he, knowing that a neighbour of his who lived in Eyton had a considerable sum of money in the house, this Elks and some other of his companions came to Eyton on the Lord's day at time of morning service, and having visors on their faces, they came into the house and found there only one servant maid who was making of a cheese, and this Elks stooping down to bind her she saw under his visor, and said, "Good Uncle Elks, do me no harm," and upon that he pulled out his knife and cut her throat. His companions being terrified at the act fled away to Baschurch Church, and Elks seeing his companions were gone fled likewise and took no money, and for haste shut the door after him and left his dog in the house, and came to Marton, but stayed not there, but ran to Petton to church whither he came sweating exceedingly a little before the end of service.
>
> When people came from church to Eyton, they found the girl dead, and Elks' dog in the house almost bursting with eating the cheese. They followed the dog, who brought them to Elks' house, and upon this, Elks was apprehended on suspicion.

We will see later in this introduction that theft was common in seventeenth century Myddle, although the above incident seems to have happened in the sixteenth century. "Good Uncle Elks" was presumably not a relative of the maid servant's, but the term was an adopted one (made familiar by anthropologists), i.e. was an expression of a particular kind of

a close community relationship. This murder like the three previously discussed was a crime of passion, enacted in the heat of the moment out of spontaneous feelings of rage and aggression. The people of Myddle were capable however of much more deliberate, dispassionate and cold acts of murder, as is shown by an anecdote of Gough's about the attempt of three Myddle wives to rid themselves of their husbands through poisoning. A certain Thomas Hodden, husband of Elizabeth Hodden

> died, leaving his wife a young wanton widow, who soon after married with one Onslow, a quiet, peaceable man; but she soon grew into dislike of him, and was willing to be shot of him. There were other women in Myddle, at that time, that were weary of their husbands, and it was reported that this woman and two more made an agreement to poison their husbands all in one night; which (as it is said) was attempted by them all; but Onslow only died; the other two escaped very hardly. This wicked act was soon blazed abroad and Elizabeth Onslow fled into Wales, to her father's relations; but being pursued, she was found upon a holiday, dancing on the top of a hill amongst a company of young people.

In spite of this being a description of a murder, the reader cannot but be fascinated by the account of Elizabeth Onslow "dancing on the top of a hill amongst a company of young people" when apprehended. So even here where the quality of deliberateness is to be found, the spontaneity of her reactions in the aftermath has a very seventeenth century ring.

There was only one other murder of the total of ten that could be described as cold-blooded, and this involved another member of the Elks family.

> There was one Thomas Elks, of Knockin, who had an elder brother, who married and had one son, and soon after died and his wife also, and left the child very young.

> The grandmother was guardian to the child. This grandmother was mother unto Thomas Elks, and was so indulgent of him, that she loved him best of any of her children; and by supplying him with money to feed his extravagances, she undid him. But when she was gone poor, and could not supply him, he considered that this child stood in his way between him and the estate, and therefore contrived to remove him: and to that end he hired a poor boy, of Knockin, to entice the child into the corn fields to gather flowers. The corn was then at highest. Thomas Elks met the two children in the fields; sent the poor boy home, and took the child in his arms into the lower end of the field where he had provided a pail of water, and putting the child's head into the pail of water he stifled him to death, and left him in the corn.

But much more typical of homicide in Myddle was the following incident. A young maid was a

> servant to a gentleman who lived near Wellington, and as this young woman was holding water for her master to wash his hands in the kitchen, he cast a little water from off his finger into her face, which her mistress (who was present), seeing, and conceiving it too familiar an action, she in a rage took up the cleaver, and gave her such a blow on the head that she died.

This was the only other murder committed by a woman in Gough's account; like today, most murder and physical violence was committed by men. But the homicide rate was much higher for both men and women in the seventeenth century than it is today. It is impossible to calculate the rate for seventeenth century Myddle with any precision, as Gough does not always tell us when murders took place, and whether all the victims were living in Myddle at the time. According to recently published work, the homicide rate in thirteenth century England was in the range of 9 - 47 annual homicides per 100,000 population,[2] while other research indicates a rate for the sixteenth/seventeenth century period of 5 - 18 per 100,000.[3]

The rate for Myddle appears to have been as high as that found for the thirteenth century, but whatever the precise levels of homicide, it is clear that they were very much higher in all these periods than they are today. The homicide rate in Great Britain during the period 1930 - 59 was 0.4 per 100,000, and there has been little change in recent years.[4] Thus homicide in pre-industrial England — the thirteenth to the seventeenth century — was at least ten times as great as it is today, and may have even been a *hundred* times at particular periods. Certainly the number of violent murders described by Gough for his small rural community confirms the findings of research based on more statistical techniques.

Violence did not of course always result in death, and Gough describes a number of aggressive incidents of a non-fatal kind. He often mentions them in passing as if they were fairly commonplace, and almost murderous attacks were treated as if they were merely everyday incidents. An example of this occurred when Robert Morrall met his father-in-law William Tyler:

> Old William Tyler was his utter enemy, and often threatened to be his death, but Morrall was too hard for him. They met accidentally at a stile in Houlston, and discoursing friendly, they sat down on each side of the stile; but Tyler having a halter in his hand, cast it about Morrall's neck and drew him over the stile, and was likely to have hanged him: but Morrall by his strength and agility freed himself, and did not forbear to beat Tyler severely.

Tyler was obviously a very violent man who was capable of the most extreme acts of aggression, although he never actually murdered anyone as far as we know. But this violence was not limited to a few individuals, but was culturally sanctioned and at times could explode so as to almost engulf and involve the whole community. Gough was fascinated by

Tyler's personality and gave several pages to his exploits and personal history; the following incident described at length illustrates the communal nature of violence. Tyler owed money to a Mr. Bradocke, who had unsuccessfully attempted to serve a warrant on him.

> Afterwards Mr. Bradocke sent his tenant, William Byron (a little man, but stout of his hands), to serve Tyler with another warrant. Byron came (upon Sunday) to Myddle Church to morning prayer (for in those days all writs and processes might be served on the Lord's day). William Tyler came to church with a good backsword by his side, which then was not usual. After service, Byron stood at the church stile; and as soon as Tyler was gone over the stile, Byron leapt on his back, and cast him down. Many of Tyler's companions, and some women of his relations, came to rescue Tyler; but the high constable, Mr. Hatchett, a bold and discreet man, was present, some say on purpose, and he quieted the people. Roger Sandford, of Newton (who married Mary Bradocke, aunt to Mr. Bradocke), was there, with his servants and friends, to assist Byron; and one William Hussey, servant to Roger Sandford, came to assist Byron; and Tyler got Hussey's thumb in his mouth, and worried all the flesh to the bare bone: but Hugh Suker, a weaver, standing by with a pike-staff in his hand, put the pikes into Tyler's mouth and wrenched open his teeth, and released Hussey. At last Tyler was set on horseback, and Byron leapt up behind him to hold him there, and William Hussey led the horse, and thus Tyler went toward the jail. But the consternation and lamentation of Tyler's friends, especially the women, was such as I cannot easily demonstrate . . .
>
> All the company followed William Tyler out of town; and at the town's end there, upon a bank near the pinfold, stood John Gossage and several others of Tyler's drunken companions, with a pailful of ale. Gossage cried, "Ah, Will! art going to the jail?" Tyler said, "It is too true." Then says Gossage, "Come, boys; fall on!" but Tyler cried, "Hold, hold. It is to no purpose;" so they took him away. When they came a little below the Lea Hall, the miller of the windmill met them, carrying a sword on his shoulder, with the hilt behind him; Tyler

> put his hand in the hilt of the sword and drew it out, and struck at Hussey; but Byron soon pitched him beside the horse, and took the sword from him. Byron would not give the sword to the miller; and Hussey carried the naked sword in his hand, and led the horse; and so Tyler was brought to jail.

The story speaks for itself and is so rich in detail, that we can only touch on some of its sociological implications. The explosion of violence was contained by the presence of the high constable, although Tyler himself stopped his friends from using violence on his behalf after he had been arrested. We are in a different cultural world to that of today; Gough's world is that of Shakespeare's, a world that has not yet been "civilised", a world in which the Englishman of today — polite, tolerant and non-violent — would find very frightening. But Gough's social world is one of blood and roses — violence, but also of lamentation, loyalty, sadness and love — social intensities which English communities of today certainly lack. With Gough we are not in Freud's world of civilization and its discontents, but are in an era of passionate acting out of impulse and feeling. The language is rich in colour and feeling, and there are passages in Gough's writing which could be mistaken for the work of Shakespeare.

Freud believed that the acting out of intense feelings of violence was associated with a relative absence of neurosis, in particular freedom from clinical states of depression and melancholy.[5] This is based on the theoretical assumption that aggression not expressed outwardly is invariably turned inwards against the self, and that feelings of depression are the result of self-punishment and self-hatred. Several sociologists — including Durkheim — have pointed to the inverse correlation between homicide and suicide rates, i.e. the more murder, the less suicide, and vice versa.[6] This conclusion has come in for a certain amount of criticism in recent years,

mainly on the grounds that such an inverse correlation does not hold in some societies studied.[7] However, most of the exceptions are for non-European societies in which additional cultural factors appear to be acting to complicate the analysis. In European societies Freud's theory seems to fit rather well, and in particular, Catholic countries have (at least until very recently) high homicide but low suicide rates, and Protestant countries the reverse. Seventeenth century England was still "Catholic" from this point of view, and certainly much of Gough's book could easily be mistaken for a description of Ireland and its historical culture until very recently. There were only two definite cases of suicide in Myddle as described Gough, although there was a third ambiguous case of a man who was suffering from grief due to his brother's death, who was soon afterward found dead in a well in his garden. Even if we count this as a case of suicide, the rate seems to have been very low compared to modern experience. Suicide rates were quite low generally in England in the pre-industrial period — varying between 0.6 and 4.0 annual suicides per 100,000 population,[8] compared to about 9.0 per 100,000 today. Whereas suicide is about ten times as common as homicide today, in Myddle homicide was about four times as common as suicide, and this was probably fairly typical of the country as a whole.

The suicide that did occur in Myddle seems to have been linked with violence, as is seen in the following case, which was one of the two unambiguous cases. A certain Clarke was son in law to Richard Wolph, and Clarke's wife having died he,

> by fair and flattering speeches, persuaded the old man to deliver all his estate to him, on condition of being maintained while he lived. Clarke having now got an estate, followed his old way of drinking; and when he came home drunk, he would so abuse the old man, that he made him a weary of his life; and, therefore, in a

> melancholic fit of grief, he went on foot to Wem, and bought poison, which he eat up as he came homeward; and when he came home he was extremely sick, and vomited exceedingly: he told what he had done, and would fain have lived; but no antidote could immediately be had, so he died. The coroner's inquest found him a *felo de se;* and he was buried on Myddle Hill, at that crossway where the roadway from Ellesmere to Shrewsbury, called the Lower-way, goes over cross the way that goes from Myddle toward the Red Bull, but was removed next night: and some say he was interred in a rye field of his own, which is over against John Benion's, in that corner of the piece next the place where Penbrook's gate stood.

The traditional practice of burying a suicide at the crossroads was followed in this instance, although the corpse was reburied privately the following day.

Why was there so much violence in Myddle and other seventeenth century English communities? One answer perhaps can be found in the sanctioning of violence by the government of the day and the relevant local authorities; hanging was of course practised and two of the ten persons responsible for the homicides mentioned by Gough were dealt with in this way. The possible deterrent effect of hanging must have been weakened by the frequency with which murderers escaped this form of punishment: two of the ten escaped detection, three successfully pleaded benefit of clergy — which in effect was a privilege of the rich — one languished in prison until released by the parliamentary authorities during the Civil War, and the fate of two is unknown. There is little evidence anyway that hanging or capital punishment has any deterrent effect, and the violence sanctioned by the authorities is more likely to have increased homicide. Several hangings are mentioned by Gough, but they are usually for quite trivial offences such as horse stealing, theft, and in one particularly pathetic case, a boy was hung for helping in a prison escape. Institu-

tions such as the pillory helped encourage violence; this can be illustrated by the treatment of one Clarke, a Roman Catholic, who had been heard to utter threatening statements about the Church of England. After having been put in the pillory

> The people, by pelting him with eggs, turnips, carrots, stones and dirt, used him so hardly, that the under-sheriff took him down, for fear he should be killed outright. The people followed him to the jail door, and pelted him all the way. He lay some while sick and sore at Shrewsbury, and after he was brought to Ellesmere and there put to stand on the pillory, where he found the like favour from the under-sheriff, and the like hard usage, or worse, from the people; and hereupon the high sheriff wrote a letter to the judge, and acquainted him what he had done, and with all told him, that he could promise to put Clarke upon the pillory at Oswestry, but could not promise to bring him alive from amongst the enraged Welshmen; and thereupon the rest of the punishment was remitted.

Another factor in the high level of violence was almost certainly the amount of drunkenness and general consumption of alcohol. At least three of the ten homicides involved very heavy drinking, and we have seen how violent incidents of the kind associated with William Tyler and his friends were linked with drunkenness. Gough's pages are full of accounts of drunkenness and alcoholic drinking, the first alone having twenty-three entries in the subject index. Mentions of alehouses and inns proliferate, and a common theme is the economic ruin of families and individuals through debt on account of drink. Drinking was not confined to men, and there are several references to women going to the local alehouses, some obviously on a day-to-day basis (women appeared to have been free of some of the social constraints imposed on them

in the later Victorian period — Gough himself admired women of "masculine spirit"). He moralizes on occasions about the evils of drink, but was capable of great sympathy for certain individuals partly ruined in this way. The following story shows him at his best, weaving a delightful mix of the comic and tragic, revealing at the same time a central feature of seventeenth century social life.

Thomas Hayward the second was a handsome gentleman, a good country scholar and a pretty clerk. He was a person well reputed in his country and of a general acquaintance. He was just and faithful in affirming or denying any matter in controversy, so that less credit was given to some men's oath than to his bare word. He was well skilled in the art of good husbandry. His father left him a farm of thirty pounds (fee simple) in Newton-on-the-Hill and the lease of this farm in Balderton. He had eight pounds (land in fee simple) left him by an uncle in Whixhall. He married with Alice, the daughter of Mr. Wihen, high school master, in Shrewsbury. He had a good fortune with her in money, besides houses in town of considerable yearly value. She was a comely woman, but highly bred and unfit for a country life, besides she was shrewd with tongue, so that they lived unquietly and uncomfortably, and their estate consumed insensibly.

He had little quietness at home which caused him to frequent public houses merely for his natural sustenance, and there meeting with company and being generally well beloved he stayed often too long. His intimate friend was Mr. Hotchkins of Webscott, and indeed there seemed to be a natural sympathy between them for they were both of them very just honest persons and well beloved — but their deportment when they were in drink was very different for Mr. Hodgkins could go but not speak, and Mr. Hayward could speak as well and seemed to be more acute and witty in his drink then at other times but could not go.

This Thomas Hayward sold and consumed all his estate and was afterwards maintained on charity by his eldest son.

Addiction to drink and the local ale-house was not confined to the poor and the culturally rough; in fact the distinction between a respectable middle class and a rough working class did not properly emerge until the nineteenth century.[9] The segregation of social classes also probably did not arise until the same period, and the easy relationship between people of different social statuses was partly a function of cultural spontaneity (including drinking) mentioned earlier. An example of this lies in the relationship between Thomas Jukes and Sir Humphrey Lea.

> Thomas Jukes was a bawling, bold, confident person; he often kept company with his betters, but showed them no more respect than if they had been his equals or inferiors. He was a great bowler, and often bowled with Sir Humphrey Lea at a bowling green on Haremeare Heath, near the end of the Lea Lane; where he would make no more account of Sir Humphrey, than if he had been a plough-boy. He would ordinarily tell him he lied, and sometimes throw the ball at his head, and then they parted in wrath. But within few days, Sir Humphrey would ride to Newton, and take Jukes with him to the bowls; and if they did not fall out, would take him home and make him drunk.

The familiar mixture of aggression, drunkenness and sociability is to be found in this anecdote. It also illustrates the relative social openness of a community like Myddle, and this may have been partly a function of it having been in a woodland area. Contemporaries believed that woodland communities were particularly prone to violence; for example, Norden wrote that "the people bred amongst woods are naturally more stubborn and uncivil than in the champion counties", and Aubrey saw the woodlanders as "mean people (who) live lawless (with) nobody to govern them, they care for nobody, having no dependence on anybody."[10] This was because settlements were scattered in woodland areas — there was a

total of seven townships (hamlets) within the parish of Myddle, plus the chapelry of Hadnall — and they tended to have a large number of freeholders and independent small farmers; this can be contrasted with champion villages, where the population tended to be concentrated into a single nucleated village under the control of the local squire.

An additional factor in the case of Myddle was that it was a marcher lordship, created to deal with border violence between the Welsh and the English. The marcher lord was given certain summary legal and military powers, including the power of immediate execution of Welsh raiders and criminals transgressing local laws. This institution was no longer in being when Gough was writing, but it may have left a tradition of violence in its wake. An instance of this was the heriot custom in lordship marches; the heriot on entering the lease of a farm was "the best weapon" — and the availability of personal weapons was associated with many of the incidents of violence described by Gough.

But the use of personal weapons in violence was not confined to border areas and they were worn almost universally at about this time. At the end of the sixteenth century William Harrison wrote:

> ". . . seldom shall you see any of my countrymen above eighteen or twenty years old to go without a dagger at least at his back or by his side . . . Our nobility wear commonly swords or rapiers with their daggers, as doth every common servingman also that followeth his lord and master."[11]

Little is known about the history of personal weapons — as far as I know virtually no research has been done on this important social historical subject — but it is probable that the wearing of such weapons declined mainly in the eighteenth century. This appears to have coincided with a dramatic fall in the

homicide rate,[12] and both probably began to decline at the very beginning of the eighteenth century after Gough had completed his work. I suspect it is no accident that this was the period when the industrial revolution was getting underway, although what was cause and what was consequence is difficult to disentangle. Such a major topic is clearly beyond the scope of this introduction, although we might notice in passing that the decline of homicide and the outward expression of aggression occurred at the same time as the growth of puritanism (in particular Methodism), which Weber saw as instrumental to the development of capitalism.[13]

One special factor in the creation of violence during Gough's lifetime was of course the Civil War. Gough gives a number of accounts of incidents in the Civil War, some of which were based on personal experience, and it is this personal flavour which brings to life so vividly his narrative. An example of this was when he witnessed Robert More trying to recruit men for the king's army:

> I was then a youth of about eight or nine years of age, and I went to see this great show. And there I saw a multitude of men, and upon the highest bank of the hill I saw this Robert More standing, with a paper in his hand, and three or four soldier's pikes, stuck upright in the ground by him; and there he made a proclamation, that if any person would serve the king, as a soldier in the wars, he should have fourteen groats a week for his pay.

It is often because Gough knew the participants — or at least knew of them — that he was able to bring out the human side of a war which has often been treated in an abstract fashion. Listen to the following description of an incident between royalist and parliamentary forces; a certain Scoggan was made governor of a garrison placed at Abright Hussey:

> I remember the soldiers fetched bedding from Newton for the use of the soldiers there. They took only one

> coarse bed hilling from my father. A party of horse, of the parliament side, came on a Sunday, in the afternoon, and faced this garrison, and Scoggan, standing in a window, in an upper room, cried aloud, that the others heard him say, "Let such a number go to such a place, and so many to such a place; and let twenty come with me:" (but he had but eight in all in the house). And Scoggan, seeing one Phillip Bunny among the enemies, who was a tailor, born in Hadnall, he took a fowling gun, and called to Bunny, and said, "Bunny, have at thee!" and shot him through the leg, and killed his horse. The parliament soldiers took up Bunny, and departed.

Gough certainly makes us question some of our preconceptions about the Civil War period. The association between puritanism and parliamentarianism comes in for a shaking by the following story:

> Mr. Mackworth made Captain Hill (a prodigal drunken fellow, who before the wars was a pitiful barber in this town) lieutenant of the castle. But the townsmen and garrison soldiers hated him; and therefore as soon as there was a prospect of the return of King Charles II they conspired against him; and one of the townsmen sent for him out of the castle to drink with him at the Loggerheads, an alehouse hard by; and as soon as he was gone out of the castle, the soldiers shut the gate and cast his clothes and boots over the wall, and immediately the town was in an uproar; and Hill for fear of his life fled away that night and I never heard more of him.

A drunken barber made the lieutenant of a parliamentary garrison, and ejected on the advent of the return of the king — it is this type of evidence which leads to the re-writing of history books. But how reliable is Gough as an informant? Where it has been possible to check him against other sources, he has been found to be highly accurate.[14] He had the habit of repeating himself without realising it, and this allows us to check on his internal consistency; most of the repetitions are trivial and have been eliminated from the edited text, but in

order to let the reader compare one duplicated story for himself, I give the following important passage which will be found in alternative form on page 118.

> Robert Hayward the eldest son of Thomas Hayward and Alice his wife, was set apprentice to a refiner of silver in London. (I have heard him say that his father gave only the price of an old cow with him.) His master was a dissenter and was one of that sect which are called millenarians, or fifth monarchy men. After the restoration of King Charles II, the men of this sect were persuaded or rather deluded by their teachers and ringleaders, that now the time was come that Christ's Kingdom was to begin on earth, that they must provide themselves of arms and fight for their Lord and King against Antichrist; that they need not fear, although they were but few, for one of them should chase a 100, and 100 should chase 10,000, and by such persuasions these poor deluded people made an insurrection in the city, which being showed to his majesty and his council, the king commanded that his life guard and the city militia should be sent to suppress them. I heard it reported that in the streets of the city they fought very desperately, and some were killed but many wounded on both sides. At last the city militia got some behind them, and some came upon them through cross streets, so that being encompassed about on all sides they were forced to lay down their arms and cry quarter; the prisons in London were filled with them. Robert Hayward was one of the prisoners. Some of the ringleaders were executed and some of the rest were fined, and those that had nothing were set at liberty.

Although both accounts give more-or-less the same version of the uprising, the above is more detailed on the degree of resistance and the tenacity of the rebellion. In the text account "they were all pardoned except their ringleader who I think was hanged", whereas in above "some of the ringleaders were executed". Gough was probably at his least reliable when he had no direct personal experience of the event described, and

fortunately for us, most of his stories relate to the arena of his own personal life, i.e. the community of Myddle.

I mentioned at an earlier point the prevalence of theft in Myddle — thirteen pages in the text mention the subject — and not surprisingly most of the goods stolen were agricultural produce. I quote the following story at length as it illuminates a number of sociological themes in the one passage. A certain Reece Wenlocke

> was descended of good parentage, who were tenants of a good farm, called Whottall, in Ellesmere Lordship. But the father of this Reece was a bad husband, and a pilfering, thievish person, and this son, Reece, and another son, named John, who lived at Bald Meadow, in this parish, were as bad as their father. They never stole any considerable goods, but were night walkers, and robbed orchards and gardens, and stole hay out of meadows, and corn when it was cut in the fields, and any small things that persons by carelessness had left out of doors. Reece had a cow, which was stolen away, and it is reported that he went to a woman, whom they called the wise woman of Montgomery, to know what had become of his cow; and as he went, he put a stone in his pocket, and told a neighbour of his that was with him that he would know whether she were a wise woman or not, and whether she knew that he had a stone in his pocket. And it is said, that when he came to her, she said, thou hast a stone in thy pocket, but it is not so big as that stone wherewith thou didst knock out such a neighbour's harrow tines. But the greatest diskindness that he did to his neighbours was, by tearing their hedges. And it is reported, that he had made a new oven; and, according to the manner of such things, it was at first to be well burnt, to make it fit for use, and this he intended to do in the night. At that time William Higginson dwelt at Webscot, and he had a servant, named Richard Mercer, a very waggish fellow. This Mercer did imagine that Reece would tear his master's hedges to burn the oven; and as he walked by a hedge, which was near Reece's house, he saw there a great dry stick of wood, and took it home with him, and bored a hole in the end of it with

> an auger, and put a good quantity of powder in it, and a peg after it, and put it again into the hedge. And it happened, that Reece Wenlocke, among other hedgewood, took this stick to burn in his oven; and when he cast it into the fire in the oven, it blew up the top of it, and set fire on the end of the house. Reece went out and made hideous crying, fire! fire! William Higginson, being the next neighbour, heard him, and called Mercer, but he said I know what is the matter; however, they went both down to the Meare House, but Reece had put out the fire that was in the end of the house, and the oven was broken to pieces.

The combination of theft, humour and violence makes compelling reading, although it is easy to forget the ruthlessness involved in blowing up someone's house as a part of a practical joke. The theft which took place seemed fairly indiscriminate, and if we are worried today about the level of burglary and theft, we can take historical comfort in how much more our ancestors were prone to this particular problem. Gough's mention of the wise woman of Montgomery is his only reference to a contemporary belief in magic, although various beliefs which we would now consider superstitious (for example, the linking of pigeons with disease) are referred to. Some social historians have stressed the importance of witchcraft beliefs, but this is for other areas of the country and for an earlier period of the seventeenth century.[15] Its complete absence in Myddle is somewhat surprising nonetheless, particularly when it is remembered that Gough was capable of taking his history back a hundred years or more to before when he was born (the practise of oral history was obviously very strong in the village).

Ruthlessness was not confined to acts of personal violence, but could extend to personal relationships within the family. A certain Samuel Downton had contracted a great deal of debt, mainly through drink, and had come to run an alehouse.

> After some years this Samuel Downton and his wife (having sold some of their household goods) got away from Cockshutt in the night-time and left all their children behind them — four of which were after maintained by the parish of Ellesmere. They went into Staffordshire and there he went a begging like an old decrepit person and she carried a box with pins and laces. But after awhile she got a new spark that travelled the country and went away with him, and then this Samuel came again to Alderton to his son Thomas who maintained him during his life.

The harsh treatment of children seems to have been rare going by the evidence provided by Gough; they were occasionally deserted as in the Downton family, and sometimes (as we have seen) violence was used against them. But there are as many references to indulgent treatment of children, and this perhaps explains in the main why so many children were prepared to maintain and take care of their aged parents (there are eight pages in the text in which this is mentioned). Gough does mention however hostile reactions of children towards their parents; for example, one of the disputes resulting in homicide started when Charles Hesketh used "very scurrilous, abusive, and undutiful language towards his parents."

Fairly frequent mention is made of desertion and separation between marriage partners, such as occurred between Samuel Downton and his wife. Flight was a common response to unresolvable situations (Ireland was frequently mentioned as a place that people ran to in difficulty) such as a marriage breakdown; the other common reason for running away was in order to avoid responsibility for an illegitimate child. Illegitimacy appears frequently in Gough's pages (sixteen pages in the text include references to it), and the following gives a flavour of his treatment of the subject. William and Margaret Challoner had

> three daughters, two of which are as impudent whores as any in this country; one of them has two bastards, and she being run out of the country, they are both maintained by the parish. The other is now (Jan. 20, 1701) great with a bastard, and at Christmas last was sent by order into Wem parish, where her last service and settlement was. She has fathered it on Stephen Formeston, her uncle's son, and he has fled.

According to the local parish register, only about one per cent of all baptisms in the sixteenth and seventeenth centuries were of illegitimate children,[16] but this figure is very unreliable when set against Gough's evidence. Probably many illegitimate children were never baptised, and this should make one very wary of using these statistics uncritically.[17] Gough himself did not mention all examples of illicit sexuality in Myddle; the Anglican ecclesiastical court charged Arthur Davies and Jane Morris in 1699 and 1700 with "living together in open fornication",[18] and although Gough refers to them after they were married, there is no mention of any sexual impropriety.

Not surprisingly, venereal disease appears more than once in Gough — there are three pages of the text which mention it. Disease and illness were very common in Myddle at this time, and although there is no systematic treatment, we do get an invaluable insight into the subject. The symptoms of rickets and scurvy are described, and the presence of these illnesses indicate that inadequacies of diet were present. Both diseases were however extremely rare, and other evidence in Gough's book suggests that most people were adequately fed — meat appearing to be a central part of the staple diet. (The over-consumption of meat may have been a reason for the case of scurvy.) The most serious disease at this time appears to have been "fever", and there was at least one damaging epidemic outbreak in Gough's time (the exact cause of this fever is unknown — it was probably typhus). Plague had appeared in Shrewsbury, and Gough mentions certain individuals catching

and dying from it in London — but by this period it was mainly an urban disease, on the point of disappearance. There are three mentions of smallpox, and although it was very widespread at this time, it was still a relatively benign disease — its virulence only really increased at the beginning of the eighteenth century. There is a frequent mention of childlessness and this may have been because of the prevalence of diseases like smallpox, because even in mild form it is capable of producing infertility. Lameness appears fairly frequently, often due to the accidents which were a common hazard in seventeenth century Myddle. Illness was treated by doctors and apothecaries, although probably only the wealthy used their services to any extent; much more common was the practice of amateur medicine, and women seemed to have played a significant role in this, particularly in surgical operations (this may have been associated with their roles of midwives). Gough does give an example of what we might call magical medicine; one woman tried to cure her illness through the "King's Touch" — this was the practice of people being touched by the king when he was touring the countryside, in the belief that he had charismatic powers of cure — sadly with the lady in question, the cure was unsuccessful.

If Gough is at all a reliable guide, mental illness was extremely rare at this time; there was only one case of what might be called a psychotic illness, and one other case of what we would now call mental defectiveness — although Gough describes the sufferer much more evocatively, in calling him an "innocent". Of course there were people displaying neurotic symptoms, but these seemed to have been less frequent than they are today. Melancholy is mentioned on four pages, but given the number of people mentioned in the book, this does not appear to have been a common complaint. This is consistent with the relatively low suicide rate, and it would there-

fore seem that people living in this seventeenth century community were less afflicted by the various forms of mental illness. This may have been due partly to their ability to express openly their most intense feelings — including those of aggression — in an open social context. Another factor might have been to the close-knit nature of the community; this is most strikingly illustrated by Gough's own knowledge of the people in the village — who today could know so much about so many people in the community in which they live?

We should not exaggerate however the absence of personal problems at this time; there are frequent mentions of unhappy marriages, quarrels and violence. One major problem that many people had to face was poverty and destitution. Gough mentions in passing the practice of paupers being made to wear a paupers' badge — a P sewn onto their clothing — which reminds us of the harshness of seventeenth century life, particularly in the treatment of the poor. Admittedly, Gough tells us that there were virtually no parish poor in his father's time — the payment of the poor-rate was virtually non-existent — but there were clearly people in great destitution, with mentions of begging and children being forced to maintain their aged relatives. Bankruptcy and debt were very common, often as we have seen on account of drink, but also due to the vagaries of trade and commerce. Many merchants and tradesmen are said to have gone bankrupt — Gough tells us that they "broke" — and this was frequently because of a chain reaction of bankruptcies. This subject is most often mentioned in connection with people living in Shrewsbury and other local towns, but in this connection London looms surprisingly large in the lives of the people of this small rural community. But London was the centre of prosperity as well as bankruptcy, and a number of poor people are said by Gough to have made their fortune by emigrating to that place.

The rise and fall in prosperity of tradesmen and merchants is a theme which is mirrored in the surprisingly large amount of social mobility. Nine pages in the text mention cases of upward mobility, and ten downward — with an additional seven pages giving cases of general social mobility — a total of twenty-six pages. This may have been the result of the relatively open nature of the social structure of the community discussed earlier. Education was also much more common in Myddle than might be expected, with frequent mentions of schools and the teaching of both reading and writing. Myddle in this respect was a "civilized" community, and we must set this aspect of social life against the violence and drunkenness discussed earlier in the introduction. The latter emphasis could be misleading if we did not balance it out against descriptions of contrary behaviour given to us by Gough. Many people are described as peaceable, honest, just, charitable, pious, hospitable and hard-working. Most good stories tend to involve the vices rather than the virtues, and Gough himself sometimes admits that he has little to say about a particular person because of their quiet peaceableness (there are ten pages of the text with an entry in the index under the heading of "peaceable"). The conclusions we come to about the nature and quality of life in seventeenth century must ultimately be personal and based on our own values; but as happy endings are best, I will conclude by quoting at length from Gough's account of a man who he considered to have lived a virtuous and happy life. Thomas Ash

> was a proper, comely person; his father gave him good country education, which, with the benefit of a good natural wit, a strong memory, a courteous and mild behaviour, a smooth and affable way of discourse, an honest and religious disposition, made him a complete and hopeful young man, insomuch as Mr. Edward Hanmer, of Marton, was easily induced to give him his

daughter Elizabeth to wife. This was a very suitable match, for she was a lovely, proper gentlewoman, and so like to her husband in disposition, that it should seem there was a sympathy in nature between them, and therefore they lived a loving and comfortable life together. This Thomas Ash was not so much blamed for being too nice in observing the canons, as he called them, of the first counsel of the apostles at Jerusalem, in abstaining from blood and things strangled, as he was commended for avoiding that abominable sin of profane swearing. For this Thomas Ash was much in debt; but how it was contracted I cannot say, unless he was charged with the payment of portions to his sisters, and I doubt he had but little portion with his wife; however he bore an honest mind, and was willing to pay every man, and to that end he set his tenement to Edward Payne of Meriton, for raising of money to pay debts; and to shelter himself from the fatigue of duns, he listed himself soldier in the king's service in the wars, tempore Car. I, and continued a soldier until the king's forces were utterly dispersed, but never attained to any higher post than a corporal of foot. At his return, he brought nothing home but a crazy body and many scars, the symptoms of the dangerous service which he had performed, and besides, he fould little of his debts paid, for the payment of taxes and charges of repairs had taken up most part of the rent; but he being minded that none should lose by him, sold his lease to William Formeston. He had some money to spare when he had satisfied his debts, and with that he took a lease off Mr. Crosse of Yorton, of several pieces of ground near Yorton Heath, and there he built a little warm house, made a neat little garden, planted a pretty orchard, built several outhouses, and made everything very handsome and convenient, and there he and his loving wife spent their old age, though not in a plentiful, yet in a peaceable and contented condition.

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5. S. Freud, "Mourning and Melancholia", *Standard Edition*, 14.
6. Emile Durkheim, *Suicide* (Routledge, 1968), pp. 338-360; A. F. Henry and J. F. Short, *Suicide and Homicide* (1964).
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8. P. E. H. Hair, "A Note on the Incidence of Tudor Suicide", *Local Population Studies*, No. 5 (Autumn 1970).
9. See for example, Mary Thale (Ed), *The Autobiography of Francis Place* (1972).
10. Quoted in David G. Hey, *An English Rural Community: Myddle Under the Tudors and Stuarts* (1974), p. 7. I have relied on Hey's excellent study for much of my background information on Myddle.
11. William Harrison, *The Description of England* (1968), p. 237.
12. Beattie, *op. cit.*, p. 61. Homicide rates in Surrey and Sussex fell from an average of about six per 100,000 in 1663-94 to just over two per 100,000 in 1722-24 and to lower figures thereafter.
13. Following Freud, puritanism can be seen as the turning of aggression inwards against the self, i.e. through the creation of a harsh, self-punishing super-ego.
14. Hey, *op. cit.*
15. See for example, A. D. J. Macfarlane, "Witchcraft in Tudor and Stuart Essex", in Cockburn, *op. cit.*
16. Hey, *op. cit.*, p. 224.
17. The treatment of illegitimacy in Peter Laslett's *Family Life and Illicit Love in Earlier Generations* (1977) is perhaps open to criticism on these grounds. It would be interesting to see how the new historical methodology of total reconstitution would stand up to independent assessment through the kind of information contained in Gough.
18. Hey, *op. cit.*, p. 225.

DR PETER RAZZELL

INTRODUCTION

On Monday, September 24th, 1849 *The Morning Chronicle* published an account of a visit to the cholera districts of Bermondsey — the first of a series of articles on the London poor by Henry Mayhew. The area he concentrated on was Jacob's Island, one of the few districts surviving the great fire of London; the island was surrounded by a tidal ditch which had become one vast open sewer and Mayhew described a part of the area as follows:

> We then journeyed on to London-street, down which the tidal ditch continues its course. In No. 1 of this street the cholera first appeared seventeen years ago, and spread up it with fearful virulence; but this year it appeared at the opposite end, and ran down it with like severity. As we passed along the reeking banks of the sewer the sun shone upon a narrow slip of the water. In the bright light it appeared the colour of a strong green tea, and positively looked as solid as black marble in the shadow — indeed it was more like watery mud than muddy water; and yet we were assured that this was the only water that the wretched inhabitants had to drink. As we gazed in horror at it, we saw drains and sewers emptying their filthy contents into it; we saw a whole tier of doorless privies in the open road, common to men and women, built over it; we heard bucket after bucket of filth splash into it, and the limbs of the vagrant boys bathing in it seemed, by pure force of contrast, white as Parian marble. And yet, as we stood doubting the fearful statement, we saw a little child, from one of the galleries opposite, lower a tin can with a rope to fill a large bucket that stood beside her. In each of the balconies that hung over the stream the same-self tub was to be seen in which the inhabitants put the mucky liquid to stand, so that they may, after it has rested a day or two, skim the fluid from the solid particles of filth, pollution and disease. As the little thing dangled her tin cup as gently as possible into the stream, a bucket of night soil was poured down from the next gallery.[1]

The impact of the article was considerable; as a result of it for example, Charles Kingsley and the Christian Socialists pressed for sanitary reform.[2] Mayhew's great skill lay in his ability to vividly recreate scenes and events encountered — we feel as we read his account that we are there in Bermondsey, seeing what he saw, 130 years ago. Mayhew also achieved the impact that he did through pioneering what we would now call oral history — or in his words, "the first attempt to publish the history of the people, from the lips of the people themselves."[3]

There was nothing new of course in the concern for the conditions under which the poor lived — "The Condition of England" question was long-standing, and had been probed and investigated since the beginning of the century in a series of medical, poor law and other government reports. Perhaps what was new was a sharpening of the concern of the propertied classes for the stability of the social order in which they so clearly had an overwhelming vested interest; *The Morning Chronicle* in its editorial, announcing the commencement of the national survey of labour and the poor, argued

> "the starving or mendicant state of a large portion of the people . . . if suffered to remain unremedied many years longer, will eat, like a dry rot, into the very framework of our society, and haply bring down the whole fabric with a crash."[4]

The Chartist agitation of the previous year had left its mark, and the "dangerous classes" is a phrase which appears frequently in *The Morning Chronicle* — although Mayhew only used it to rebut the assumptions and fears which it concealed. A secondary concern revealed by *The Morning Chronicle* editorial was the injustice of society as it was then constituted — "No man of feeling or reflection can look abroad without being shocked and startled by the sight of enormous wealth and unbounded luxury, placed in direct juxtaposition with the lowest extremes of indigence

and privation."[5] But again none of this was new — the middle class public had long been aware through novels as well as government reports of the existence of the poor — what *was* new was that a man of great sensitivity of language and feeling, was about to embark on one of the greatest surveys of human life ever undertaken, and this "factual" survey was to have an impact on contemporaries that no other writing on the poor had ever had. To understand how Mayhew achieved this impact is one of the aims of this introduction.

Mayhew himself claimed that he had been responsible for suggesting the national survey to *The Morning Chronicle*, but this was disputed by the newspaper in an editorial after Mayhew had broken with them.[6] Whatever the origin of the survey, Mayhew's first letter appeared in the newspaper on October 19th, 1849, and a series of eighty-two letters by him continued until December 12th, 1850. Just over a third of this material was incorporated in Mayhew's later study, *London Labour And The London Poor*, but the bulk of it has never been published (although selections have appeared in the last few years[7]). The survey covered many regions of England and Wales, and was divided between three types of area — the rural, manufacturing and metropolitan. Mayhew was appointed the metropolitan correspondent and he appears to have been helped by his brother "Gus", as well as by Charles Knight and Henry Wood, along with assistants, stenographers and general helpers.[8] It was Mayhew's contribution that soon attracted attention and the great majority of letters to the newspaper concerned his accounts of the London poor, rather than those on the countryside or industrial areas. Not only was there great general interest, but novelists of the day were clearly influenced by what they read — Charles Kingsley incorporated some of Mayhew's work into his novel *Alton Locke* and someone of the stature of Thackeray wrote in the March 1850 issue of *Punch:*

> "A clever and earnest-minded writer gets a commission from *The Morning Chronicle* newspaper, and reports upon the state of our poor in London; he goes amongst labouring people and poor of all kinds — and brings back what? A picture of human life so wonderful, so awful, so piteous and pathetic, so exciting and terrible, that readers of romances own that they never read anything like to it; and that the griefs, struggles, strange adventures here depicted exceed anything that any of us could imagine . . ."[9]

Mayhew achieved this effect on his readers by combining the survey side of his work with illustrations drawn from vivid individual autobiographical histories. It was this latter approach which gave his work such emotional force; people could identify for the first time with the poor, not just as depicted in a novel, but through the words of individuals whose lives were being laid out before the reader. No amount of statistical and official information on the poor could come near to Mayhew's work for emotional impact; he may have arrived at his method partly through his journalistic experience, but ironically, it was probably his adherence to natural science which led him to such a literal rendering of the evidence given to him by the people he interviewed. But also Mayhew understood the poor: there were elements in his character and experience which led him to sympathize and identify with them, as we will now see.

He was born in London in 1812 the son of a self-made solicitor, and was educated at Westminster Public School. The evidence we have suggests his father was both tyrannical and unsympathetic to all his children, particularly to his sons; he also appears to have been violent with his wife. Mayhew wrote a satire on his father, suggesting that he had a particular dislike for the front of respectability that his father presented to the world.[10] Although Mayhew appears to have been a brilliant pupil, his indolence and rebelliousness led him to leave the school at an early age; he refused to be flogged by the headmaster for a minor mis-

demeanour and immediately left the school never to return. Similarly, after a brief period of apprenticeship in his father solicitor's business, he caused his father some embarrassment by forgetting to lodge legal papers, and fled the house not to see his father for several years. Mayhew's brilliance, indolence and humour led him to adopt the life of a literary bohemian, writing for satirical magazines (he claimed to be one of the co-founders of *Punch*), newspapers, as well as his own plays, short stories and novels. Much of this writing had a radical edge which was probably linked with his reaction against the conservative respectability of his father, although his work was also characterized by some of the middle-class assumptions of the day, showing that he had not escaped the influence of his bourgeois background.[11]

One aspect of Mayhew's character which perhaps has not been sufficiently stressed in other commentaries on his work, was his interest in the natural sciences. According to one account, he had unsuccessfully tried to persuade his father to allow him to become an experimental chemist,[12] and when he left home, he spent much of his time on such experiments (he is reputed to have nearly blown up his brother's house on one occasion![13]), and his interest in natural science clearly informed the way he approached *The Morning Chronicle* survey. He wrote to the editor of that paper in February 1850 explaining his approach:

> I made up my mind to deal with human nature as a natural philosopher or a chemist deals with any material object; and, as a man who had devoted some little of his time to physical and metaphysical science, I must say I did most heartily rejoice that it should have been left to me to apply the laws of inductive philosophy for the first time, I believe, in the world to the abstract questions of political economy.[14]

Although this stress on science and political economy would seem a far cry from Mayhew the great originator of working class oral history, with all its moving and vivid writing, the

contradiction is not as great as it might seem. Mayhew always stressed he was presenting a *factual* picture of the London poor as he found them; when in dispute with the editor of *The Morning Chronicle* about the content of some of his articles — the editor had removed some passages antipathetic to free trade — Mayhew insisted that the original report of the speech of a boot-maker be restored on the grounds that he was "a person collecting and registering facts."[15] His notion of natural science was essentially that it was an inductive discipline, with factual information being collected in great detail before valid generalisations could be reached. It was partly on these grounds that he was critical of the political economists of the day; he believed that they constructed their theories without familiarizing themselves with the complexities of the situations they were trying to explain.

An obvious weakness in Mayhew's method was that he did not use a strict process of random sampling in selecting informants — his work was carried out before this had been developed — but he did attempt wherever possible to avoid undue bias. This is illustrated by the dispute that arose over the reliability of his evidence on Ragged Schools; his assistant R. Knight gave the following account of the method of selecting informants in a letter to *The Morning Chronicle:*

> I was directed by your Special Correspondent to obtain for him the addresses of some of the boys and girls who attended the Ragged School in Westminster, so that he might be able to visit them at their homes. Your correspondent desired me to take the names of the first parties that came to hand, so that neither particularly good nor bad cases might be selected, but such as might be presumed to be fair average examples of the practical tendency of the school in question.[16]

Mayhew comes near here to a random sampling method, but elsewhere he was too dependent on special sources of information to be able to achieve this aim. Frequently

he used key informants — doctors, clergymen, trade union leaders — to both provide information on a subject and introduce him to other informants in the area that he was interested in. The disadvantages and potential bias in this method is obvious, but in practice it seems to have been remarkably successful. All of Mayhew's key informants appear to have been intelligent and well-informed men, and were able to provide him with a range and depth of information that would have been unavailable elsewhere (this is perhaps a method that social scientists today might benefit from rediscovering). A check on the reliability and objectivity of the information given was the public nature of the survey — errors were open to correction through the letter column of the newspaper, and that there were only one or two corrections of this kind,[17] bears testimony to the high overall accuracy of Mayhew's work.

The major theme of the survey was of course poverty, and an introduction of this kind can only touch upon some of the more important aspects of the subject as it was treated by Mayhew. One of the things that he revealed to his contemporaries was the complexity of poverty, as well as its inevitability. Anything which could destroy a family's ordinary means of livelihood — illness, old age, death or accident — could throw it into the most extreme and abject poverty. I quote at some length the following account given to Mayhew of what happened to a coalwhipper (a labourer unloading coal) after an accident:

> I was a coalwhipper. I had a wife and two children. Four months ago, coming off my day's work, my foot slipped, and I fell and broke my leg. I was taken to the hospital, and remained there ten weeks. At the time of the accident I had no money at all by me, but was in debt by the amount of ten shillings to my landlord. I had a few clothes of myself and wife. While I was in the hospital I did not receive anything from our benefit society, because I had not been able to keep up my subscription. My wife and children lived, while I was in hospital, by pawning my things, and going from door to

door, to every one she knowed, to give her a bit. The men who worked in the same gang as myself made up 4s. 6d. for me, and that, with two loaves of bread that they had from the relieving-officer, was all they got. While I was in the hospital, the landlord seized for the rent the few things that my wife had not pawned, and turned her and my two little children into the street — one was a boy three years old, and the other a baby just turned ten months. My wife went to her mother, and she kept her and my little ones for three weeks, till she could do so no longer. My mother, poor old woman, was most as bad off as we were. My mother only works on the ground — out in the country at gardening. She makes about 7s. a week in summer, and in the winter she only has only 9d. a day to live upon; but she had at least a shelter for her child, and she willingly shared that with her daughter and daughter's children. She pawned all the clothes she had to keep them from starving — but at last everything was gone from the poor old woman, and then I got my brother to take my family in. My brother worked at garden work, the same as my mother-in-law did. He made about 15s. a week in summer, and about half that in the winter time . . . He had only one room, but he got in a bundle of straw for me, and we lived and slept there for seven weeks. He got credit for more than £1 of bread, and tea, and sugar for us; and now he can't pay, and the man threatens to summon him for it. After I left my brother's, I came to live in the neighbourhood of Wapping, for I thought I might manage to do a day's work at coalwhipping, and I couldn't bear to live on his little earning any longer — he could scarcely keep himself then. At last I got a ship to deliver, but I was too weak to do the work, and in pulling at the ropes, my hand got sore, and festered for want of nourishment . . . After this I was obliged to lay up again, and that's the only job of work that I have been able to do for this last four months . . . I had one pennyworth of bread this morning. We altogether had half-a-quatern loaf among the four of us, but no tea nor coffee. Yesterday we had some bread, and tea, and butter, but wherever my wife got it from I don't know. I was three days, but a short time back, without a taste of food (here he burst out crying). I had nothing but water which passed my lips. I had merely a little at home, and that my wife and children had. I would rather starve myself than let them do so. Indeed, I've done it over and over again. I never

> begged. I'd die in the streets first. I never told nobody of my life. The foreman of my gang was the only one besides God that knew of my misery; and his wife came to me and brought me money and brought me food; and himself too, many a time ("I had a wife and five children of my own to maintain, and it grieved me to my heart," said the man who sat by, "to see them want, and I unable to do more for them.")[18]

Anyone tempted to dismantle the welfare state would do well to ponder this passage at some length; there is no doubt whatsoever from the voluminous evidence produced by Mayhew and the other correspondents of *The Morning Chronicle*, that this man's experience of what happened in sickness and ill-health was entirely typical. It is not only the extreme poverty of the family itself, but the poverty of their neighbours, workmates and relatives which gives the report such importance in revealing the terrible conditions under which the poor of Victorian England lived. The harshness with which the family were treated by the landlord and the relieving officer obviously added considerably to their misery; only the support of neighbours, workmates and above all, relatives, enabled them to survive at all.

Mayhew makes it very clear that these cases were not merely examples of individual distress, but were characteristic of whole classes of people. Poverty of this kind was the result of structural changes in society, a theme which became Mayhew's over-riding concern in his *Morning Chronicle* letters. He analysed the poverty resulting from changes in the organisation of trades, and began to generalise this into an indictment of the whole of capitalist society. Before he embarked on this analysis, he gathered together a vast amount of empirical evidence on the incidence and nature of poverty, and perhaps what was so unusual about this, was his ability to write so well about what other authors had managed to make so mundane and boring; here is his description of the hiring of labourers in the docks:

> As the foreman calls from a book the names, some men jump upon the backs of the others, so as to lift themselves high above the rest, and attract the notice of him who hires them. All are shouting. Some cry aloud his surname, some his christian name; others call out their own names, to remind him that they are there. Now the appeal is made in Irish blarney, now in broken English. Indeed it is a sight to sadden the most callous, to see *thousands* of men struggling for only one day's hire, the scuffle being made the fiercer by the knowledge that hundreds out of the number assembled must be left to idle the day out in want. To look in the faces of that hungry crowd is to see a sight that must be ever remembered.[19]

He went on to detail the poverty of the dock labourers, and illustrated this in brilliant fashion through interviews with individual dockers and their families — families that lived in one squalid, unheated and virtually unfurnished room, who were frequently subject to hunger and illness, without proper clothing — children without shoes and socks — and could only find work if they were prepared to participate in the scramble described above. Many of the people seeking dock work had previously been silk-weavers living and working in the Spitalfields area; the drastic decline in the prosperity in this trade was delineated by Mayhew in one of his first letters.[20]

Although silk-weaving was the most dramatic example of an occupation falling into destitution, most of the trades covered by Mayhew were subject to something of the same process. Real wages fell amongst nearly all occupational groups, and *The Morning Chronicle* survey provides an unrivalled series of economic histories of various trades from the late eighteenth century onwards. Workers in the shoe- and boot-making trade had suffered severely in living standards since the prosperity of the Napoleonic wars, as was revealed by one of Mayhew's informants:

> In 1812 the boot-makers received their highest wages. If an average could have been taken then of the earnings of the

> trade, one with another, I think it would have been about 35s. a man. The great decrease (from 35s. to 13s. 6d. a week) that has taken place is not so much owing to the decrease of wages as to the increase of hands, and the consequent decrease of work coming to each man. I know myself that my late master used to earn £2 a week on average many years back, but of late years I am sure he has not made 15s. a week.[21]

Mayhew unfortunately did not collect systematic information on changes in prices — the evidence he did publish suggests that prices only begun to fall significantly after the mid-1840's. But the qualitative evidence on living standards more than outweighs this deficiency. Here is a description of a boot-maker's earnings and style of life in the early years of the century:

> I got work in Mr. Hoby's . . . not long after the battle of Waterloo, in 1815, and was told by my fellow workmen that I wasn't born soon enough to see good times; but I've lived long enough to see bad ones. Though I wasn't born soon enough, as they said I could earn, and did earn £150 a year, something short of £3 a week; and that for eight years when trade became not so good . . . I could then play my £1 a corner at whist. I *wouldn't* play at that time for less than 5s. I could afford a glass of wine, but was never a drinker; and for all that, I had my £100 in the Four per Cents for a long time (I lent it to a friend afterwards), and from £40 to £50 in the savings bank. Some made more than me, though I *must* work. I can't stand still. One journeyman, to my knowledge, saved £2,000; he once made 34 pairs of boots in three weeks. The bootmen then at Mr. Hoby's were all respectable men; they were like gentlemen — smoking their pipes in their frilled shirts, like gentlemen — all but the drunkards. At the trade meetings, Hoby's best men used to have one corner of the room to themselves, and were called the House of Lords. There was more than one hundred of us when I became one; and before then there were an even greater number. Mr. Hoby has paid five hundred pounds a week in wages. It was easy to save money in those days; one could hardly help it. We shall never see the like again.[22]

Contrast this with the life-style of a boot-closer who

> assured me that he had dealt with his baker for fourteen or fifteen years and had never been able to get out of debt lately . . . As for a coat, he said, "Oh, God bless my soul, sir, I haven't bought one for this six or seven years, and my missus has not been able to purchase a gown for the same time; to do so out of my earnings *now* is impossible. If it wasn't for a cousin of mine that is in place, we shouldn't have a thing to our backs, and working for the best wages too . . . Wages have been going down ever since 1830. Before that time my wife attended to her domestic duties only . . . Since that period my wife has been obliged to work at shoe-binding, and my daughter as well . . . My comforts have certainly not increased in proportion with the price of provisions. In 1811 to 1815 bread was very high — I think about 1s. 10½d. the best loaf — and I can say I was much more comfortable then than at present. I had a meat dinner at that time every day, but now I'm days without seeing the sight of it. If provisions were not as cheap as they are now we should be starving outright . . ."[23]

These were men who worked in the "honourable" part of the trade — working on the premises of their employer for fixed hours, their conditions of work regulated by agreement with their trade union. Although increasingly impoverished by the fall in wages, their situation was much better than that of people working in the "dishonourable" sector — those who either worked for themselves as "chamber masters" in their own homes, or were employed by them. This sector was strongly concentrated in the east end of London, whereas the more respectable part of the trade were concentrated mainly in the west end. This polarisation of the trades — with about ten per cent "honourable" and ninety per cent "dishonourable" — was revealed by Mayhew to be common in the London trades. He summarized the markedly different life-styles of the two groups and illustrated it with reference to the tailoring trade:

> The very dwellings of the people are sufficient to tell you the wide difference between the two classes. In the one you occa-

> sionally find small statues of Shakespeare beneath glass shades; in the other all is dirt and foetor. The working tailor's comfortable first-floor at the West-end is redolent with the perfume of the small bunch of violets that stand in the tumbler over the mantel-piece; the sweater's wretched garret is rank with the stench of filth and herrings. The honourable part of the trade are really intelligent artisans, while the slopworkers are generally almost brutified with their incessant toil, wretched pay, miserable food, and filthy homes.[24]

The sweating system at its worst could be highly dangerous to health and life, as was revealed by someone who had worked for one:

> One sweater I worked with had four children, six men, and they, together with his wife, sister-in-law, and himself, all lived in two rooms, the largest of which was about eight feet by ten. We worked in the smallest room and slept there as well — all six of us. There were two turn-up beds in it, and we slept three in a bed. There was no chimney, and indeed no ventilation whatever. I was near losing my life there . . . Almost all the men were consumptive, and I myself attended the dispensary for disease of the lungs.[25]

What had brought about the terrible mass of misery and poverty that week after week filled *The Morning Chronicle's* pages? The answer of the political economists of the day was that it was largely due to an over-rapid expansion of population, and it was this Malthusian orthodoxy that Mayhew was most concerned to dispute. He did not contest that an over-supply of labour would lead to a fall in wages and living standards, but criticized the Malthusian conclusion on empirical grounds. In his later work *London Labour And The London Poor,* he argued that there had been no excessive increase in population in the first half of the nineteenth century, stating that the demand for labour as measured by various output/production series, had more than kept pace with population increase.[26] He did not seem to realise that this contradicted his own findings about the increasing poverty of the mass

of the people, although he could have saved part of his argument by stressing the re-distribution of income from poor to rich. The re-distribution would have had to have been very dramatic to account for the depth of poverty he found in his survey, and there is no evidence that it ever reached this scale. The major problem with Mayhew's argument is that he used production series for commodities such as cotton and wool, which are known to have expanded very dramatically, the textile industry being central to the industrial revolution then taking place. The standard of living and how it changed in this period has of course become a subject of extensive scholarly debate, but this does not appear to be resolvable with existing statistical data. Mayhew's own detailed qualitative evidence seems much more useful in telling us what was happening at this time, and the conclusion from his survey must be that there was a vast increase in poverty during the first half of the nineteenth century.

How are we to reconcile the above conclusion with some of the statistical series on wages which appear to contradict it? The answer lies I believe in what the bootmaker told Mayhew in the interview quoted previously — that it was not so much a fall in wage rates of existing trades that was responsible, but a significant decrease in the amount of employment available and the growth of sweated work practices outside of the recognized (and presumably the statistically measured) regular trades. Mayhew himself stated that "in the generality of trades the calculation is that one-third of the hands are fully employed, one-third partially, and one-third unemployed throughout the year."[27] This would seem to bring the analysis back to an over-supply of labour and an excessively expanding population, but Mayhew had a series of detailed arguments based on his empirical findings with which to counter this thesis. For him the surplus of labour was the result of the competitiveness of contemporary capitalist society, and he brought

this out in a number of separate but related themes. He recognized that the introduction of new technology had a significant impact on the creation of surplus labour; for example, he described in some detail the effect of steam machinery on the employment of sawyers and how it had both reduced their numbers and income.[28] But the effect of the new technology was very limited in London as most industries were labour-intensive; what Mayhew did trace however was the impact of the industrial revolution of the textile industry in Lancashire, for some of the labour displaced found its way on to the London labour market. One man who had become destitute gave Mayhew the following account of his life:

> "I am thirty-eight" he said, "and have been a cotton-spinner, working at Chorlton-upon-Medlock. I can neither read nor write. When I was a young man, twenty years ago, I could earn £2 10s. clear money every week, after paying two piecers and a scavenger. Each piecer had 7s. 6d. a week — they are girls; the scavenger — a boy to clean the wheels of the cotton spinning machine had 2s. 6d. I was master of them wheels in the factory. This state of things continued until about the year 1837. I lived well and enjoyed myself, being a hearty man, noways a drunkard, working every day from half-past five in the morning till half-past seven at night — long hours that time, master. I didn't care about money as long as I was decent and respectable. I had a turn for sporting at the wakes down there. In 1837 the 'self-actors' (machines with steam power) had come into common use. One girl can mind three pairs — that used to be three men's work — getting 15s. for the work which gave three men £7 10s. Out of one factory 400 hands were flung in one week, men and women together. We had a meeting of the union, but nothing could be done, and we were told to go and mind the three pairs, as the girls did, for 15s. a week. We wouldn't do that. Some went for soldiers, some to sea, some to Stopport (Stockport), to get work in factories where the self-actors wer'nt agait."[28]

The Luddite reaction to new technology becomes completely understandable, its beneficiaries at this time being almost entirely the owners of factories and their like. The

sawyers had destroyed the first mechanical mills in London (these were run by horse-power but on the same principle as the later steam mills), but had eventually succumbed to the new technology.

Mayhew realized however that technology was not the prime moving force in the early capitalist transformation of society, at least in the London area. Much more important was the "extraction of labour-surplus" through changes in the organisation of what Marx called the social relationships of production — in particular the development of petty capitalism in various forms. Mayhew did not of course analyse the course of events in such simple analytical terms; he gave a much more descriptive account of what he called the effects of the "competitive system". He analysed the increase of surplus labour under two headings: the increase in the number of labourers and the increase in the amount of labour extracted from an existing labour force. He saw six ways of increasing the number of labourers: "(1) By the undue increase of apprentices. (2) By drafting into the ranks of labour those who should be otherwise engaged, as women and children. (3) By the importation of labourers from abroad. (4) By the migration of country labourers to towns, and so overcrowding the market in the cities. (5) By the depression of other trades. (6) By the undue increase of the people themselves."[29] Three, four and six are all direct effects of increasing population and belong if you like to the "opposition argument". One and two form a part of Mayhew's main argument (five is rather nebulous), although he does not spell this out. He grouped the means of increasing the amount of labour from a fixed labour force under seven headings: "(1) By extra supervision when the workmen are paid by the day . . . (2) By increasing the workman's interest in his work; as in piece work, where the payment of the operative is made proportional to the quantity of work done by him . . . (3) By large quantities of work given out at one time; as in 'lump-work' and 'contract

work'. (4) By the domestic system of work, or giving out materials to be made up at the homes of the workpeople. (5) By the middleman system of labour. (6) By the prevalence of small master. (7) By a reduced rate of pay, as forcing operatives to labour both longer and quicker, in order to make up the same amount of income."[30] Many of these headings overlap as Mayhew himself was prepared to admit; categories two to six all have a strong element of increasing the capitalist principle into work situations, and in practice the prevalence of the contract system and in particular the growth of small masters (petty capitalists) seem to have been most important, at least in Mayhew's work. Headings one and seven concern the control that employers were able to exert over their work force, without having to go through indirect market forces (the distinction between employer and employee becomes blurred of course in the case of the small master — a more appropriate distinction here would be between the rich capitalist and the poor worker who actually provided the labour, under whatever relationship of production).

That employers were able to extract enormous amounts of extra labour through direct control was brought out by Mayhew in a number of places; perhaps the most striking example was the "strapping system" in the carpentry and joinery trade:

> Concerning this I received the following extraordinary account from a man after his heavy day's labour; and never in all my experience have I seen so bad an instance of over-work. The poor fellow was so fatigued that he could hardly rest in his seat. As he spoke he sighed deeply and heavily, and appeared almost spirit-broken with excessive labour: — "I work at what is called the strapping shop," he said, "and have worked at nothing else for these many years past in London. I call 'strapping', doing as much work as a human being or a horse possibly can in a day, and that without any hanging upon the collar, but with the foreman's eyes constantly fixed upon you, from six o'clock in the morning to six o'clock at night. The shop in which I work is for all the world like a prison — the

> silent system is as strictly carried out there as in a model gaol. If a man was to ask any common question of his neighbour, except it was connected with his trade, he would be discharged there and then. If a journeyman makes the least mistake, he is packed off just the same. A man working in such places is almost always in fear; for the most trifling things he is thrown out of work in an instant . . . I suppose since I knew the trade a man does four times the work that he did formerly . . . What's worse than that, the men are everyone striving one against the other . . . They are all tearing along from the first thing in the morning to the last thing at night, as hard as they can go, and when the time comes to knock off they are ready to drop. I was hours after I got home last night before I could get a wink of sleep; the soles of my feet were on fire, and my arms ached to that degree that I could hardly lift my hand to my head."[31]

The result of this terrible exploitation of labour was that many joiners were "quite old men and gray with spectacles on, by the time they are forty."[32]

It is easy now to understand current trade union practices which regulate and control the amount of work to be done independently of the "logic of production." Trade unions were of course active during the whole of the nineteenth century and we must ask why they were unable to prevent the extreme conditions described above. This is perhaps the crucial question that Mayhew never answered in his discussion of *political-economy*, yet the answer to such a question is to be found in his own survey. Unions had been very active in the protection of living standards and working conditions, even when they had not achieved legal recognition. One boot-maker described the strike of 1812 which resulted in victory for the union:

> The masters, at that time, after holding out for thirteen weeks, gave way, yielding to all the demands of the men. "The *scabs* had no chance in those days," said my informant, "the wages men had it all their own way; they could do anything, and there were no slop shops then. Some scabs went to Mr. Hoby 'occasioning' (that is asking whether he 'had occasion for another hand'), but he said to them. 'I can do nothing; go to

> my masters (the journeymen) in the Parr's Head, Swallow-street' (the sign of the public-house used by the men that managed the strike)."[33]

The key to the success of unions at this time was provided by another of Mayhew's informants:

> I believe the reduction of wages in our trade is due chiefly to the supra-abundance of workmen; that is the real cause of our prices having gone down, because when men are scarce, or work is plentiful, they *will* have good wages. From the year 1798 our wages began to increase partly because the number of hands was decreased by war, and partly because the foreign orders were much greater then than now.[34]

After the Napoleonic wars labour flooded back onto the market, and with population doubling in the first half of the nineteenth century, the supply of labour greatly began to exceed its demand. This of course is a highly complex question, much debated by economists, sociologists and historians, the critical element in the debate being the balance between supply and demand for labour, and its relationship with the distribution of real resources within an early capitalist economy. Another boot-maker put this very simply when he told Mayhew:

> The cause of the trade being so overstocked with hands is, I believe, due in great measure to the increase of population. Every pair of feet there is born, certainly wants a pair of shoes; but unfortunately, as society is at present constituted, they cannot get them. The poor, you see, sir increase at a greater rate than the rich.[35]

Several of Mayhew's artisan informants showed a remarkably good grasp of basic economics, and one or two even anticipated Marx and Keynes in their understanding of the effects of under-consumption on the capitalist economy. One man believed in particular that the new technology would have disastrous effects on the economy:

> Suppose, I say, that *all* human labour is done away by it, and the working men are turned into paupers and criminals, then what I want to know is who are to be the customers of the

> capitalists? The capitalists themselves, we should remember, spend little or none (comparatively speaking) of the money *they* get; for, of course, it is the object of every capitalist to save all he can, and so increase the bulk of money out of which he makes his profits. The working men, however, spend *all* they receive — it's true a small amount is put into the savings bank, but that's a mere drop in the ocean; and so the working classes constitute the great proportion of the customers of the country. The lower their wages are reduced of course the less they have to spend, and when they are entirely superseded by machinery, of course they'll have nothing at all to spend, and then, I ask again, who are to be the capitalists' customers?[36]

These dire predictions did not come to full realization in the hundred years or so after they were made, and this was partly because the industrial revolution had brought about an improvement of average living standards after the 1840's, mainly through a fall in prices. A number of informants told Mayhew how the fall in prices of bread, meat, fruit and vegetables, clothing and other goods, had improved their lot from the mid-1840's onwards, and this was due to a number of factors — new technology, railways, more efficient farming — and undoubtedly this development was the great turning point in the history of capitalism. There were of course many other factors that prevented the pauperization of the working classes predicted by Marx — perhaps one of the most important being the development of specialization and the growth of the division of labour, which enabled the labour force through their unions to exploit the dependency of employers on small numbers of key workers. At the time that Mayhew wrote however, there was little evidence of this development, and the unions were weak and the mass of the population in a pauperized state.

What Mayhew failed to realize was the importance of the rate of expansion of the population for the conditions under which the struggle between capital and labour was conducted. (I assume here that population was expanding

for other than economic reasons, and was primarily a function of medical and other non-economic factors.[37]) Throughout his survey there is constant mention of a massive surplus of labour demanding work which was not there to be had;[38] this enabled employers to ruthlessly crush strikes and union activity, either by employing blackleg labour, or by sending work into non-unionized sectors and areas of the country. What Mayhew did realize was that this surplus of labour enabled employers to extract even further surpluses through the modes of exploitation discussed above — formulated by Mayhew in the phrase, "Over-work makes under-pay, and under-pay makes over-work."[39] A surplus of population did not operate in a vacuum, it was employed within a certain social relationship of production, and this could be crucial for the development of the economy. In the case of London during the middle of the nineteenth century, it was the growth of petty-capitalism that was crucial. This took many guises — sub-contracting, chamber-masters, sweaters, etc. — but the critical development was the exploitation of labour through a system of production which gave workers a personal but minimal interest in profitability.

A cabinet-maker gave the following explanation of why so many men became small capitalists working on their own account:

> One of the inducements . . . for men to take for making up for themselves is to get a living when thrown out of work until they can hear of something better . . . Another of the reasons for the men turning small masters is the little capital that it requires for them to start themselves . . . Many works for themselves, because nobody else won't employ them, their work is so bad. Many weavers has took to our business of late . . . Another reason for men turning little masters is because employment's more certain like that way; a man can't be turned off easily, you see, when he works for himself. Again, some men prefer being small masters because they are more independent like; when they're working for themselves, they can begin working when they please, and knock off when-

> ever they like. But the principal reason is because there ain't enough work at the regular shops to employ them all.[40]

These small masters were drawn into a system of ruthless competition, and the money paid to them by the warehouses — the "slaughterers" — became barely sufficient for subsistence. Many of the chamber-masters were sweaters, employing their wives and children and any other source of cheap labour, but none of them were real beneficiaries from the long and grinding hours of work — it was the owners of the warehouses and their customers who really gained from this system of exploitation. The major reason why so many small masters were prepared to tolerate these conditions was because there was no alternative — a surplus of labour through a rapidly-expanding population had thrown them out of regular work and into pauperized independence, which in turn helped destroy the power of the trade unions in the "honourable" sector of the trade.

Although Mayhew failed to link population growth with the changes in the structure of the social relationships of production which he so effectively described, he provided in his survey nearly all that we would want to know to understand the development of contemporary capitalism. However, his survey went well beyond the confines of this major theme, and to the sociologist, his work provides a range of fascinating detail on other sociological subjects. One theme that constantly recurs is the growth of a culture of respectability during the nineteenth century, a subject which obviously fascinated Mayhew. There are frequent mentions in the survey of the decline in drunkenness and brutality which characterized many English workmen of an earlier epoch; here is Mayhew's interview with a cabinet-maker on the subject of respectability:

> "Within my recollection," said an intelligent cabinet-maker, "there was much drinking, among the cabinet-makers. This was fifteen years back. Now I am satisfied that at least seven-eighths of all who are in society are sober and temperate men.

> Indeed, good masters won't have tipplers now-a-days." . . . The great majority of the cabinet-makers are married men, and were described to me by the best informed parties as generally domestic men, living, whenever it was possible, near their workshops, and going home to every meal. They are not much of play-goers, a Christmas pantomime or any holiday spectacle being exceptions, especially where there is a family. "I don't know a card-player," said a man who had every means of knowing, "amongst us, I think you'll find more cabinet-makers than any other trade members of mechanics' institutes and literary institutions and attenders of lectures." Some journeymen cabinet-makers have saved money, and I found them all speak highly of the advantages they, as well as their masters, derive from their trade society.[41]

These respectable artisans were of course only a minority of the total of working people; we saw earlier how the members of the "honourable" west end trade lived very different lives to those of the east end. The respectable artisans were family men, living quiet private lives, markedly in contrast with the life of the "rough" working class, which was violent, noisy and gregarious. Mayhew had a deeply ambivalent attitude towards respectability; on the one hand he admired the "rational" sobrietry, cleanliness and cultured life-style of his intelligent artisans, yet on the other was greatly attracted to the spontaneity and colour of his street folk, vagabonds, delinquents, labourers and other unrespectable inhabitants of London. The intelligence of the respectable artisan enabled him to take an active interest in union and political matters, whereas the unskilled workmen tended to passively acquiese in the miseries of his lot:

> The transition from the artisan to the labourer is curious in many respects. In passing from the skilled operative of the West End to the unskilled workman of the Eastern quarter of London, the moral and intellectual change is so great that it seems as if we were in a new land and among another race. The artisans are sufficiently educated and thoughtful to have a sense of their importance in the state . . . The unskilled labourers are a different class of people. As yet they are as unpolitical as footmen. Instead of entertaining violently demo-

> cratic opinions, they appear to have no political opinions whatever — or, if they do possess any, they rather lean towards the maintenance "of things as they are," than towards the ascendancy of the working people.[42]

Not only were the unskilled unpolitical, but they tended to be more addicted to violence, drunkenness and dishonesty than the rest of the population, Mayhew finding from official statistical returns of crime that the labourers of London were "nine times as dishonest, five times as drunken, and nine times as savage, as the rest of the community."[43]

What Mayhew most disliked about the unrespectable however was the dirt and squalor in which they lived; in discussing the importance of fish in the diet of the poor — the railway had ushered in an era of very cheap fish in London — he wrote:

> The rooms of the very neediest of our needy metropolitan population, always smell of fish; most frequently of herrings. So much so, indeed, that to those, like myself, have been in the habit of visiting their dwellings, the smell of herrings, even in comfortable houses, savours from association, so strongly of squalor and wretchedness as to be often most oppressive.[44]

This echoes the passage quoted earlier, which contrasted the west end tailor's comfortable apartment with flowers and pictures, and "the sweater's wretched garret . . . rank with the stench of filth and herrings." Mayhew believed that the poor of the east end were "brutified with their incessant toil, wretched pay, miserable food, and filthy homes", and in a number of places in his survey he uses strong moral language to condemn what he considered to be the vices of the unrespectable poor. Listen to the following account of the lives of pickpockets and note the mixture of moral disapproval and insightful sociological and psychological analysis:

> It is a singular fact that as a body the pickpockets are generally very sparing of drink. My informant never knew

> any one of these young pickpockets or "gonoffs" to be drunk, or to seem in any way anxious for drink. They are mostly libidinous, indeed universally so, and spend whatever money they can spare upon the low prostitution round about the neighbourhood . . . Nor can their vicious propensities be ascribed to ignorance, for we have seen that out of 55 individuals 40 could read and write, while four could read . . . Neither can the depravity of their early associations be named as the cause of their delinquencies for we have seen that, as a class, their fathers are men well to do in the world. Indeed their errors seem to have rather a physical than either an intellectual or moral cause. They seem to be naturally of an erratic and self-willed temperament, objecting to the restraints of home, and incapable of continuous application to any one occupation whatsoever. They are essentially the idle and the vagabond; and they seem generally to attribute the commencement of their career to harsh government at home.[45]

Much of this account could be applied to Mayhew himself — his own reaction against parental authority, his "erratic and self-willed temperament", and his restlessness. Although current sociological fashion is against the kind of physiological explanation of delinquency given by Mayhew, there is probably as much evidence in its favour as with rival more widely accepted theories.

The delinquents were rebels, but rebels with energy, intelligence, humour and a love of life. It is these qualities which inform some of Mayhew's best-known work, the writing on street entertainers, costermongers, tricksters and the host of other colourful characters which fill his pages. Listen to the marvellous account of one of the many tricks played on a gullible public:

> I've done *the shivering dodge* too — gone out in the cold weather half naked. One man has practised it so much that he can't get off shivering now. Shaking Jemmy went on with his shivering so long that he couldn't help it at last. He shivered like a jelly — like a calf's foot with the ague — on the hottest day in summer.[46]

And some of Mayhew's characters are so close in language to Dickens, that the reader finds himself unconsciously carried

from one to the other. One of the Punch and Judy men told Mayhew:

> One of my pardners was buried by the workhouse; and even old Pike, the most noted showman as ever was, died in the workhouse. Pike and Porsini — Porsini was the first original street Punch, and Pike was his apprentice — their names is handed down to prosperity among the noblemen and footmen of the land. They both died in the workhouse, and, in course, I shall do the same. Something else *might* turn up, to be sure. We can't say what this luck of the world is. I'm obliged to strive wery hard — wery hard indeed, sir — now, to get a living, and then not get it after all at times — compelled to go short often.[47]

The comic quality of the language conceals of course the real suffering of the street performers — Mayhew met a street clown on the verge of starvation, minutes afterwards transformed into an apparently happy and laughing performer[48] — but their human quality shines through their sufferings, and there is almost something moving in the quaintness of their language.

Mayhew was acutely aware of how sociological factors influenced the adoption of respectability or its opposite; he gave a great deal of space for example to the effects of the system of paying wages in public-houses to men working in the coal-unloading trade. For many years it had led to widespread drunkenness and brutality — many men beating their wives because of disputes over the spending of money on drink — and Mayhew summarized the effects of the system in the following passage:

> The children of the coalwhippers were almost reared in the tap-room, and a person who had great experience in the trade tells me he knew as many as 500 youths who were transported, and as many more who met with an untimely death. At one house there were forty young robust men employed about seventeen years ago, and of these are only two living at present. My informant tells me that he has frequently seen as many as 100 men at one time fighting pell-mell at King James's stairs, and the publican standing by to see fair play.[49]

Similarly amongst dockers the irregularity of work and income led to "irregularity of habits" — drunkenness, violence and the squandering of money.[50] In the last resort, Mayhew's sympathy for the poor was so great that it overrode his own middle class prejudices. In a number of places he observed that morality was very different when viewed from the perspective of middle class comfort as against the realities of life amongst the poor:

> It is easy enough to be moral after a good dinner beside a snug sea-coal fire, and with our hearts well warmed with fine old port. It is easy enough for those that can enjoy these things daily to pay their poor-rates, rent their pew, and "love their neighbours as themselves"; but place the self-same highly respectable people on a raft without sup or bite on the high sea, *and they would toss up who should eat their fellows* . . . Morality on £5000 a year in Belgrave-square, is a very different thing to morality on slop-wages in Bethnal-green.[51]

In his speech to the tailors at a special public meeting on the 28th October, 1850, explaining his reasons for withdrawing from *The Morning Chronicle,* he passionately denounced the inequities of contemporary capitalist society, and perhaps came nearest to a socialist ethic and philosophy. He subsequently went on to write *London Labour and the London Poor,* some of which included part of his *Morning Chronicle* material. After this work, he fell into oblivion and obscurity. The poor seemed to bring out the very best of Mayhew; without them, his work sunk back into the rather pedestrian satirical plays and novels written for a middle class reading public (*The Morning Chronicle* survey was read by a wide range of social classes[52]).

The very best of Mayhew was the material he collected on the lives of the poor, "from the lips of the people themselves". The range and depth of these autobiographies is so brilliant, that no amount of commentary can even come near to their quality and importance. Mayhew opened up a new history of the English people in this part of his work, as his informants had come from all parts of the

country and spanned the complete age range. The reader has to read the survey itself to appreciate this part of his work. Dances and music at the harvest celebrations, vagabond life in the countryside and its pleasures and hardships, the problems of a country linen-draper, the harshness of convict life in Australia — the floggings and killings — the brutal conditions on board ship for emigrants (but not convicts — these were protected by their military escort), the meekness and deference of some of the poor, suffering the worst of all poverties, the colour prejudice experienced by an Indian street entertainer — this and a host of other subjects are covered in what we would now consider the beginnings of oral history. Mayhew died in July 1887, forgotten and unknown; he is now recognized as one of the great pioneers of sociological study, but above all, he was a man of deep sympathy and compassion for the suffering of the poor.

Peter Razzell

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Mr. WILLIAM

SHAKESPEARES

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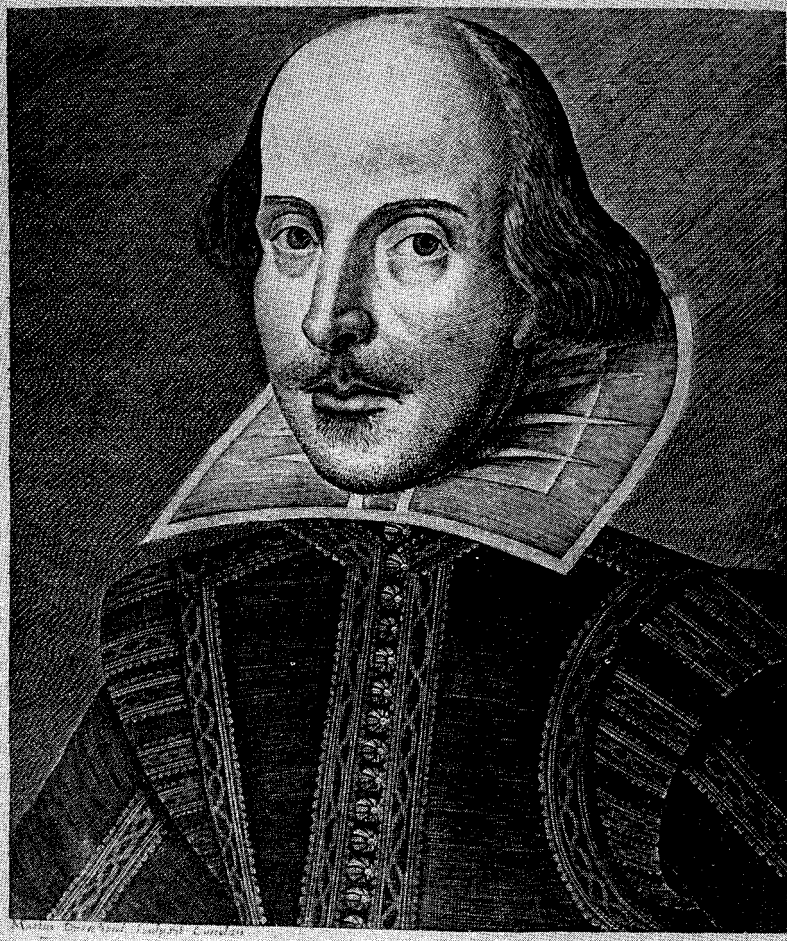

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William Shakespeare: The Anatomy Of An Enigma

William Shakespeare: The Anatomy Of An Enigma

by Peter Razzell

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To Josephine and Luke, my daughter and son
– and to the memory of my fellow sociologist,
Stephen Schenck

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CHAPTER 1:
INTRODUCTION

In 1657, Thomas Plume, Archdeacon of Rochester, wrote about Shakespeare: "He was a glover's son – Sir John Mennis saw once his old Father in his shop – a merry Cheekd old man – that said – Will was a good Honest Fellow, but he durst have crackt a jeast with him at any time."[1] This description of Shakespeare's relationship with his father, is virtually the only direct personal account that has come down to us, and tantalisingly, illuminates a small fragment of Shakespeare's enigmatic biography. The major aim of this book is to unravel this enigma: to reveal the private face behind the public image, and to discover the person obscured by literary mythology. This can be viewed as "a quest for Shakespeare" – unravelling a series of strands which bring us nearer to an understanding of the man and the major events which shaped his life and writing.

The relationship which had the most influence on him, and had the greatest impact on his writing, was that with his father, John Shakespeare. The first half of the book will be about John Shakespeare – including his relationship with his son – and the central thesis of this part of the book can be stated as follows: the character of John Falstaff was based directly on Shakespeare's father, helping to explain not only key events in John Shakespeare's life, but also critical experiences in Shakespeare's own biography. Not only does this thesis help illuminate the Falstaff plays – *The Merry Wives Of Windsor*, *Henry IV, Parts 1 & 2* and *Henry V*, but also a number of the other works, including *Hamlet*. The second half of the book will focus on Shakespeare's own life independently of his father's – but even here, I will argue, John Shakespeare cast a long shadow over his son's life, including a history of alcoholism.

Although all the documentary evidence for a biography of Shakespeare and his father will be scrutinized in careful detail, and this will be supplemented wherever appropriate by evidence from the plays and poems, one additional major source of evidence will be used: that derived from sociological research. This has been carried out in the belief – along with C.Wright Mills – that the "sociological imagination" has a crucial role in explaining personal biography. Elements of economic and social history have been used by previous biographers of Shakespeare, but this has not been done in a systematic fashion. Much new work has been carried out in the last few years, using a more sociological approach to history, and this can illuminate biography, sometimes in quite a vivid way. For example, Shakespeare married Anne Hathaway when he was eighteen and she was twenty-six. Previous biographers have thought of her as an "older woman" – yet recent research has shown that the average age of first marriage of women in rural areas surrounding Stratford was about twenty-five, whereas the average age for men in Stratford was twenty-seven. From this we can conclude that it was not so much that Anne Hathaway was an "older woman", but rather that Shakespeare was a "younger man", compared to his contemporaries marrying during the same period.

This is a relatively minor example of the use of a sociological perspective, and a further example will help illustrate a more major theme. Shakespeare has always been thought of as coming from a narrow provincial background, which has been one of the difficulties in accepting his authorship of the plays. His father is known to have been a glover and probably a butcher at one stage of his career. The idea that the man who wrote some of the greatest plays ever written, should have been the son of a butcher – and even apprenticed to that butcher – just seemed too unbelievable to some Shakespeare admirers. I will be arguing later that this represents a misunderstanding of Shakespeare's Stratford experience; but more importantly, it was not realised until recently, that John Shakespeare was not merely a Stratford artisan, but in fact was a trader operating on a large scale, buying and speculating in a number of commodities (including the lending of money), and operating over a wide geographical area, including London.

This type of trading activity – designated by Everitt as "individual free trading" – gave rise to a particular way of life, with distinct and separate cultural values. In particular, these traders were highly cosmopolitan and lived not only in a provincial world, but operated in a metropolitan

cultural setting. This helps to explain how Shakespeare came to acquire the cultural knowledge which enabled him to write plays of such universal appeal. But this conclusion can only be reached by examining a great deal of economic and social historical evidence, as will be the case with a number of the other themes in the book. At times, it will be necessary to switch from the realm of the personal and the biographical, to a more abstract sociological level, but in every case, the discussion of detailed economic and social historical evidence will lead to a greater understanding of Shakespeare's life and work.

Although a sociological perspective is central, much of the book is devoted to Shakespeare's more personal development. This has been undertaken through a careful examination of the known documentary material, linked to a textual analysis of a number of the plays. The linkage between biography and textual analysis is necessarily speculative and clearly must be approached with great caution, if only to avoid the imaginative but fanciful and untestable speculations that have marred much Shakespeare scholarship. Such a linkage can only be justified if it illuminates a major and central aspect of Shakespeare's life and work, while at the same time following the documentary and textual evidence in rigorous detail.

Far too many works on Shakespeare have been marred by excessive idealisation of their subject, illustrated by the recent tendency to use the Chandos portrait, rather than the well-attested Folio illustration or the bust of Shakespeare placed in Stratford Church by his wife and daughters. One critic complained that the latter made Shakespeare look like a "self-satisfied pork butcher"; the fact that his father was probably a butcher (among other occupations), and that Shakespeare was apprenticed to his father, seemed to have escaped this critic. For such scholars, the Shakespeare that they venerate, must appear as a figure matching his literary stature – perhaps understandable in an age when most of our traditional "gods" have been found wanting. And it is perhaps for this same reason, that the move to deny Shakespeare's authorship of the plays (in spite of overwhelming evidence to the contrary) has flourished. Although some of this is a form of class snobbery – how could a butcher's son and apprentice write such great works of art? – I will be arguing (as we have seen above), that Shakespeare's background was much more cosmopolitan that previously realised.

A good biography must include "warts and all", but excessive denigration is just as undesirable as excessive idealisation: any biography

of Shakespeare must scrupulously follow the known documentary, legal and oral-historical evidence. Where the plays are used as a source of evidence – as they are in this book – it is important that material selected is used systematically, and not just in isolated fashion to buttress a particular case. It is for this reason that I have quoted extensively, and often *verbatim* from the plays and poems, allowing the material as much as possible, to speak for itself.

It could be argued that it is an error to assume that the plays can be used as a source of biographical material, when in fact they were written for a public and commercial audience. It could be further argued that the plays were a product of historical, cultural, political, literary, psychological and philosophical forces of such complexity, that they do not lend themselves to biographical analysis. It is self-evidently true that Shakespeare's plays are highly complex in their origin, with innumerable factors shaping their content and nature. But this should not deter us from focussing on particular aspects of the work; no analysis or criticism would be possible without specialist focus, and if at times this involves discussing material out of context, this can be justified if it adds to our understanding of the author and his work. Of course characters in the plays are not real people and they were put there by Shakespeare mainly for dramatic purposes, but I hope to show that particular plays were of special autobiographical significance, and that certain characters – in particular Falstaff – were of central importance in Shakespeare's own life. This does not mean that there can be any literal translation of character into biographical reality, but it does mean that certain characters can reveal important truths about a writer's life, and if taken in this spirit, can illuminate the author both as a man and a writer.

From Ben Jonson onwards, critics of Shakespeare's work have noted the imaginative, free-flowing quality of the plays, with frequent errors of historical fact and logical inconsistencies in plot and structure. Many of the plays have an almost dream-like quality, and can be seen (to use Freud's phrase) as "over-determined", with multiple determinants of content, including a biographical dimension. In some instances, this has been widely ackowledged; for example the "little eyases" passage in the Folio edition of *Hamlet*, which is universally recognized as a reference to the influence of the children's theatre. It is a good example of how Shakespeare used material of personal significance, and introduced it into the texture of the play, as if it were an intrinsic part of the drama. This only becomes obvious where the material is of a relatively public nature,

and it becomes more difficult to recognize passages of private personal significance, particularly where the material is "unconscious", although we tread on notoriously dangerous ground with this potentially lethal concept. One of the characteristics of Shakespeare's work is that he will often take a theme – for example the issue of the morality of pre- marital conception in *Measure For Measure* – and work it, and re- work it, through various characters and sub-plots: and very often these thematic repetitions are of autobiographical significance.

Unfortunately, Freud, Ernest Jones and other psychoanalysts writing on Shakespeare, applied the psychoanalytical method purely speculatively, and in such a manner as to make any empirical evaluation difficult. Although the psychological analysis of a particular theme in a dramatic work is sometimes productive, it is necessary to assess the effect of other factors which might account for the phenomenon in question. For example, the character of Falstaff is largely an invention of Shakespeare's, and not a reflection in detail of a known historical or literary character, but this conclusion can only be reached through an examination of the historical and literary evidence. This cautionary process is well-understood, being so near to self-evident common-sense, but it is not so well-understood from the other side, i.e. the importance of checking purely literary and historical conclusions against psychological considerations. An important example perhaps of this is the question of the date on which *Hamlet* was written. There has been much scholarship and analysis of historical and literary source material, but as far as I know, no detailed discussion of an important psychological fact, the date of Shakespeare's father's death, and how this might fit into the known chronology of the writing of the play.

An appeal to examine all forms of empirical evidence in testing ideas will command universal assent, but some of the speculation in this book will predictably provoke a critical response. The justification for speculation exists where it is possible to test at least part of the ideas through further historical research. Much speculation takes place tacitly, with biographers decrying the validity of using the plays as a source of biographical material, and then proceeding to do just that, albeit in a piecemeal rather than a systematic fashion. Random historical and empirical research is unlikely to throw up much new material on Shakespeare's life, whereas the systematic search for material directed by particular hypotheses may well lead to important discoveries.

An example of this is the oral tradition of Shakespeare poaching deer from Sir Thomas Lucy; this tradition has been viewed with scepticism by some Shakespeare biographers, largely on the ground that there is no evidence that Sir Thomas Lucy owned a deer park at the time. But an examination of the plays makes it clear how important this incident was to Shakespeare (references to killing deer are to be found in eighteen of the plays). Because of this, a detailed search was made for further evidence, resulting in the discovery of much new important material. I will argue that being caught and punished for this, had a profound effect on his subsequent development, particularly in leading to his exile from Stratford and creating the reaction against his youthful wildness. Much of this new evidence is on deer parks owned by Sir Thomas Lucy, and although highly detailed and at times quite technical, I hope this will make a substantive contribution to Shakespeare biography.

This example of the poaching tradition also illustrates one major weakness in existing Shakespeare scholarship: many biographers have been primarily interested in the literary aspect of Shakespeare's life, and as a result have tended to take a "metropolitan" view, and therefore been somewhat disdainful of the oral tradition, which has invariably been locally based. (It is perhaps for this reason that there has been no definitive scholarly study of the history of Stratford- on-Avon – all the more remarkable when we remember the vast amount of material which has been collected on the town, lending itself eminently to new historical techniques and methods of research.) Malone was a major example of this, listing the various errors he believed that Rowe had made in his biography based mainly on oral sources. In fact, Rowe has stood the test of subsequent scholarship remarkably well, in particular with his knowledge of John Shakespeare's occupation as a wool dealer, his information on Shakespeare's wife's maiden name, and I will be arguing in this book, his description of the young Shakespeare's poaching activities. Many scholars have on general grounds decried the value of the oral tradition, and then have proceeded to smuggle it into their argument to buttress a particular thesis. Given the paucity of information on Shakepeare's life, a general principle for the use of the oral evidence – at least that which derived from living memory – should be that it is to be treated as valid, until proved otherwise.

That the oral tradition could span long periods is shown by Richard Gough's vivid recollections on the lives of the people of the village of Myddle, Shropshire, written at the end of the seventeenth century, and

published in *The History Of Myddle*.[2] Gough in his accounts of individual families, occasionally spanned a period of nearly two hundred years, showing that at least for a village in Shropshire, the oral tradition was very strong indeed. And Gough's delightful language gives us historical insight into the cultural world that helped shape Shakespeare. Those who find it difficult to believe that Shakespeare could have written the plays, do not understand the richness of this oral tradition, which can be documented from at least Gough through to Henry Mayhew and beyond. In fact, I would argue, it is difficult to imagine the plays being written by anyone not educated in this tradition, and this is particularly true with respect to the great popular comic characters, such as Falstaff.

Bold hypotheses following known evidence are not in themselves sufficient: for an idea to be worthwhile it must be testable through future documentary research. It is the nature of this book, that it lends itself to detailed factual scrutiny, particularly with respect to the character and nature of John Shakespeare's associates in later life. For example, John Shakespeare's two friends and associates John Audley and Thomas Cooley, acting for surety for John Shakespeare and each other in the Queen's Bench Court in 1580 – further research on these two figures will shed considerable light on John Shakespeare's character and behaviour. Hopefully, there are a number of ideas in this book which will lend themselves to critical scrutiny, so that the book's conclusions will be open to future evaluation. Whether or not subsequent research validates all elements of this book, it is hoped it will make a stimulating and provocative contribution to Shakespeare scholarship. In the last resort, the interest of the book will derive from all our fascination with the man who produced some of the greatest written works of art in the English language. If it adds to our understanding of the man and his work, it will be worthwhile.

CHAPTER 2:
THE RISE OF JOHN SHAKESPEARE

Other than the sole brief contemporary description of John Shakespeare, the evidence for his biography is exclusively documentary. Halliday has summarized this evidence in his *Shakespeare Companion*, as follows:

> "**Shakespeare, John** (d.1601), son of Richard Shakespeare, and the poet's father must have left Snitterfield sometime before 1552, when he is first mentioned in the Stratford records, he, Humphrey Reynolds and Adrian Quiney each being fined 12d. for making a dunghill in Henley Street, where presumably he was living. In a suit of 1556 he is first called a 'glover', a trade he followed until at least 1586, when he again appears as a glover; he did not sign his name, but made his mark, sometimes in the form of a pair of glovers' dividers. He also traded in barley, timber and wool, and possibly other commodities. In other documents he is styled 'yeoman', that is, a man of substance under the degree of gentleman. The twenty years of 1556-1576 are years of prosperity:
> 1556. Buys two houses; the 'Woolshop' in Henley Street and another in Greenhill Street.
> 1557. Marries Mary Arden, daughter of his father's landlord at Snitterfield.
> 1558. Birth of his first child, Joan. (Six other children were born 1562-74, and Edmund in 1580.)
> 1557-62. Successively borough constable, affeeror (assessor of fines), and chamberlain.
> 1561. Administers his father's estate.
> 1564. Birth of William Shakespeare.
> 1565. Alderman; 1568 Bailiff (mayor); 1571 Chief Alderman and J.P.
> 1575. Buys two more houses; sites unknown, but probably the Birthplace, and an adjoining house to the west, destroyed in the fire of 1594.
>
> The twenty years of 1576-96 appear to be years of adversity.
> 1577. He ceases to attend council meetings.
> 1578-9. Mortgages his wife's Wilmcote property, lets Asbies, and sells her share in the Snitterfield estate.
> 1580. Fined £40 for failing to appear before the court of Queen's Bench to

give security that he would keep the peace.
1586. Replaced by another Alderman because 'Mr Shaxpere dothe not come to the halles when they be warned, nor hathe not done of long tyme'.
1587. Sued for part of the debt of his brother Henry.
1592. Included in a list of recusants 'for not cominge monthlie to the churche . . . It is sayd . . . for feare of processe for debtte'.
His fortunes are restored 1596-1601 probably by the poet:
1596. The Grant of Arms.
1597. His son William buys New Place. Attempts to recover the mortgaged estate from John Lambert.
1599. Applies for leave to impale the arms of Arden.
1601. Again appears as a member of the borough council. On 8 September, 'Mr Johannes Shakepeare' was buried in Stratford churchyard."[1]

Very little has been added to our knowledge of John Shakespeare since Halliday wrote the above, except for a recent and very important discovery about his economic activities, made by D.L.Thomas and N.E.Evans and published in 1984. This will be discussed in some detail later, but the scale of John Shakespeare's transactions – he made loans of £180 in 1568, and bought wool worth £210 in 1571 – sharpens the enigma which has fascinated all scholars familiar with Shakespeare's father's biography: the sudden transition in status from prosperous, successful, and respectable Stratford burgher to impoverished corporation and church absentee. As Thomas and Evans have written in the light of their findings about his prosperity in the early 1570's: "All the more surprising then, is his fall from public position later in the 1570's. Having attained the highest elective office that Stratford had to offer, John Shakespeare withdrew from borough life after 1576; he ceased attending meetings of the council and was ultimately removed from his position as alderman. At the same time, he got into debt and sold land. His decline was dramatic but is as yet unexplained."[2]

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In order to analyse this dramatic decline we must first examine John Shakespeare's economic history in as great a detail as the documentary evidence will allow. It has already been noted that his main trade was that of glover, and it is this occupational description that is applied to him in most contemporary records. (He is also described as a "whittawer" – defined as someone who turns skins into white leather.) However, he was also a dealer in wool and a moneylender, as well as someone who bought and sold timber, barley, and according to Lee, sheep, skins, meat and

leather.[3] The documentary record is confirmed by Shakespeare's earliest biographers, Aubrey and Rowe, who deriving information from local Stratford inhabitants, wrote that John Shakespeare had been a dealer in wool (Rowe) and a butcher (Aubrey).

In retrospect, Shakespeare's father's multiplicity of activities should not surprise us; Stratford had grown up as an agricultural marketing town, serving a wide area of midland towns and surrounding counties, stretching through Gloucestershire, Oxfordshire, Staffordshire, Cheshire, Lancashire, Shropshire and Wales.[4] Nearly all agricultural products were traded through the town – corn, seed, horses, cattle, and as we shall see later most importantly of all, barley and malt.[5] Other than the production of malt, the making of gloves and related products was probably the most important manufacturing activity in the Elizabethan and early Stuart period. The trade flourished during the late sixteenth century, culminating in the formation of the Glovers and Whittawers Company in 1606[6]; glovers occupied the most prominent position on market days, and some of the most eminent members of the corporation were glovers.[7] But focussing on John Shakespeare' trade as a glover, is quite misleading. As Lee has written about Stratford : "Small farmers lived there in number, and . . . they dealt in all the products yielded by the cultivation of land and stock. Thus, in 1597 George Perry, of Stratford, was described as engaging in, 'besides his glover's trade, buying and selling of wool and corn, and making of malt'."[8] And as Lee further points out, John Shakespeare cultivated far more land than the majority of his neighbours, having inherited land from his father in Snitterfield (the exact amount is not clear), as well as leasing fourteen acres of land called Ingon Meadow in 1570, and owning substantial land – about 86 acres – in nearby Wilmcote through his wife Mary's inheritance.

In order to understand John Shakespeare's economic situation, we must analyze it in the context of the very radical changes that were taking place in English society at the time. Population had begun to increase at the end of the fifteenth century, and by the middle of the sixteenth century was expanding very rapidly – probably largely due to the progressive elimination of the plague from the countryside. Wrigley and Schofield have estimated that population increased from 2,773,851 in 1541 to 4,011,563 in 1601: an increase of forty-five per cent.[9] According to their figures, the population grew most rapidly in the periods 1546-1556 and 1561- 1586, and this growth was associated with a rapid increase in the prices of agricultural produce. This can be illustrated by the changes in

the price of wheat at Exeter, for which there is a series going back to the year 1316. Looking at the price by half-century, it was stable for the two hundred-year period 1316-1499, varying between 6.9 and 6.21 shillings a quarter; in 1500-49 it increased slightly to 8.28 – and then in the following half-century, 1550-99, it jumped dramatically to 22.72, and continued to increase in 1600-49 to 37.81, before stabilizing again in 1650-99 at 39.52.[10] Most of this increase was probably due to a growth of demand resulting from expanding population, although the influx of silver and the debasement of the coinage probably also played a part.

Overall, between the beginning and the end of the sixteenth century, the price of grain rose sixfold and cattle and sheep by five times – the latter being typical of an increase in the prices of a basket of twenty-five consumer goods during the same period.[11] Prices of most products rose steadily throughout the whole of the sixteenth century, although there were some variations in particular decades. Cattle and sheep prices increased steadily and progressively throughout the whole of the sixteenth century, and this was also largely true of the products that John Shakespeare was trading in: wool and sheepskins. The latter had jumped in price in the 1560's – more than doubling in a decade – but continued to rise steadily thereafter; the former moved more erratically, and can best be charted by the following indices (1450-99 = 100): 1540-49: 153; 1550-59: 206; 1560-69: 205; 1570-79: 234; 1580-89: 225; 1590- 99: 315.[12] Wool prices were not only affected by domestic demand, but also by exports, and although the latter probably fell during the 1560s, they increased by about ten per cent in the next decade, changes reflected in the price of wool. There is no indication in these figures that John Shakespeare's economic problems were due to falling prices and a lack of demand for wool; on the contrary, the decade when he ran into difficulties – the 1570's – wool prices rose by over fourteen per cent.

The increase in population and the general level of prices led both directly and indirectly to a sharp polarization in the distribution of wealth, with some groups becoming very much richer and others poorer – with an additional growth in the absolute numbers of the poor. That this polarization did not become more extreme was largely due to the innovation in farming methods. It is a matter of controversy as to whether these changes constituted an "agricultural revolution", but there is general agreement that grain yields did increase, largely through the application of convertible husbandry, involving a mix of arable and

pastoral farming. As a result, yields are estimated as having increased by 30% between 1450 and 1650.[13]

The above changes all refer to national trends, and there were of course significant regional variations, particularly in agricultural developments. Although there has been no detailed study of the economic and social history of Stratford and the immediately surrounding area, we are fortunate in having Skipp's study of five parishes within the Forest of Arden, some fifteen miles north of Stratford. Population increased rapidly during the late sixteenth century, from about 2,250 in the 1570's to 3,400 in 1650: an increase of the order of 50%; the rate of natural increase was particularly strong during the last quarter of the century – 45%. The price of arable produce trebled between 1530-59 and 1590-1619 (a slightly greater increase than the national average), whereas cattle and oxen more than doubled during the same period.[14] Rents in this area "often lagged behind prices to quite an extraordinary extent", and the result was a marked increase in the wealth "of the farmer: as against the landless labourer or craftsman on the one hand, and the landlord on the other."[15] This increasing wealth of farmers was not primarily due to improved methods of cultivation, but was a direct consequence of the much higher prices for their produce, at a time when rents were lagging very greatly behind prices. This was not the case in all areas of England – in some places landlords extracted rents in pace with the rate of inflation – and it was perhaps the absence of such pressure that led to the new agricultural methods not being introduced into the Forest of Arden until the early seventeenth century.

The effect of all these changes was the "Great Rebuilding" and "Great Refurnishing" of this part of Warwickshire; this was reflected most visibly in the proliferation of timber-frame farm-houses. From the study of local inventories, it emerges that houses with four or more rooms increased from 14% in 1530- 69, 41% in 1570-99, to 76% in 1600-24.[16] This was associated with a general change in the pattern of consumption: "increased personal wealth led to a rise in the demand for consumer products of every kind."[17] The simple, crude stools, forms, trestles and table boards of the mid-Tudor period, progressively gave way to joined tables, benches, turned chairs and bedsteads. The value of household goods increased by about 300%, and this included personal clothing: the number of garments per person nearly doubled between 1530-69 and 1570- 1609, an increase from an average of 6.4 to 11.4.[18] The latter growth in demand was directly relevant to John Shakespeare's trade as a glover

and whittawer. This shift in consumption affected all household goods: between 1530-69 and 1610-49, the number of napkins listed in the inventories rose from an average of one to ten, table cloths from an average of one to two, flaxen sheets from three to six.[19]

The result was that the number of craftsmen increased significantly from the earlier period. Whereas in Tudor times many craftsmen were only part-time workers – working both as farmers and craftsmen – by the Elizabethan period there were large numbers of full-time craftsmen listed in the leather, textile, wood, building and metal trades.[20] Many of them were specialists – for example, in the building trade, sawyers, turners and joiners – and there was an associated rise in the quality of consumer products during the period. The changes not only applied to farming and craftwork but also to the whole structure of the economy; whereas in the earlier period, 1530- 69, only one in eight inventories specify ready cash, by 1610- 49 this had risen to one in three, and by 1650-89 eight in ten died with cash about them. Likewise, before 1575 there were no farmers owned monies lent out for interest, but by 1625-49 one in every four had risk capital out on loan.[21]

These changes occurred relatively late in the countryside, and the major development in trade and capital transactions took place in the rapidly growing towns, particularly London. An index of this is the change in the size of London's population, which according to Finlay was as follows: 1500: 50,000; 1550: 70,000; 1580: 100,000; 1600: 200,000; 1650: 240,000.[22] Although these figures are only very approximate, they do show a very rapid increase in the second half of the sixteenth century, particularly in the last two decades. London's population increased by nearly fifty per cent between 1550 and 1580, an indication of the growth of trading activity during John Shakespeare's most active years.

For Stratford itself, although we have no general economic and social history of the town, we are fortunate in having Martin's monograph on its demographic history for the Tudor and Stuart period. Using ecclesiastical and hearth tax returns, Martin has estimated that Stratford's population increased from 1,503 in 1563 to 2,597 in 1673 – an increase of 61.56%.[23] Although not quite so substantial an increase as the surrounding rural parishes, it nevertheless did grow very significantly over the whole period. There are no population figures available for the period we are most interested in: the latter half of the sixteenth century. It is possible to make very approximate estimates from the number of marriages; these increased from 173 in the period 1558-1568 to 195 in 1591-1601. Assuming

a constant marriage rate, population would have increased by about 12.71% from 1563 to 1596 – a rate of increase of 0.41% per annum, slightly less than the 0.56% per annum found by Martin.

Most of the growth in the late sixteenth century period must have been due to immigration: an analysis of the parish registers reveals that the number of baptisms between 1558 and 1601 was 2,682, whereas the number of burials was only slightly less at 2,667[24] – a natural increase of only fifteen people over the whole period. A major reason for this lack of natural growth was the large number of burials due to plague; there were two main plague years in this period: in 1564 (the year of Shakespeare's birth) 259 people died – representing about a sixth of the total population – while in 1597, there was only a slightly lesser mortality, with 181 people dying.[25] (The mortality in this year may have been partly due to dearth in the grain supply.) Stratford's experience illustrates what was true of many English towns: plague and other diseases periodically decimated their populations, giving rise to a severe check on natural growth. London was the most outstanding example of this: its enhanced mortality meant that in order for its population to grow, it had to absorb about half of the natural growth of the whole country.[26]

From this and other evidence, it appears that Stratford shared in the prosperity of the area, functioning as it did as a market town covering the complete range of agricultural and other produce. The town had become thoroughly capitalistic in its economic structure and ethos; virtually all the leading townsmen and neighbouring gentry were engaged in the hoarding of barley and malt, in spite of contemporary national legislation and local council rulings against forestalling and engrossing.[27] On more than one occasion this nearly led to rioting during periods of poor harvest, and this will be dealt with in greater detail later in the book. In order to exploit the huge growth in profitability of farming, it was not enough to merely provide consumer goods and services for the surrounding population; most of the leading townsmen functioned not only as producers, but were also engaged in speculative activity. This can be illustrated by the example of Thomas Rogers, Bailiff of the Borough, who in 1595 was a butcher by trade, but was also engaged in extensive buying and selling of corn, malt and cattle.[28] John Shakespeare's multiple trading activities were not in themselves abnormal in Elizabethan Stratford, but the scale and scope of his ventures did differ from those of the average Stratford tradesman. This is so important for understanding John Shakespeare's economic position – and the way this shaped the

milieu which in turn influenced Shakespeare's cultural world – that the whole subject will be considered in some detail.

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John Shakespeare's father, Richard, probably lived for most of his life in Snitterfield, a village about three miles north of Stratford, and is known to have been a copyholder on one manor from at least October 1535 until October 1560, and a tenant on Robert Arden's land from 1528-60. When he died in 1561, Richard Shakespeare's goods and belongings were valued at £39.17s. – approximately £5,600 at current prices.[29] (The current value of all economic transactions will be indicated in brackets in order to appreciate the sums of money involved – all prices will be inflated by a ratio of 140, a minimal figure which is discussed in footnote 29.) Richard Shakespeare was a relatively prosperous husbandman, but like all small tenant farmers at this time, operated on a very modest scale. Skipp has summarized the position of farmers in the Forest of Arden : "The average fully inventoried summer farm for the period 1530- 69 is estimated to have covered about 33 acres, of which roughly a third (10 acres) was arable and two-thirds (23 acres) grass. In other words, the mid-Tudor peasant was growing corn almost exclusively for subsistance purposes; for profit he concentrated on animal husbandry."[30]

There is some uncertainty as to how much land John Shakespeare inherited from his father; nothing is known about his direct inheritance but there are two surviving documents referring to sales of land by John Shakespeare in Snitterfield to Robert Webbe in October 1579 for £4 (£560) and in Easter 1580 for £40 (£5,600).[31] This could either be a reference to two separate parcels of land, or a mistake in the price of the same plot of land – although from the wording of the documents, it looks as if the smaller plot was inherited from Richard Shakespeare, and the larger through John Shakespeare's wife, Mary Arden. What is clear, is that when John Shakespeare married Mary Arden some time before 1558, he inherited eighty- six acres of land at Wilmcote, near Stratford, which had been left to Mary by her father Robert Arden, Richard Shakespeare's landlord.[32] Soon after 26 November 1556, when Robert Arden made his will, John Shakespeare had probably acquired through marriage over 100 acres of land – and this put him into the category of a prosperous yeoman farmer by contemporary standards. However, he had moved to Stratford by 1552, and at that time was working as a glover,

presumably after a seven-year apprenticeship in the town. He was probably born in about 1530 and so at the beginning of the 1550's, was a young man in his early twenties.

There is evidence that John Shakespeare had already commenced his climb to prosperity before his marriage to Mary Arden, for he had begun making investments and speculating in commodities previous to that date. In June 1556, he was sued by Thomas Siche, a husbandman from Arscote in Worcestershire, for a debt of £7 (£980) – and the case was resolved by default in John Shakespeare's favour. This dispute had led to violence but John Shakespeare had pleaded a use of just force on Siche – perhaps an indication of his strong and forceful personality.[33] In October, 1556, he bought two houses, one in Grenhill Street, presumably an investment, and the other in Henley Street – probably the house (East House) he had lived in since 1552, and bought in preparation for his marriage. In 1575 he paid £40 (£5,600) for the Middle and West Houses in what is now known as the Birthplace, and so his purchase of two houses in 1556 constituted a considerable outlay of money. (Another way of seeing this, is to set it against the incomes of various groups at this time: as late as 1688, Gregory King stated that a shopkeeper and tradesman's income was only £45 a year, with labouring people and out servants earning as little as £15.)

The first evidence of speculative activity on John Shakespeare's part, comes from a suit he bought against Henry Field, of Stratford, in November 1556 for the non-delivery of eighteen quarters of barley. Barley was fetching about £1 a quarter, and so this consignment was worth the very considerable sum of £18 – or about £2,500 at present values.[34] Prices of barley had increased significantly during the 1550's – rising from an index figure of 197 in 1540-49 to 450 in 1550-59[35] – and it appears that John Shakespeare was speculating in this commodity to maximize profits in a rapidly rising market. The engrossing and forestalling of barley and malt was commonplace in Stratford, and from the very beginning of corporation records, there is reference to such activity. In October 1555, the council prohibited inhabitants of the town from buying or storing barley for non-residents (although the storage of malt was allowed), and this presumably was enacted to create a monopoly for residents.[36] This was an era when trade was controlled and regulated, illustrated by the town's attitude to non-resident speculators buying corn on the open market : "no farrener beynge a badger shall not in eny on market day from henesforth lode ouer & above iij horysys but yt they

Brynge the Justice letteres for the safcondyt & ley vp no corne in peyne of £v."[37] This restriction on trade was to have very important consequences, in creating a new body of individual traders operating outside market towns – and we will see that Shakespeare's father was one of the first to trade in such a fashion.

John Shakespeare continued to be very active in local trade, and became embroiled in a number of legal disputes. Unfortunately, the details of all these are not known, partly because the Court of Record for which most evidence is available, dealt only with local issues, and very often only gave superficial information, particularly during the period 1570-84 when records were incomplete. Nevertheless, John Shakespeare was involved in twenty-five suits over a forty-year period, in sixteen of which he was the complainant and nine the defendant.[38] Where information is available, the evidence is that most of these disputes were for relatively small sums; this was because the court was purely a local one, and was used by tradesmen for collecting debts and settling other relatively minor matters.

It is only recently that it has been realized that John Shakespeare did not confine his business activities to Stratford, but ranged very widely, both geographically and economically. In 1984, Thomas and Evans published a summary of the findings of legal cases conducted in the Court of the Exchequer, which completely changes our perception of John Shakespeare's business life.[39] Four prosecutions were brought against him, two in 1570 alleging illegal loans, and two in 1572 for illegally dealing in wool. In 1570, all forms of money-lending for interest were illegal, and in order to enforce the law, the government rewarded informers by granting them a half share of penalties imposed upon offenders. The first informer was Anthony Harrison of Evesham, Worcestershire, who claimed in the formal information submitted on the 21st October 1569, that John Shakespeare had lent John Mussum of "Wulton" (Walton) in Warwickshire the sum of £100 (£14,000). The money was loaned some time after 26 October 1568, and the transaction was alleged to have taken place at Westminster; the money was to be repaid on or before the 1st November 1569, together with £20 (£2,800) interest – representing an annual rate of interest of 20%. No record of any subsequent proceedings has survived, and it is possible that Harrison and John Shakespeare settled out of court.

The second accusation was laid later in the same year by James Langrake of Whittlebury, Northamptonshire. He claimed that John

"Shappere alias Shakespeare", glover of "Stratford upon Haven", had lent John Musshem of Walton, Warwickshire £80 (£11,200) some time after the 25th October 1568, the loan to be repaid on or before 30th November 1568, with £20 (£2,800) interest – an exceptionally high annual rate of interest of about *260%*. The transaction also took place at Westminster, but in this instance, it was pursued to a conclusion; the judges of the Exchequer issued a writ to the sheriff of Warwickshire to bring John Shakespeare to court, but in the event, on the 3rd February 1570, he came to London of his own volition. Although denying guilt, he compounded with the court and was fined forty shillings. John Mussum was probably a business partner of John Shakespeare's. (Walton was just outside Stratford, and both Mussum and John Shakespeare were sued for debt in 1573 and 1578 by Henry Higford of Solihull, who had previously worked in Stratford as town clerk.) That both transactions took place at Westminster, on two consecutive days, and that the second one was for a very short period at a very high rate of interest, suggests that the loans were a part of a series of highly speculative and profitable ventures, made at very short notice.

Two years after the prosecutions for usury, John Shakespeare again suffered from the attentions of Langrake. In February 1572, Langrake claimed that John Shakespeare and John Lockeley, both of "Stretford super Haven", had illegally bought from Walter Newsam and others 200 tods of wool, at fourteen shillings per tod – a total purchase price of £140 (£19,600). The offence was committed on or after 26th February 1571, and again took place at Westminster. Later in the same year, Langrake claimed that John Shakespeare on or after the 1st September 1571 had bought at Snitterfield from Edward and Richard "Graunte" and others, 100 tods of wool at fourteen shillings a tod – a total purchase price of £70 (£9,800). There is no further record of these prosecutions, and it is probable that John Shakespeare compounded with Langrake in an out-of-court settlement. The prosecutions were brought under a statute of 1552, which allowed only manufacturers or merchants of the Staple to buy wool, and the potential penalty was the forfeiture of double the value of wool bought.

Not a great deal is known about the parties involved in these various prosecutions; virtually nothing is known about Anthony Harrison, except that he came from Evesham in Worcestershire. John Lockeley was another of John Shakespeare's partners, and was also a glover in Stratford-on-Avon; Richard Grant was Edward Grant's father, and they

were members of a Catholic gentry family which owned an estate at Northbrook, near Snitterfied in Warwickshire – and will appear again later in the book. James Langrake was accused in 1570 of raping one of his servants, and attempted to intimidate his accusers by threatening to bring an accusation against them in the Exchequer – the outcome of the case is not known. In 1574 he was imprisoned along with eleven other informers for compounding with offenders without the agreement of the court, and in the following February he was fined £40 (£5,600) and banned from bringing further informations for a year.

Although usury was an offence under the law, it was essentially anachronistic. It had been practised widely during the medieval period, and had been legalized for a time during Henry VIII's reign. The law under which John Shakespeare was prosecuted was modified in 1571, with those lending under ten per cent only having to forfeit the interest if prosecuted. However, although usury can be seen as an outdated economic crime, it was still attracted great moral opprobium, and was a relatively rare practice in 1570 – only a total of 181 prosecutions for usury took place that year in the Exchequer, and as we have seen from Skipp's study of the Forest of Arden, none of the farmers' inventories showed money out on loan before 1575. In this respect, John Shakespeare's money-lending activities were something of an innovation in this area of Warwickshire.

It is important to realize that these speculative projects were not confined to the years 1568-71; we have already seen how John Shakespeare was buying barley in 1556, and continued to be embroiled in various disputes about money during the whole of his working life. In 1599 he sued John Walford of Marlborough in Wiltshire for non-payment of £21 (£2,940), John Shakespeare alleging that he had sold Walford twenty-one tods of wool at Stratford on the 4th November 1568, and that the £21 owing in cash had never been paid.[40] In 1571 he sued Richard Quiney (father of Thomas Quiney who married Shakespeare's daughter Judith in 1616) for £50 (£7,000)[41], and the following year brought a summons in the Court of Common Pleas against John Luther, which is sufficiently revealing to be worth quoting fully:

> "London. John Luther, late of London, glover alias John Luther of Banbury, Oxon., glover, was summoned to answer John Shaxbere of Stretford on Avon in co. Warwick, yeoman, in a plea that he owed him £50 (£7,000) etc. And John Shaxbere, by Henry Burr his attorney, said that John Luther on 16 June 13 Eliz. at London by a certain writing obligatory

> had acknowledged himself bound to the same John Shaxbere in the aforesaid £50 to be paid on demand. The said John Shaxbere had been damaged to the value of £10 (£1,400). And he produced there in Court the writing. And John Luther, by Thomas Gardener his attorney, came and defended force and injury, etc."[42]

The case went in favour of John Shakespeare and he was awarded 33s.4d. (£233) costs, but the significance of this document is that although John Shakespeare was probably unable to write – he signed all documents with a cross or his mark, a pair of glovers' dividers – he was capable of conducting complex transactions in writing, and therefore was presumably able to read. But perhaps more importantly, this case against Luther reveals the cosmopolitan nature of John Shakespeare's business life – engaged in dealings with someone who moved between London and Banbury, the transaction not only taking place in London, but also being brought to trial there. John Shakespeare had dealings with people living in London, Worcestershire, Northamptonshire, Oxfordshire, and, we shall see later, in Coventry, Nottingham and Stoke in Staffordshire. Most of his larger transactions took place in London – confirming the earlier emphasis on the importance of London as a trading centre – and this evidence totally changes our perception of John Shakespeare's world. His son William's background has always been thought of as a narrow provincial one, and this has been one of the difficulties in accepting Shakespeare's authorship of the plays. But John Shakespeare's life was anything but provincial, and we will now consider his economic and cultural world in some detail.

CHAPTER 3:
JOHN SHAKESPEARE'S CULTURAL WORLD

In 1588, John Shakespeare made a submission in connection with a dispute with his nephew, John Lambert, over the sale of land, and included in that submission a reference to twenty pounds he claimed was owing to him. Most revealingly, he stated that he had "totally lost and failed to acquire the whole gain, advantage and profit which he by buying and bargaining with the aforesaid twenty pounds have had and acquired, to the loss of thirty pounds."[1] This is the credo – "buying and bargaining" – of the middleman, a group whose activities Everitt has designated, "the free trading between individuals".[2] He has defined this as the "type of bargaining which was mostly nearly 'free', or emancipated from official control: to dealing between individual traders, farmers and manufacturers in private."[3]

But before we consider the evidence that Everitt has published on the organisation and culture of this group of individual traders, we must examine information directly relevant to John Shakespeare's occupation as wool dealer. Bowden, in his definitive study of the wool trade in Tudor and Stuart England, provides us with the necessary background to understand John Shakespeare's transition from glover to wool-dealer. Writing about middlemen in the wool trade, Bowden tells us:

> "The second group of middlemen – the glovers, fellmongers, leathersellers etc – came to deal in wool through their interest in sheep skins, which they bought as a normal part of their business. When purchased from the farmer these skins were covered by a growth of fell wool, and this had first to be removed before the pelts could be put through the various processes of manufacture. As the wool was no use to the leather industry it was then sold (sometimes sorted according to quality) to wool dealers and manufacturers. From selling superfluous wool to dealing in fleece wool was but a short step to take; and in the second half of the sixteenth century the trade

> in fleece wool of a few of the members of this group appears to have been as large as that conducted by many of the Staplers."[4]

And if we had any doubt that this applied to the Warwickshire area, Bowden goes on to state that the "glovers of the central and east Midlands . . . were great wool dealers."[5] Remnants of wool were found in John Shakespeare's Henley Street house (now known as the Birthplace), and part of this house was traditionally referred to in Stratford as 'the woolshop'; Bowden tells us that after the wool was bought, it was most frequently "carried to the dealer's house or wharehouse".[6] Along with the documentary evidence on John Shakespeare's dealings in wool, this extra information amply confirms the accuracy of Rowe's statement that John Shakespeare was a wool-dealer.

It is likely that John Shakespeare used sheep from the farms that he owned and leased for both fleeces and wool, and given that there were several references to him working as a butcher (this will be discussed later), he probably butchered as well as skinned sheep. In addition to the sheep from his own land, he was in a very good position through his contacts with farmers in the surrounding countryside – both his father and brother were small farmers – to gain ready access to other supplies of sheep. According to Bowden, one of the most important types of "wool growers were the husbandmen or 'petty breeders'. These small farmers, often living to the margin of subsistence, needed money with which to get in their harvests, to pay their rents, or simply to meet normal, everyday expenses."[7] Skipp has described the husbandmen of the Forest of Arden, with their thirty-three acre farms, two thirds of which were put down to grass to raise animals for cash – a natural source of supply for John Shakespeare's wool-dealing business.

But wool-dealing also contained the seeds of money-lending: "When a seller gave credit for wool he received a higher price for it than he would have done had he accepted payment in ready money. The price of wool sold on credit thus contained an element of interest . . . [and in] the sixteenth century this interest charge was normally disguised as part of the principal, but after the [Usury] Act of 1571 . . . it was sometimes recorded as a separate item."[8] And so John Shakespeare's business as a wool-dealer may well have directly led him into explicit money-lending ventures, although the evidence considered suggests that, quite independently, he had become one of Everitt's "individual traders", willing to "buy and bargain" any commodity that would make a profit.

Everitt has shown that this type of trading grew rapidly in the sixteenth century, particularly after about 1570. He has made a study of it through the records of disputes between traders in the Courts of Chancery and Requests, which provide a detailed picture of John Shakespeare's economic and cultural world. The majority of transactions took place privately in inns and farmhouses (to escape the trade restrictions such as those imposed by on corn buyers by Stratford Corporation), and were on a sufficiently large scale, to require goods to be delivered at a later date, frequently in several instalments. All were conducted on a credit basis, for which legal bonds were drawn up by a lawyer or scrivener. Many of the traders worked with partners, although these partnerships were very frequently only ephemeral arrangements. According to Everitt, because of the absence of banks, traders necessarily had to rely on their credit in the local community, and this often "operated through a network of neighbours, friends, and relatives. Sons, fathers, brothers, cousins, wives, uncles, mothers, brothers-in-law: all were drawn into the circle."[9]

In John Shakespeare's case, it does seem that he turned to neighbours, friends and relatives for financial help during periods of difficulty. Although there is no direct evidence that Shakespeare worked with his father on his trading activity, John Shakespeare did associate his son William with his legal suit against the Lambert family in 1588. Fripp argues very convincingly that Shakespeare showed "extraordinary knowledge, and large accurate usage, in his writings from the beginning, of legal terminology and procedure" (*Shakespeare: Man And Artist*, Vol.1, page 138), and it is probable that he worked for his father in drafting legal bonds for trading transactions. (Possibly under the tutelage of Henry Rogers, John Shakespeare's business partner and town clerk of Stratford?) Everitt has described the culture which grew up amongst individual traders:

> "In consequence of this network of kinship and acquaintance, the packmen, carriers, woolmen, and factors who engaged in the private agricultural market were not simply unconnected individuals. By the end of Queen Elizabeth's reign they had developed into a distinct and self-conscious community on their own: a kind of society of wayfarers, partially separate from the settled society of the manor house, a village, and market town. It must not be supposed that this society was altogether new; its origins went back to the medieval wool merchant, and perhaps beyond. But it was at the end of Elizabeth's reign, so far as the agricultural market is concerned, that it became a recognizable *community*, with its own characteristic customs, traditions, and ideals. Much of the dealing in which travelling merchants engaged took place in farmhouses. Some took place in

> barns, and some in warehouses and corn-chambers. Perhaps the most characteristic meeting-place of the wayfaring community, however, was the provincial inn. The Elizabethan inn has no exact counterpart in the modern world. It was the hotel, the bank, the warehouse, the exchange, the scrivener's office, and the market place of many a trader."[10]

John Shakespeare's carried on many of his business meetings in London, and perhaps Everitt under-estimates the importance of London as the centre of internal trading. But with that caveat, and allowing for the relatively early period of John Shakespeare's trading, much of the above would apply to his case. No doubt many of his business meetings took place in the inns of Stratford and the surrounding area, although he was almost certainly familiar with the inns of Westminster and other areas outside the Stratford region. Everitt has elaborated on the role of the innkeeper in trading activities:

> "The Tudor and Stuart innkeeper was thus in a powerful position to influence the course of private trading. Many a publican provided cellars or outbuildings for the storage of his clients' goods. Some converted their halls or parlours into private auction rooms. A few engaged in private dealings on their own account . . . Most innkeepers, however, confined their activities to 'finding chapmen' for customers and arranging bargains between two of his customers himself, as they were sitting at supper in the hall of the inn. Agreement between prospective dealers was rarely reached without a lengthy series of 'speeches' and 'communications', and the company often sat far into the night before the transaction was concluded. Sometimes an unscupulous innkeeper would allow some hapless yeoman (well plied with ale) to be 'cozened of his money' by the 'glozing terms . . . smooth words, and fair speeches' of the other party concerned; though no doubt most landlords, for the sake of their own reputation, endeavoured to encourage fair dealing. When the bargain was agreed, the local scrivener (sometimes himself one of the guests) was called upon to draw up one of the bonds, and the deed was read out to the assembled company."[11]

If this passage conveys the impression that the culture of the traders was defined solely by the inn and its practices, Everitt corrects this impression by bringing out the more spiritual side of their life:

> "It is not surprising if the wayfaring community developed an ethos of its own dissimilar to that of the settled society of town and village. Its spirit of speculation and adventure ran counter to the stable traditions of the English peasantry . . . it is not fanciful to trace a connection between the spread of private trading in the early seventeenth century and the rapid rise of Independency. For Independency was not a rural and static religion, like anglicanism, nor rigid and urban, like presbyterianism, but mobile, virile, and impatient of human institutions, like the wayfaring community itself."[12]

Perhaps Everitt presents too ideal a picture of the independent trader's ethos and culture, but it does have the merit of pointing up the cosmopolitan and dynamic nature of this way of life. It also makes it much more comprehensible as to how Shakespeare acquired the cultural knowledge and background to write plays of such universal and general appeal. We will return to Everitt's work later in the book, as it provides one of the keys for explaining John Shakespeare's fall. Before leaving it however, we should note that Everitt does recognize that the activities of a trader like John Shakespeare were not always confined to one category; writing of "the conflicting aspirations of the market town and private trader", he notes that "many traders engaged in both spheres of activity, and it would be misleading to draw too sharp a distinction between them."[13] And this was particularly the case with Stratford: we will see later how most of the leading townsmen engaged in speculative trading in corn, and perhaps a majority of them could be categorized as individual traders during this period.

This can be illustrated by John Shakespeare's friend and associate, Adrian Quiney; he was a mercer by trade and lived in Henley Street (he was fined along with John Shakespeare for making an unauthorized dunghill in 1552). He and John Shakespeare served on the council together – they both held high office during the period of the reform of the Gild Chapel – and in 1571, when Quiney was bailiff and John Shakespeare was chief alderman, they "rode to London together on borough business, with permission from the aldermen and burgesses to proceed 'according to their discretions'."[14] Adrian Quiney was a business partner with his son Richard, and the correspondence between the Quineys and Abraham Sturley, reveals how widespread individual trading was in Stratford, with frequent references to loans, investments and possible speculative bargains.

An example of this is the loan/ business transaction that Richard Quiney sought to bring off with Shakespeare in 1598. Quiney appears to have been short of money, and in the first instance, sought a loan of £30 from Shakespeare. But a letter from Adrian to Richard in 1598 suggests a much broader possible arrangement, and is very revealing of local attitudes to money and trade : "Yff yow bargen with Wm Sha .. or receve money therfor, brynge youre money homme that yow maye; and see howe knite stockynges be sold; ther ys gret byinge of them at Aysshome. Edward Wheat and Harrye, youre brother man, were both at Evyshome thys daye senet, and, as I harde, bestow £20 [£2,800] ther in knyt hosse; wherefore I thynke yow maye doo good, yff yow can have money."[15]

We see here a direct link between the trading activities of John Shakespeare and his son William, and it is clear that such activities were the norm in Stratford, rather than the exception. The Quiney family was one of the most respectable in the town: they bore arms, had been long settled in the community, and were influential members of the corporation.[16] They were well-educated – Richard conducted much of his correspondence with Abraham Sturley, who had been educated at Queen's College, Cambridge, in Latin – and appear from the language of this correspondence, to have been strongly puritan. Nevertheless, along with all other leading townsmen, they frequently engaged in speculative activity (particularly in corn and malt), and Adrian's letter to his son Richard, brings out just how far they were involved in a bargaining mentality – although in the example quoted, they were buying and selling stockings for their mercer's business.

John Shakespeare in the earlier period was just as respectable and eminent in the town as any of the other residents. Starting in 1556, he was appointed to Stratford Corporation, and held office continuously until the 1570's: he was successively aletaster (1556), burgess (1557), constable (1558), afeeror (1559 and 1561), chamberlain (1561 and 1562), alderman (1565), high bailiff (1568-69), and chief alderman (1571).[17] It is true that he was fined for non-attendance in 1557 and probably at other times as well, but his overall record of attendance was as good as any other member of the council. He is known to have missed only one meeting of the corporation in the thirteen years that records were kept from 1564 to 1577.[18] By any standards, Shakespeare's father was a highly conscientious and active member of the town council, until his sudden withdrawal at the end of 1576 or the beginning of 1577. After that date, he ceased to attend, even though he was still nominally an alderman for another ten years.[19] John Shakespeare's role as a member of the corporation should not be exaggerated however: the council only met about once a month, and there are only thirty-two specific occasions when he is mentioned as attending in the eleven-year period 1566-77 (although there is only one occasion when he is listed as being absent.) There is no inconsistency between regular participation in corporation affairs, and life as an individual trader, including visits to London and elsewhere. In fact, he was prosecuted for usury and wool-dealing at the very time he had achieved highest office in Stratford – 1568-71 – when he was bailiff and chief alderman, and had visited London with Adrian Quiney on council business.

His last known date of certain attendance was on the 5th September 1576, thus suggesting he withdrew from corporation meetings sometime between

the 5th September, 1576 and 23rd January, 1577. That this withdrawal was not made directly on economic grounds is indicated by a number of items of evidence. The following is recorded in the Minutes and Accounts of Stratford Corporation : "5th December 1576 – It is also ordered and agreed upon that every alderman shall pay (savinge Mr Lewes and Mr Plumley) xii.d. [£7] a pece this present yere towardes the wages of the common bedyll, and the said Mr Lewes and Mr Plumley to pay viii.d. [£4.65] a pece, and all the burgesses shall pay iiii.d. a pece this present yere savinge that Mr Nicholas Barnehurst shall pay for his part xii.d. towardes the wages of the said bedyll."[20] It was customary for the levies on burgesses and aldermen to be a reflection of their economic status; burgesses being in the main poorer than aldermen paid a third of the latter's levy, and individual aldermen, such as Mr Lewes and Mr Plumley paid less on account of poverty, or more in the case of Burgess Barnehurst because of his wealth.

At this critical time of Alderman Shakespeare's life, he was considered by his colleagues to be capable of paying the full levy for an alderman. But a year later, the position had changed dramatically: "29th January 1578: At this hall yt ys agreed that every alderman, except suche under wrytten excepted, shall paye towardes the furniture of three pikemen, two billmen and one archer vi.s. viii.d.[£47] . . . Mr Shaxpeare iii.s. iv.d.[£23.50] ..."[21] Up until this point, John Shakespeare had always been assessed in the normal way, and during the twenty years he had been associated with the council, he had if anything, loaned money to the corporation in his day-to-day business on their behalf. And here he was being levied at half the normal rate for an alderman – the first indication of his ensuing economic difficulties. He failed to pay this reduced levy, and was altogether excused on the 19th November 1578 from a weekly contribution of 4d. towards the relief of the poor.[22] During this same year, as we will see later, he began to sell and mortgage land which he had acquired through inheritance.

That his withdrawal from council meetings was not initially due to economic factors is further confirmed by current corporation practice; when a member of the council got into economic difficulty, the corporation either made reduced levies or allowed the member in question to resign on grounds of poverty, and this certainly happened to other members of the council.[23] In spite of John Shakespeare only attending once – as far as is known – during the ten years after 1577, no attempt was made to expel him from the council; but finally, on the 6th September 1586, it was noted in the corporation minutes, that "Mr Shaxspere dothe not come to the halles when ... warned nor hathe done of long tyme" – and he was expelled.[24] It is clear

from this that it was not any antagonism on the part of the council – either on economic, religious or social grounds – that was responsible for his withdrawal; on the contrary, the corporation had been extraordinarily patient before expelling such a prolonged absentee, which presumably was a measure of their esteem given his previous twenty years of conscientious and valuable service.

The puzzle of the dramatic transformation in John Shakespeare's life is deepened when we examine his more general economic circumstances during this period. All the evidence is that he continued to enjoy prosperity and social position right up until the time of his withdrawal from the council; in October 1575 he bought two houses costing £40 (£5,600)[25], and more importantly, in about 1576 he applied for a grant of arms, which appears in part to have been successful. According to the commentary attached to the grant of arms made to Shakespeare senior in 1596, "This John shoeth a patierne under Clarent Cookes hand – paper xx years past."[26] As Fripp has pointed out, in order to apply for a grant of arms the applicant had to convince the relevant authority that he could live without "manual labour" and "bear the port, countenance and charge of a man of substance"[27], and apparently John Shakespeare successfully convinced the Clarencieux King-at-Arms Robert Cook that he was capable of this, as Cook did suggest an actual 'patierne' for a grant of arms. Clearly, John Shakespeare considered himself a sufficient economic and social success in about 1576 to warrant the title and social status of 'gentleman'.

One possible explanation for the sudden decline in Shakespeare senior's economic and social fortunes is ill-health – this in principle could account for both his sudden poverty, and his abrupt withdrawal from corporation meetings. However, when a councillor was sick he was excused attendance, with the explicit statement that the person was 'infirmus'[28] – and there was no indication in the language with which John Shakespeare was expelled – "dothe not come to halles when . . . warned" – that there was any presence of illness. However, there is one form of explanation that has attracted a great deal of scholarly interest, and which will be considered in some detail in the next section – that John Shakespeare was a religious recusant and withdrew from the council on grounds of religious persecution.

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There have been two diametrically opposed religious explanations as to why John Shakespeare withdrew from corporation meetings: one that he was a Puritan, the other that he was Catholic. The most scholarly advocate

puritan thesis is Edgar Fripp. Most of Fripp's evidence is taken from Stratford corporation records; in 1563 and 1564/1565 Shakespeare's father was active chamberlain for the borough, and made the following payments which were incorporated in his annual account: "10 Jan. 1564 – Item payd for defaysing ymages in ye chappell – 2s.; March 1565 – Item payd for taknge doune ye rood loft in ye chappell – 2s.; Item payd to Peter Start for workynge ye seattes – 6s."[29] These were typical puritan measures and there were other similar items of general expenditure on the chapel during this period, suggesting that the building was undergoing a structural reformation along Protestant lines. The chapel had originally belonged to the Gild of the Holy Cross, but became the property and official chapel of the Stratford Corporation in 1553 when the borough received its charter. (The corporation met in the Gild Hall adjoining the chapel, which was near the centre of the town, unlike the parish church which was on its periphery.) In 1571 when Shakespeare senior was Chief Alderman (deputy mayor) it was decided by the council that "Mr Adrian Queny (the mayor) ... shuld sell the copes and vesmentes", and later in 1573 when John Shakespeare was an ordinary alderman, the corporation arranged "for glasinge the chapell wyndowes".

It was on the basis of this and other evidence that Fripp concluded that John Shakespeare was a Puritan, and for this period when he was active on the council, the evidence certainly points in this direction. The conclusion has been disputed by other historians, such as Mutschmann and Wentersdorf, who have argued as follows: "At the end of 1563, the Guild Chapel . . . was Protestantized, and the images were defaced. John Shakespeare it is true, was a member of the corporation, and as Chamberlain had to pay the bills for the defacing; there is nothing, however, to indicate his personal views about this procedure, which was carried out in compliance with government instructions, and very tardily at that."[30] There are a number of problems with this argument: firstly, it ignores the fact that John Shakespeare was actively associated with nearly *all* the extensive changes in the chapel. Secondly, it is largely self-contradictory: the "lateness" of the reforms (and they were certainly later than elsewhere – London made orders for the destruction and sale of Catholic objects in 1559, Leicester in 1561[31]), would highlight the role of *John Shakespeare* in making the changes, i.e. suggesting that he took a major part of the initiative in carrying out reforms when he achieved office.

But the religious affiliation and attitudes of Shakespeare's father, can only be properly understood in the context of the religious and social life of the town, and as this is somewhat complex, I will attempt to summarize its

relevant features. The corporation was the successor to the medieval gild, and acquired most of its property and much of its structure of authority from the gild. Additionally, its social life was partly determined by the nature of the gild; not only was the corporation considered a "brotherhood" – loyalty to the council by its members was a primary requisite – but it was also seen as a religious fraternity. Quoting Fripp: "On Leet Days, Fair Days, and certain other occasions, Aldermen and Burgesses attended in their gowns at his [the Mayor's] house to escort him to Church or through the market or in perambulation of the Borough boundaries. They wore their gowns at Church on Sundays and holidays, and had seats near the pulpit."[32]

There is no direct evidence about John Shakespeare's attendance at church during the period he was a member of the council, but we do know that he was one of the most active members of the corporation (certainly in terms of attendance at its meetings), and as Bailiff and Chief Alderman at different times, he would have attended church as a part of his official duties. But what is the evidence that Stratford was a puritan corporation, other than the information already quoted from council records? *The Victoria County History of Warwickshire* has summarized one reading of the evidence as follows: "The defacing of the images in the Gild Chapel, carried out when Shakespeare's father was chamberlain in 1563; the prohibition of stage plays in 1602, and the tone of the correspondence and wills of many of the leading townsmen all indicate the puritan atmosphere of Elizabethan Stratford."[33]

There is support for this conclusion from other sources; in January, 1575, Warwick Corporation wrote of the members of the Stratford Council that "they are men known of good credit, honest behaviour, upright dealing and such as upon their credits might be trusted"[34] – and coming from a body known for its strong puritan leanings, this must be taken as confirmation of the overall puritan nature of the Stratford Corporation. (Although in the light of the previous discussion of the importance of personal credit amongst traders, it is interesting to note the emphasis on credit-worthiness in this recommendation.) Likewise, when a survey of the ministry was made in Warwickshire in November, 1586, it was noted that the minister at Stratford, the Reverend Barton, was "a precher, learned, zealous and godlike and fit for the ministerie".[35]

But this image of a strongly puritan Stratford is misleading in some respects. The council throughout John Shakespeare's lifetime supported and paid for the performance of plays in the town virtually every year (in some years three or four companies of players were paid for[36]); the town entertained the players with drink and food at the local inns, The Bear and

The Swan, as well as entertaining local justices, visiting preachers and its own officers, and expenditure on wine was one of the chief expenses. However, during the Elizabethan period, the antagonism of puritans towards the theatre and drinking was not as strong as it was to be in the following century, and it has even been argued that during the early period of the Reformation there was a natural alliance between puritanism, the theatre and the drink trade – all antagonistic to the old Catholic authoritarianism and suppression of free discussion of new ideas and practices.[37] (Fripp has also pointed out that a number of the innkeepers in Warwickshire were of the puritan persuasion – not surprising given that many of them were acting as brokers and bankers for individual traders, many of whom were ardent Protestants.) And the behaviour of the leading townsmen in other contexts – legal disputes over debts, physical assaults and scurrilous personal attacks[38] – hardly suggests the sober and restrained respectability that we latterly associate with puritanism.

None of this however contradicts Fripp's argument that during the period that John Shakespeare was a member of the corporation he was at the very least sympathetic to the Protestant cause, and even possibly strongly in favour. Fripp uses this conclusion to go on to argue a much more contentious thesis as to why Shakespeare senior abruptly withdrew from the council, and suddenly appeared to lose economic status. Fripp's argument runs as follows: as a result of Bishop Whitgift's investigation of recusancy in Warwickshire in 1577, John Shakespeare became frightened and "went to earth" (withdrew from public life), and in order to cover up his recusancy he became "suddenly anxious to appear 'of no account', 'a very beggar', ready to plead 'debt' and 'fear of process', unwilling to pay his levies and fines . . . "[39] Fripp concluded that Shakespeare's father in effect went into hiding; he withdrew from public life and presented himself as a virtual bankrupt in order to escape the effects of prosecution for being a puritan recusant.

The evidence however when scrutinised in detail does not support Fripp's thesis: Bishop Whitgift did not institute his inquiry until *October 1577*[40], whereas John Shakespeare stopped attending corporation meetings some time between the 5th September 1576 and 23rd January, 1577, i.e. *about a year before Whitgift's enquiry*. It is true that the Grand Ecclesiastical Commission had been set up in April 1576 to amongst other things "order, correct, reform and punish any persons willfully and obstinately absenting themselves from church and service"[41], but as Fripp himself points out, the Commissioners were mainly concerned with "papists", and only acted

were not started until October 1577. There is other even more important evidence against Fripp's thesis, but this will be considered at a later and more appropriate stage.

The alternative hypothesis, that John Shakespeare's withdrawal from the council and fall into poverty was due to his espousal of Roman Catholicism, has most forcibly been argued by Mutschmann and Wentersdorf: "The explanation of John Shakespeare's abrupt break with borough life must be sought . . . with the beginnings of the Catholic counter-reformation in England, the mission whose aim was to rouse Catholics from their easygoing attitude of outward conformity with the Anglican Church . . . Among those who responded by becoming open recusants voluntarily withdrawing from public life rather than take the forbidden oath of supremacy or attend Anglican services, was Alderman John Shakespeare."[42] Like Fripp, Mutschmann and Wentersdorf explain Shakespeare senior's economic difficulties as being the result of his religious recusancy; directly through the imposition of heavy fines, and indirectly through the attempt to avoid fines by handing over his property "for safekeeping to trustworthy friends", devising his "lands to tenants, friends or . . . relatives", some of whom turned out to be untrustworthy and refused to return this property after the pressure of religious persecution had lifted.[43]

Most of this argument is speculative, lacking any direct evidence or documentary confirmation. The only evidence solidly in its favour is the Catholic religious will and testament made in John Shakespeare's name, found in the loft of the Shakespeare Henley Street house in 1757. Although this document was in standard form, most scholars now accept that it is genuine, and that in all probablity John Shakespeare did die a Catholic.[44] But all the evidence is that this was a late conversion, for during the period that John Shakespeare was a member of the corporation, he actively supported the Protestant reformation of the gild chapel, and nowhere is there any mention of fines, difficulties, or religious persecution on Catholic grounds. The only mention of his "recusancy" is the inclusion of his name in the list of recusants for the year of 1592; but his name was very clearly grouped with eight others who were stated as "not comminge monthlie to the churche . . . It is sayd . . . for feare of process for debtte", and there is good evidence that all nine people on this list were in deep economic difficulty at this time[45], and none of them appear on the recusancy roll for 1593.[46]

Others in the 1592 recusancy survey were explicitly listed as avoiding church because of their Catholic affiliations, but John Shakespeare was not

one of them. He had good reason to fear arrest for debt: in 1589 William Burbage had sued him in the London Court of Common Pleas for the recovery of £7 (£980) awarded him in 1582, and was granted his suit, with 35 shillings damages (£245).[47] The original suit had arisen out of the leasing of a house by John Shakespeare, the £7 apparently being the sum that Burbage had originally paid for the lease (one must assume that John Shakespeare did not fulfill his part of the bargain). The older Shakespeare did not pay this debt or the damages arising out of the 1589 case, and an initial warrant for his arrest was issued after he failed to appear before the Justices of the Common Pleas in April, 1592.[48] As Eccles has concluded: "Shakespeare's father had good reason to fear arrest, for he had never paid the debt of seven pounds and damages which William Burbage had never recovered from him in 1589."[49]

Both Fripp and Mutschmann & Wentersdorf make a great deal of the transfer of land by John Shakespeare to friends and relatives, allegedly to avoid the effects of religious persecution. Both groups of authors argue that Shakespeare's father was cheated out of land in this way, and they particularly had in mind the mortgaging of the land inherited through Mary Arden at Wilmcote in the parish of Aston Cantlow. Shakespeare senior mortagaged this land to his brother-in-law Edmund Lambert (he was married to Mary Arden's sister) in 1578, and this transaction and its aftermath is of such central importance for understanding John Shakespeare's character, that the next chapter will be devoted entirely to a discussion of it.

CHAPTER 4:
THE SHAKESPEARE/LAMBERT DISPUTE

The first surviving legal document relating to the Shakespeare/Lambert land transactions is the foot of fine registered in Easter of 1579. The relevant substance of this document is as follows

> "This is the final agreement made in the court in Westminster ... Between Edmund Lambert complainant and John Shakespeare and his wife Mary defendants concerning two messuages two gardens fifty acres of land two acres of pasture four acres of pasture and common of pasture of all sorts of beasts with appurtenances in Aston Cantlow whence a plea of agreement had been summoned between them in the same Court. That is to say that the aforesaid John and Mary recognized the aforesaid tenement and common pasture with the appurtenances to be the right of . . . Edmund and his heirs in perpetuity . . . and for this recognizance concession quitclaim warrant fine and concord the same Edmund gave the aforesaid John and Mary forty pounds sterling."[1]

On the face of it, this was a straight sale of about 56 acres of land by the Shakespeares to Edmund Lambert for £40 (£5,600). The references to complainant and defendant were standard sixteenth century legal terminology; as Lewis has put it, "one of the legal methods of conveying and alienating land in the sixteenth century . . . was a fictitious suit in which one party sued the other for wrongfully witholding a given property. The defendant admitted the fictitious fact, and then the complainant paid the defendant for giving the premises back to him."[2] No mention was made in this foot of fine to any mortgage arrangement, and there was no reference to the land in question being held by other parties under lease; in fact the land was both mortgaged and let under lease.

In November 1579 a foot of fine was registered on a lease on land in Wilmcote owned by the Shakespeares. There has been much confusion

and disagreement among Shakespeare scholars as to the significance of this lease, and its salient features run as follows:

> "Between Thomas Webbe and Humphrey Hooper, the complainants, and John Shakespeare and Mary his wife and George Gibbs, the defendants, concerning seventy acres of land, six acres of meadow, ten acres of pasture and common of pasture for all manner of animals with the appurtenances, in Wilmcote . . . That is to say: the aforesaid John and Mary and George recognized the aforesaid tenements and common of pasture, with the appurtenances, to be the property of . . . the same Thomas and Humphrey [held] as a gift of the aforesaid John and Mary and George . . . And for this recognition . . . the same Thomas and Humphrey granted to the aforesaid George the aforesaid tenements and common of pasture . . . from the feast of Saint Michael the Archangel [29th September] . . . one thousand five hundred and eighty, even to the end of a term of twenty-one years ...; he to render therefrom annually to the aforesaid Thomas and Humphrey, and to the heirs of Thomas himself, one-half of a quarter of wheat and one half of a quarter of barley to be paid annually on the feast-day of the Nativity of our Lord . . . The above-mentioned Thomas and Humphrey furthermore (had) granted to the above mentioned John and Mary a reversion of the above mentioned tenements and common of pasture, with the appurtenances, and the above-mentioned revenue previously reserved, and they (had) restored these to them in the same Court, to have and to hold by the same John and Mary, and the heirs of Mary herself . . . in perpetuity."[3]

This lease can only properly understood when its purpose from the Shakespeares' point of view is made clear; this will be discussed later, but for the moment we can note: 1. That Thomas Webbe and Humphrey Hooper in effect leased the land from the Shakespeares until the 29th September, 1580 – and this was achieved through fictional devices of complainant/defendant and gift/counter-gift. 2. That after this date in 1580 the land was leased for twenty-one years at a peppercorn rent to George Gibbs, who was almost certainly acting as the Shakeapeares' agent and steward.[4] In 1588 John Shakespeare lodged a bill of complaint against Edmund Lambert and revealed a mortgage which had been arranged in 1578. It is such a significant document for the reading of John Shakespeare's character, that it will be quoted very fully:

> "John Shakespeare complains . . . that . . . Edmund (Lambert) during his lifetime, to wit, on the fourteenth day of November (1578) . . . through a certain indenture bearing the date and the year aforesaid, had bought for himself and his heirs from the aforementioned John Shakespeare and Mary his wife one messuage or tenement, one virgate of land, and four acres of arable land with the appurtenances in Wilmcote in the said county of Warwick, to have and to hold . . . forever; provided always that if the said John Shakespeare, his heirs, executors, administrators or assigns either

paid or caused to be paid to the aforesaid Edmund forty pounds of legal English money on the day of the feast of Saint Michael the Archangel [29th September] . . . one thousand five hundred and eighty, that the then aforesaid indenture and all things therein would be void; by virtue of which the same Edmund entered the aforementioned tenement with its appurtenancies, and from that time had possession of it as master from a fief, and thus remaining in possession from that time on, afterwards, to wit, on the first day of March (1587) . . . he died . . . after whose death the aforesaid messuage and other premises with the appurtenances descended to the aforesaid John Lambert, as son and heir of the said Edmund; and the said John Lambert, doubting his estate and interest of and in the aforesaid tenements with appurtenances to be void, and having knowledge that it was the purpose and intent of the said John Shakespeare to summon him to justice on account of the property, in consideration that the aforesaid John Shakespeare did not at once summon to court the said John Lambert on the account of the aforesaid tenment and the rest of the premises with the appurtenances, and (in consideration) that the said John Shakespeare and his wife Mary together with William Shakespeare their son, when claim had been made upon them, covenanted the said tenements and the other premises with appurtenances to said John Lambert and delivered all writings and proofs concerning the aforesaid premises; the aforesaid John Lambert . . . in consideration thereof took obligation upon himself and then and there faithfully promised that he, the same John Lambert would fully and faithfully pay and make satisfaction (to the amount of) twenty legal pounds in English money [£2,800]... the aforesaid John Lambert, however, caring but for the least for his aforesaid promise and undertaking, but with scheming and fraudulent intent craftily and cunningly to deceive and defraud this John Shakespeare of the aforesaid twenty pounds, has not up to this time paid the same twenty pounds . . . on account of which, the same John Shakespeare totally lost and failed to acquire the whole gain, advantage and profit which he by buying and bargaining with the aforesaid twenty pounds have had and acquired, to the loss of thirty pounds to John Shakespeare. And thereafter he brings a suit of law . . . and the same John Lambert defends the force and injury since etc, and says that he did not assume for himself in the way and form in which the aforesaid John Shakespeare related above against him..."[5]

John Shakespeare fundamentally contradicted himself in this bill of complaint: on the one hand he admitted that there had been an absolute tranfer of the land, subject only to the unredeemed mortgage proviso (the £40 due on the 29th September, 1580), and on the other he claimed that he was selling outstanding "evidences" for £20. Why should John Lambert agree to the payment of an additional £20 for the surrender of "evidences" when he clearly already owned the land outright? As the author of the Public Record Office pamphlet *Shakespeare In The Public*

Records concluded, John Shakespeare's claim "was dubious in the extreme"[6], and there is no evidence that the case ever reached the courts. A full discussion of the significance of this bill of complaint must wait until we have examined all the evidence in the Shakespeare/Lambert dispute. However, as previously noted, it indicates the speculative, capitalist nature of John Shakespeare's attitude and mentality: that by "buying and bargaining" he expected to be able to make a 50% profit on money invested. Additionally, it links Shakespeare himself into the transaction – suggesting that he was still living in Stratford and working with his father at this late period?

Nine years after John Shakespeare had filed the above complaint, he and his wife submitted (in 1597) an entirely different case against John Lambert, relating to the same land transaction:

> "... [In] consideracion of the somme of fowerty poundes to them by one Edmounde Lamberte . . . your sayde oratours [the Shakespeares] were contente that he, the saide Edmounde Lamberte, shoulde have and enjoye the same premisses untill such tyme as your sayde oratours did repaie unto him the saide somme of fowertie poundes; by reasone whereof the saide Edmounde did enter into the premisses and did occupie the same for the space of three or fower yeares, and the issues and the profyttes thereof did recyve and take; after which your saide oratours did tender unto the said Edmounde the sayde somme of fowerty poundes, and desired that they mighte have againe the sayde premisses accordinge to theire agreement; which money he the sayde Edmounde then refused to receyve, sayinge that he woulde not recyve the same, nor suffer your sayde oratours to have the saide premisses agayne, unlesse they woulde paye unto him certayne other money which they did owe unto him for other matters . . . shortelie after the tendringe of the sayde fowertie poundes to the saide Edmounde, and the desyre of your sayde oratours to have theire lande agayne from him, he the saide Edmounde att Barton aforesayde dyed, after whose deathe one John Lamberte, as sonne and heire of the saide Edmounde, entred into the saide premisses and occupied the same; after which entrie of the sayde John your said oratours came to him and tendred the saide money unto him . . . which he, the saide John, denyed in all thinges, and did withstande them for entringe into the premisses, and as yet doeth so contynewe still ..."[7]

This bill of complaint is totally at odds with the earlier story told by John Shakespeare in 1588: in that year, there was no mention of the repayment of the £40 mortgage, but rather a complaint that John Lambert had refused to pay £20 for further evidence of title; in 1597, the reverse was the case – no mention of non-payment of the £20, but a new claim that the £40 mortgage money had been proffered as stipulated under the original agreement. Paradoxically, John Shakespeare had

outlined very clearly in his 1588 complaint the mortgage arrangement on the land: the £40 to be paid by the 29th September 1580, or the land to be forfeited – whereas in 1597 he merely stated that Edmund Lambert should enjoy the "premisses untill such tyme your sayde oratours did repaie unto him the saide somme of fowertie pounds." And he was extremely vague in his later statement as to when he proffered the £40 in repayment of the mortgage: at one point he said he had offered to pay it "three or fower yeares" after the agreement of 1578, i.e. between 1581 and 1582, and then claimed that "shortelie after the tendringe of the sayde fowertie poundes", Edmund Lambert died – and as the latter died in 1587, this would put the date some five or six years later. And offering the £40 to John Lambert in 1587 would have been irrelevant, given that the mortgage deed clearly stipulated, on John Shakespeare's own admission, payment by 1580. John Lambert's reply to the above complaint was as follows:

> "... [The] said complainante, John Shakespeare, by indenture beringe date uppon or about the fowerteenth daye of November, [1578] . . . for and in consideracion of the summe of fortie powndes of lawfull Englishe monney unto the said complainante paide by Edmunde Lamberte, this defendantes father in the said byll named, did geve, graunte, bargaine and sell the said messuage, and one yearde and fower acres of lande with the appurtenances [in Wilmecott, in the parishe of Aston Cawntloe], unto the said Edmunde Lamberte, and his heires and assignes, to have and to holde the said messuage . . . ; in which indenture there is a condicionall proviso conteyned that, if the said complainante did paye unto the saide Edmunde Lamberte the summe of fortie powndes uppon the feastdaie of St. Michell the Archangel . . . one thousande fyve hundred and eightie, att the dwellinge howse of the said Edmund Lamberte . . . that then the said graunte, bargaine and sale . . . shulde cease and be voyde ... and this defendante further sayeth that the said complainante did not tender or paye the said summe of fortie powndes unto the said Edmunde Lamberte, this defendantes father, uppon the saide feaste daye, which was in the yeare of our Lorde God one thowsande fyve hundred and eightie, accordinge to the said provisoe in the said indenture expressed. By reason whereof this defendantes said father was lawfully and absolutely seized of the said premisses . . . and this defendante further sayeth that the said messuage, yearde lande and other premisses, or the moste part thereof, have ever, sythence the purches therof by this defendantes father, byne in lease by the demise of the said complainante; and the lease thereof beinge now somewhat nere expyred, whereby a greater value is to be yearly raised thereby, they, the said complainantes, doe now trowble and moleste this defendante by unjuste sutes in lawe, thinkinge therby, as yt shoulde seme,

> to wringe from him this defendante some further recompence for the said premisses then they have alreddy received ..."[8]

This statement of John Lambert's confirms the details of the mortgage arrangement outlined by John Shakespeare in 1588: that it was to run for two years until the 29th September 1580, whereupon if not redeemed the land would be transferred absolutely to Edmund Lambert. There is no mention of the £20 for surrender of evidences of title, and this had become an irrelevance inasmuch as the Shakespeares had dropped this contention from their claim in 1597. John Lambert's defence was very straightforward – he stood by the two-year mortgage arrangement earlier conceded by John Shakespeare – and he categorically denied that any payment had been offered to redeem this mortgage. The additional interest of his defence lies in the reference to the lease; although he does not give details of this, he does tell us that it was due to expire soon after 1597, and that there would be a significant increase in its value on renewal. This is almost certainly the lease on the Shakespeare Wilmcote land registered in 1579: it was let out on a peppercorn rent and was due to run out in 1601, but we will return to this lease when presenting a summary analysis of the whole series of transactions centring on the Wilmcote property. In reply to Lambert's defence, John Shakespeare yet again modified his story:

> "... accordinge to the condicion or proviso mencioned in the said indenture of bargaine and sale of the premisses mencioned in the said bill of complaynt, he this complaynant, John Shakespere, did come to the dwellinge-house of the said Edmunde Lambert, in Barton-uppon-the-Heathe, uppon the feaste daie of St.Michaell the archangel . . . one thousand fyve hundred and eightie, and then and there tendered to paie unto him the said Edmunde Lambert the said fortie poundes, which he was to paie for the redempcion of the said premisses; which somme the saide Edmunde did refuse to receyve, sayinge that he owed him other money, and unles that he, the said John, would paie him altogether, as well as the said fortie pounds as the other money, which he owed him over and above, he would not receave the said fortie poundes, and imediatlie after he, the said Edmunde, dyed, and by reason thereof, he, the said defendant, entered into the saide premisses, and wrongfullie kepeth and detayneth the said premisses from him the said complaynant ..."[9]

Again, there is a basic contradiction in this account: on the one hand, John Shakespeare claimed to have proffered the £40 in 1580; on the other, he stated that Edmund Lambert died "immediatlie after" – and as this happened in 1587, there is a discrepancy of something like seven years in the chronology. It is therefore not surprising in the light of all

these contradictions and inconsistencies that the case went against John Shakespeare. Although we do not have a formal record of the verdict, it is now known that the land continued in the ownership of the Lambert family; in early 1602, "John Lambert and his Margery his wife sold to Richard Smyth, for forty pounds, forty-six acres of land in Greate Wilmcote, together with two acres of meadow, three acres of pasture, and common of pasture."[10] The case had come full circle: John Shakespeare's lease had come to an end in 1601 – the year of his death – and the Wilmcote land was sold for the same sum for which it was bought – £40. (The Lamberts would have made a small profit on the transaction, as they appear to have retained five of the 56 acres that they acquired in 1578.)

Before analysing the significance of this case, we must take note of two small but interesting additional pieces of evidence on the Shakespeare/Lambert relationship. When Roger Sadler, a baker of Stratford, made his will on the 14th November 1578, he appended to it a list of debts owing to him; one of these was a debt of £5 (£700) owing by "Edmonde Lamberte" and "Edward Cornishe" (another brother-in-law of John Shakespeare), "for the debte of Mr John Shaksper".[11] Lambert was helping his brother-in-law Shakespeare with a loan at the time of the latter's economic difficulties in 1578, and two years later in May, 1580, John Shakespeare's youngest son, Edmund, was christened – and as Halliday has written, he "was probably named after his uncle, Edmund Lambert".[12] All this suggests that the Shakespeares and Lamberts were on very friendly terms until the dispute broke out between them about the Wilmcote property.

We are now in a position to consider the significance of some of the property transactions earlier discussed, in particular the leasing of the Wilmcote land registered in 1579. Given John Shakespeare's economic difficulties in 1578 and his friendly relationship with his brother-in-law, Edmund Lambert, at that time, the most logical explanation of the leasing arrangement is as follows. The lease was divided into two time zones: 1.The period up to the 29th September 1580, when it was leased to Thomas Webbe and Humphrey Hooper. 2.The twenty-one year period from 1580 until 1601 when it was held at a peppercorn rent by George Gibbs as agent for the Shakespeares – an arrangement confirmed by John Lambert's reference to the land being leased "by the demise" of John Shakespeare. This division can only be understood in relation to the mortgage agreement: up until the 29th September 1580, the land was under mortgage to the Lamberts and beyond the control of the

Shakespeares. The latter presumably let the land under lease to Webbe and Hooper either at a full market rent, or had some agreement whereby they could benefit from the income of the farm. The mortgaging and leasing arrangements were structured so that if the Shakespeares were able to redeem the mortgage in 1580, there would be no loss to them, and incidentally no gain to the Lamberts, except for the receipt of the rent from the lease for the period 1578-1580. If they were unable to redeem the mortgage, then the leasing provisions would protect them during the twenty-one year ensuing years: they would have the full income from the farm via their peppercorn/agency arrangement with George Gibbs, in effect a safeguard device as a contingency against not being able to redeem the 1578 mortgage.(It should be noted that only a part of the leased land was mortgaged to the Lamberts – what happened to the other part is not known.)

Whether or not the mortgage was redeemed in 1580, the Lamberts stood to gain very little. They had no access to the income from the land at any point between 1580 and 1601; if the mortgage had been redeemed in 1580, they would have gained nothing – except for the receipt of the rent on the leased land for the two years 1578-1580. In the event, they re-sold the land for the same price as they paid for it twenty-one years after they had acquired title in 1580, although with a very small profit by retaining five acres. All the evidence points to the original mortgage arrangement of 1578 being an act of generosity on the part of Edmund Lambert, designed to help his brother-in-law at a time of difficulty, and this is confirmed by the loan he made (with his fellow brother-in-law, Edward Cornwell/Cornishe) via Roger Sadler in the same year.

What does all this tell us about the conduct and character of John Shakespeare? We are forced to conclude from the evidence that he consistently lied in court about the facts of the case, and was ruthlessly prepared to exploit any weaknesses or possibilities for economic gain. To use his own words, he shewed "scheming and fraudulent intent craftily and cunningly to deceive and defraud" – but the language should be applied to his own conduct, not that of his Lambert relatives. It is presumably because of this that Mary Shakespeare's name was dropped at one stage from the case launched against John Lambert in 1597 [13] – her sister had been married to Edmund Lambert, and this must have put great strains on family loyalties. We can perhaps see the characters of John Shakespeare and John Lambert respectively in the language that they use in ending their submissions: John Shakespeare – "and your

sayde oratours shall daylie praye to God for the prosperous healthe of your good lordshippe with increase of honour longe to contynewe"; John Lambert – "All which matters this defendante is reddy to averre and prove, as this honorable courte shall awarde, and prayethe to be dismissed therhence with his reasonable costs and charges in this wrongfull sute by him unjustly sustyeyned."[14] One was the language of flattery and deceit; the other of simplicity and straightforwardness.

John Shakespeare's tactics in dealing with the case can perhaps best be explained by his economic circumstances. In 1588 when he lodged his first bill of complaint, he was near to a point of bankruptcy; Edmund Lambert had just died, and presumably given the complexity of the legal transactions associated with the land, he perhaps felt that he could exploit any uncertainties of title to extract more money from the new heir to the Lambert estate. By 1597 he had returned to prosperity: when he applied for a new coat of arms in 1596, he was said to be worth £500 (this was almost certainly, as we shall see, the result of his son's financial success), and he could now afford to invoke the original mortgage agreement and offer the £40 redemption payment. As John Lambert pointed out, he had a particular incentive to do this as the favourable lease on the land was soon to fall in.

With some confidence, we can now rule out the arguments of Fripp, and Mutschmann & Wentersdorf, about Shakespeare's father suffering economically as a result of religious recusancy; not only is there no direct evidence for this, but the contention put forward by these writers that John Shakespeare had been cheated out of money and property transferred for safekeeping to relatives, such as the Lamberts, collapses completely. We must therefore look elsewhere for the explanation of John Shakespeare's fall.

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The problems that beset John Shakespeare in the period after 1577 were not confined to economic difficulties and disputes over land ownership. He became embroiled in a number of quarrels and legal entanglements, which have been very succinctly summarized by Schoenbaum :

> "Troubles multiplied. In Trinity term, 1580, John was fined £20 [£2,800] for not appearing in the court of Queen's Bench to find security for keeping the Queen's peace. (At this time the Crown similarly penalized many others,

> more than 140 throughout England, with fines of £10 to £200; why is not known.) The same court also fined him another £20 as pledge for a hatmaker of Nottingham [John Audley] who had failed his day to produce surety for good behaviour . . . Evidently John Shakespeare did not always judge well the character of those for whom he vouched. Thus the £10 [£1,400] bail he stood for Michael Price, [a] felonious Stratford tinker, was forfeited. So too was his bond for £10 of a debt of £22 [£3,080] incurred by his brother Henry. On that occasion a suit followed; to escape jail John turned to his friend Alderman Hill for bail, and even swore out a writ of habeas corpus to transfer the case to another court ... Adversaries, as well as adversities, oppressed him. In the summer of 1582 he petitioned for sureties of peace against four men – Ralph Cawdrey, William Russell, Thomas Logginge, and Robert Young – 'for fear of death and mutilation of his limbs'."[15]

Little is known of these friends and associates of John Shakespeare's; John Audley had been fined £10 as a pledge for Shakespeare senior, as had the third member of this circle, Thomas Cooley of Stoke, Staffordshire[16] – and whatever the nature of the original crime, it was clearly very serious involving a breach of the Queen's peace, and incurring a very heavy fine. Michael Price, the tinker, was a near neighbour of John Shakespeare's, but not a great deal is known about him, as is the case with Ralph Cawdrey, William Russell, Thomas Logginge, and Robert Young. More is known about Henry Shakepeare, John Shakespeare's brother, and again we can do no better than quote from Schoenbaum for a summary:

> "Henry Shakespeare . . . held land in the Hales manor [Snitterfield], but he also farmed at Ingon in the nearby parish of Hampton Lucy . . . Henry was something of a ne'er-do- well. He got into a fray with one Edward Cornwell (he became the second husband of the poet's Aunt Margaret), drew blood, and was fined, but did not show up in court to answer the charge. He was imprisoned for trespass; he incurred debts and failed to honour them. On one occasion Henry refused to pay his tithe – there was a quarrel between claimants – and for a time suffered excommunication. The authorities fined him for wearing a hat instead of a cap to church: maybe a principle was involved, for many, especially Puritans, resented the Statute of Caps, promulgated to encourage the depressed craft of cappers."[17]

It is a somewhat misleading to call Henry Shakespeare a "ne'er-do-well"; John Shakespeare's chequered history after 1577 reads in similar vein to that of his brother, and generally the Stratford records for the period are littered with references to drunken affrays, fights and personal disputes.[18] Elizabethan society varied so much from our own that we must be wary of applying our own standards of reference, to what in effect

was a very different culture. References to dunghills, unmade and pitted roads covered with offal and the remains of slaughtered animals, chickens, pigs and cattle roaming unchecked in the streets, drunken brawls between men wearing knives, daggers and swords – it is easy for the reader of the Stratford documents to believe he has inadvertently stumbled into the "Wild West" – a raw, physical, and violent world.[19] The Stratford Corporation did of course try to control and regulate such excesses, and the records bear out the persistent struggle to subdue these breaches of the puritan code, destined inevitably to fail. The tension and contrast between the puritan style of the corporation and the way of life of the ordinary inhabitants of Stratford was very marked, but this is to anticipate a later argument.

CHAPTER 5:
JOHN SHAKESPEARE AS FALSTAFF

A point has now been reached when we must look to William Shakespeare for help in unravelling the enigma of his father. The first clue lies in the list of people not attending church in 1592:

Mr John Wheeler
John Wheeler, his son
Mr John Shakespeare
Mr Nicholas Barnhurst
Thomas James, alias Giles
William Baynton
Richard Harrington
William Fluellen
George Bardolfe

"IT is said that these last nine come not to church for fear of process for debt."[1]

The last two names are familiar – Bardolfe a character out of *Henry IV*, *Henry V*, and *The Merry Wives Of Windsor* – and Fluellen a soldier in *Henry V*. In a supplementary list of recusants for 1592, the name of Court appears – he was also a character out of *Henry V*.[2] It has been generally recognized by Shakespeare historians that these are the most autobiographical of Shakespeare's plays. This is particularly the case with *The Merry Wives Of Windsor*, where the names of Herne, Horne, Brome, Ford, Page, Poins, Peto, Bardolfe and Fenton appear – and these were all people resident in Stratford during Shakespeare's youth.[3] Given that the town only had a population of approximately 1,750 people (about 390 families), this is a most remarkable concentration of local names in one play, suggesting powerful autobiographical associations for Shakespeare. [It is also perhaps no co-incidence that the name of the street at the end of Henley Street is Windsor Street.] Occasionally a single local name appeared in one of his other plays with, as we have seen, the exception of the *Henry IV* and *Henry V* series where there were the four references to Bardolfe, Fluellen, Court and Poins.

Little or nothing is known of the individuals who carried the names of the characters in the plays. The person we know most about is George Bardolfe; he was more commonly known in Stratford as Bardell, and that Shakespeare had him in mind as a model is indicated by the names given to him in the different editions of the plays: Bardoll, Bardolfe and Bardolph. By trade Bardolfe/Bardell was a mercer and a grocer and had been nominated as a burgess of Stratford corporation in 1580 (he had previously been a constable). He was elected chamberlain in 1582 but was excused from serving for reasons unstated but accepted by the corporation. He was elected alderman in 1584 but does not appear to have taken up his position, as he continued to be listed as a burgess. He acted as one of the chamberlains in 1586, but from 1588 ceased attending corporation meetings and was struck off the list of burgesses (for non-attendance) on the 14th August 1590.

Writs of arrest were issued against Bardell for debt in 1588 and 1592, and he was imprisoned in the latter year; according to Eccles "the undersheriff of Warwickshire, Basil Trymnell, at first let Bardell drink in a tavern in Warwick but when warned that he might escape kept him 'in a more Strayter manner' and secured him by 'a Locke with a Longe yron Chayne and a greate clogge."[4] Bardell's debts were apparently partly due to the indebtedness of his partner Charles Baynton, but his withdrawal from corporation meetings may have been of his own making. What we know of Bardell/Bardolfe is consistent with Shakespeare's use of him as a model for the comical character Bardoll/Bardolfe/Bardolph. Practically nothing is known about Fluellen, the other person listed with John Shakespeare and George Bardolfe in 1592. His widow, like George Bardell's, was admitted in 1604 to the Stratford almhouse on grounds of poverty.[5]

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All the relevant aspects of the biographical background have now been covered, and we are in a position to state the central thesis of this section of the book. The major clue to unravelling John Shakeapeare's enigma lies in the only contemporary description of him. As we have seen, in about 1657, Thomas Plume, wrote about Shakespeare : "He was a glovers son – Sir John Mennis saw once his old Father in his shop – a merry Cheekd old man – that said – Will was a good Honest Fellow, but he durst have crackt a jeast with him at any time."[6]

Where do we find such a figure, "merry-cheeked", jesting, a man of good humour? A key is found partly in John Shakespeare's family name – his father, Richard Shakespeare, is listed in one class of records as having the surname of Shakstaff [7] – giving Shakespeare's father the name of *John Shakstaff*. The other clues have already been laid: the appearance on a list with Bardolfe and Fluellen; the impoverished man, acting "with scheming and fraudulent intent, craftily and cunningly to deceive and defraud"; the Protestant who had fallen out of grace; but above all the man who dominates Shakespeare's autobiographical plays, *The Merry Wives Of Windsor*, *1, Henry IV*, and *2, HenryIV:* Such a man is **John Falstaff.**

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The poet, W.H.Auden, saw perhaps more clearly than anyone else the paradox of Falstaff's position in the *Henry IV* plays; he wrote:

> "What sort of bad company would one expect to find Prince Hal keeping when the curtain rises on *Henry IV?* Surely, one could expect to see him surrounded by daring, rather sinister juvenile delinquents and beautiful gold-digging whores. But whom do we meet in the Boar's Head? A fat, cowardly tosspot, old enough to be his father, two down-at-heel hangers-on, a slatterny hostess and only one whore, who is not in her earliest youth either; all of them seedy, and, by any worldly standards, including those of the criminal classes, all of them *failures*. Surely, one thinks, an Heir Apparent, sowing his wild oats, could have picked himself a more exciting crew than that. As the play proceeds, our surprise is replaced by another kind of puzzle, for the better we come to know Falstaff, the clearer it becomes that the world of historical reality which a Chronicle Play claims to imitate is not a world which he can inhabit."[8]

There is virtually no literary or historical basis for the character of Falstaff, although the character may have been partly based on the Lollard, Sir John Oldcastle, who appeared briefly (he had a few lines) in the source play, *The Famous Victories Of Henry V*. Shakespeare made a play on Oldcastle's name in the epilogue of *2, Henry IV*, and there were a number of survivals of the name in the text. According to contemporary sources, Shakespeare changed the name of Oldcastle to Falstaff as a result of political pressure from Oldcastle's influential descendants. Shakespeare also probably changed the name because of the existence of a Protestant contemporary of Oldcastle, Sir John Falstolfe – and he wanted a figure with a strong puritan/protestant background for the creation of the Falstaff character.

Although cast in comical form the *Henry IV* plays bear out Falstaff's Protestant history; in *1, Henry IV* (Act 1, Scene 2) Falstaff says to Hal: "O, thou hast damanable iteration, and art indeede able to corrupt a Saint . . . Before I knew thee *Hal*, I knew nothing: and now I am (if a man shold speake truly) little better then one of the wicked. I must giue ouer this life, and I will giue it over." To which Hal jokingly replies: "I see a good amendment of life in thee: From Praying, to Purse taking." Likewise, in 2, iv, Hal says: "Now my Masters, for a true Face, and good Conscience" – to which, Falstaff rejoins: "Both of which I haue had, but their date is out..." In 3, iii, Falstaff confesses: "Do'st thou heare *Hal?* Thou know'st in the state of Innocency, *Adam* fell: and what should poor *Jacke Falstaffe* do, in the dayes of Villany? . . . Well, Ile repent, and that suddenly, while I am in some liking: I shall be out of heart shortly, and then I shall haue no strength to repent. And I haue not forgotten what the in-side of a Church is made of."

That this plea for repentence is not just a comical device, is indicated in the other Falstaff plays. In *2, Henry IV* (2, ii) Falstaff tells Pistol: "I, I , I myself sometimes, leaving the fear of God on the left hand and hiding my honour in my necessity, am fain to shuffle, to hedge, and to lurch ..." And in *The Merry Wives Of Windsor* (4, v) he confesses: "I neuer prosper'd, since I foreswore my selfe at *Primero:* well, if my winde were but long enough; I would repent." His reputation is that of an honourable man: Page tells Ford that "the Priest o' th' Towne commended him for a true man", to which Ford rejoins, "T'was a good sensible fellow". (2, i). This is echoed by Mrs Ford, who confirms Falstaff's respectable past: "hee would not sweare: praise womens modesty: and gaue such orderly and wel-behaued reproofe to all vncomelinesse, that I would haue sworne his disposition would haue gone to the truth of his words." (1, iv) His bed is "painted about with the story of the Prodigall, fresh and new" (4, v), and in *Henry V* (2, ii), as he dies, he cries "out, God, God, God".

Falstaff is a failure by worldly standards: in *The Merry Wives of Windsor* (1, iii), he confesses that he is "almost out at heeles", and is described "as poore as Job" (5, v). At the end of the play, Ford tells us that "twenty pounds of money . . . must be paid [by Falstaff] to Mr *Broome* [for which] his horses are arrested . . . [and] to repay that money will be a biting affliction." (5, v) In *2, Henry IV* (1, ii) he is again "as poore as *Job* . . . but not so Patient": "I can get no remedy against this Consumption of the purse. Borrowing onely lingers, and lingers it out, but the disease is incureable." After picking Falstaff's pocket – and

finding there a list of "one halfe penny-worth of Bread to this intollerable deale of Sacke" – Hal asks Falstaff what he has lost. Falstaff replies: "Whilst thou beliue me, *Hal?* Three or four Bonds of fortie pound apeece, and a Seale-Ring of my Grandfathers." (*1, Henry IV*, 3, iii). The point of the rejoinder is of course to bring out the comic discrepancy between the reality of Falstaff's dissolute poverty, and his claims to financial standing – but the possession of "three or four Bonds of fortie pound apiece" is more appropriate to John Shakespeare's occupation as an individual trader, than to Falstaff's world of broken-down knight.

Falstaff looks to Prince Hal for financial rescue: "[He] sayde this other day, You [the Prince] ought him a thousand pound." (*1, Henry IV*, 3, iii). Falstaff is preoccupied with obtaining this sum of £1,000: when challenged about his cowardice, he tells the prince that "I would giue a thousand pound I could run as fast as thou canst . . . here be foure of vs, haue ta'ne a thousand pound this Morning" (*1, Henry IV*, 2, iv). He asks the Lord Chief Justice: "Will your Lordship lend mee a thousand pound, to furnish me forth" (*2, Henry IV*, 1, ii), and eventually persuades the gullible Shallow to lend him the thousand pounds in anticipation of the fruits of Prince Hal's succession to the throne (*2, Henry IV*, 5, v). Although Falstaff is disappointed in Hal's reaction to him, the new king does promise him that "competence of life, I will allow you,/ That lack of meanes enforce you not to euill:/ And as we heare you do reforme your selues,/ We will according to your strength, and qualities,/ Give you aduancement." (*2, Henry IV*, 5, v).

Falstaff's lapsed Protestantism and acute poverty are exact parallels to John Shakespeare's condition and situation in the mid-1590's, when Shakespeare began writing the *Henry IV* plays. The parallel goes further: Rowe in his biography states that "my Lord Southampton, at one time, gave him [Shakespeare] a thousand Pounds, to enable him to go through with a Purchase he had a mind to"[9]; and R.B. Wheeler in his *History And Antiquities Of Stratford-Upon-Avon*, published in 1806, tells us that "the unanimous tradition of this neighbourhood is that by the uncommon bounty of the Earl of Southampton, he [Shakespeare] was enabled to purchase houses and land in Stratford."[10] Shakespeare bought New Place in 4 May 1597, but more importantly, his father's fortunes had been restored sufficiently by 1596 for him to claim when applying for a coat of arms, "that he hath Landes and tenementes of good wealth, & substance £500."[11]

Falstaff shows some sensitivity to his status as a gentleman: at one point in *2, Henry IV*, he exclaims, "As I am a Gentleman", and is rebuked by Mistress Quickly – "Nay, you said so before" – to which Falstaff rejoins again, "As I am a Gentleman."[12] John Shakespeare likewise was concerned to establish his claims to gentility: when applying for his coat of arms, he stated that he "hath maryed the daughter (& one of the heyres of Robert Arden of Wilmcoote in the said) Counte esquire", and that his "parent great Grandfather and late Antecessor, for his faithefull & approved service to the late most prudent prince king H 7 of famous memorie, was advuanced & rewarded with Landes & Tenementes geven to him in those partes of Warwikeshere where they have continewed bie some decentes in good reputacon & credit."[13] Chambers has questioned the validity of these claims, and concluded that "the landed status of the early Warwickshire Shakespeares, so far as discoverable, was that of manorial copyholders, or at the very most freeholders or leaseholders"; and that Robert Arden had never been styled esquire or gentleman – in fact, he was almost certainly a mere "husbandman".[14] Although exaggerated claims were common in the applications for coats of arms, the degree and extent of John Shakespeare's exaggerations are entirely compatible with his identification with Falstaff.

There are echoes of Falstaff in the character of the shepherd in *The Winter's Tale*. The shepherd and his son, the clown, are to be rewarded for having inadvertently adopted Perdita, daughter of Leontes, King of Sicily – and as a result, are anticipating gentleman status:

> "*Shepherd* Come Boy, I am past moe Children: but thy Sonnes and Daughters will be all Gentlemen borne.
> *Clown* You are well met (Sir:) you deny'd to fight with mee this other day, because I was no Gentleman borne . . . now I am a Gentleman? Let Boores and Franklins say it, Ile sweare it.
> *Shepherd* How if it be false (Sonne?)
> *Clown* If it be ne'er so false, a true Gentleman may sweare it, in the behalfe of his Friend. And Ile sweare to the Prince, thou art a tall Fellow of thy hands, and that thou wilt not be drunke: but I know that thou art no tall Fellow of thy hands and that thou wilt be drunke." (5, ii)

At about the time that Shakespeare was writing the Falstaff plays, his friend and rival, Ben Jonson, wrote what many consider satirical references to the Shakespeare application for a coat of arms. Schoenbaum has summarized the episode as follows: "In his *Poetaster* Jonson sneers at common players who aspire to heraldic distinctions . . . and some have seen mockery of Shakespeare's motto in *Every Man Out Of*

His Humour, in which the rustic clown Sogliardo, fresh from having laid out £30 for his arms, is mocked with the words, 'Not without mustard'.[The Shakespeares' motto was, 'Not without right'.] The parallel is suggestive, but Sogliardo is not an actor, and his coat-armour, which flaunts a boar's head, is unlike Shakespeare's."[15] *But the coat of arms was not applied for by William but by John Shakespeare – and although Sogliardo is not an actor, he is a rustic clown – and his coat armour is a "boar's head"*. What more effective way of satirizing John Shakespeare as Falstaff could Jonson have devised? – and Jonson is likely to have known the relevant background, as he was a friend of Shakespeare's, and according to one contemporary source, had visited Shakespeare in Stratford.[16] Also, Shakespeare probably satirized Jonson in the character of Nym – whose frequent exclamations of "by my humour" refer to Jonson's own satirical plays – suggesting a mild satirical battle between the two playwrights. However, it should be pointed out that Jonson's satire was for a public audience, and that any private allusions would be secondary; "Not Without Mustard", had originated with Thomas Nashe in his *Pierce Penilesse*[17], and presumably Jonson was, at least in part, making satirical reference to this.

There are references in *1, Henry IV* which can be read directly as linking John Shakespeare to Falstaff. In Act 2, Scene 4, Falstaff says "When I was about thy yeeres (*Hal*) I was not an Eagles Talent in the Waste, I could haue crept into any Aldermans Thumbe-Ring" – and as John Shakespeare was in his thirties when he was appointed an Alderman, and Shakespeare was the same age when he wrote this play, the potential association is clear.

Falstaff's relationship to Hal is that of father to son: "Thou art my Sonne: I haue partly thy Mothers Word, partly my Opinion; but chiefly, a villanous tricke of thine Eye, and a foolish hanging of thy nether Lippe, that doth warrant me. If then thou be Sonne to mee, heere lyeth the point: why, being Sonne to me, art thou so poynted at? Shall the blessed Sonne of Heauan proue a Micher, and eate Black-berryes?" (*1, Henry IV*, 2, iv) Elsewhere, Hal refers to Falstaff as "that Father Ruffian", whose "Lyes are like the Father that begets them."[18] Falstaff in turn boasts of his influence over his adopted son: "Hereof comes it, that Prince *Harry* is valiant: for the cold blood hee did naturally inherite of his Father, hee hath, like leane, stirrill, and bare Land, manured, husbanded, and tyll'd, with excellent endeauour of drinking good, and good store of fertile Sherris, that he is become very hot, and valiant. If I had a

thousand Sonnes, the first Principle I would teach them, should be to foreswore thinne Potations, and to addict themselues to Sack."[19] And Shakespeare brings this relationship into even more explicit focus in the fourth scene of Act 2 of *1, Henry IV*:

> "*Prince* Doe thou stand for my Father, and examine mee vpon the particulars of my Life.
> *Falstaff* Shall I? content: This Chayre shall bee my State, this Dagger my Scepter, and this Cushion my Crowne.
> *Prince* Thy State is taken for a Joyn'd-Stoole, thy Golden Scepter for a Leaden Dagger, and thy precious rich Crowne, for a pittifull bald Crowne."

The whole tone of the relationship between Falstaff and Hal is similar to that of John Shakespeare with his son: "Will was a good honest fellow, but he durst have crackt a jeast with him at any time." There are echoes of this relationship also in other plays: in *The Comedy Of Errors* (1, ii), Antipholus describes Dromio as "A trustie villiane sir, that very oft,/ When I am dull with care and melancholly,/ Lightens my humour with his merry jests." And likewise, in *The Merchant Of Venice* (1, i), Gratiano disclaims: "Let me play the foole,/ With mirth and laughter let old wrinkles come/ And let my Liuer rather heat with wine." Sonnets 45 and 50 suggest that Shakespeare was often in need of such merriment, particularly after a long journey: "How heavy do I journey on the way . . . The beast that bears me, tired with my woe . . . oppress'd with melancholy."

Although Hal is the hero of the *Henry IV* and *Henry V* plays, he takes on a number of 'low' disguises which have no bearing on the main narrative plot of the plays. At the end of the second scene, act two of *2, Henry IV*, in order to spy on Falstaff, Poins suggests that he and Hal "Put on two Leather Jerkins, and Aprons, and waite vpon him at his Table, like Drawers" – to which Hal rejoins: "From Prince, to a Prentice, a low transformation, that shall be mine" – the apprentice-like relationship between Hal and Falstaff mirroring the actual relationship between John Shakespeare and his son. A similar but more revealing 'low' transformation occurs in *Henry V:* Hal, now King Henry V, meets a soldier Williams (companion to Court, whose name appeared in the 1592 recusancy list), and sets in train a series of minor incidents totally irrelevant to the main plot of the play.

The king is acting *incognito* on the battlefield, and enters into a mock-quarrel with Williams; they agree to exchange gloves, to be worn in their

caps, as gages of recognition for a future encounter. The king subsequently persuades Fluellen to wear Williams's glove, tricking him into believing that it is the emblem of one of their French enemies, who he instructs Fluellen to capture. When apprehended, Williams protests to the king: "Your Majestie came not like your selfe: you appear'd to me but as a common man; witnesse the Night, your Garments, your Lowlinesse: and what your Highnesse suffer'd vnder that shape."[20]

This incident then becomes shuffled into a symbolic association with the wearing of leeks by the Welsh soldiers present in the battle, one of whom is Fluellen. The latter reminds the king of this tradition: "If your Majesties is remembred of it, the Welchmen did good seruice in a Garden where Leekes did grow, wearing Leekes in their *Monmouth* caps, which your Maiesty know to this houre is an honourable badge of the seruice: And I do beleeue your Maiesty takes no scorne to weare the Leeke vppon S.Tauies day" – to which the king replies, "I weare it for a memorable honor: For I am Welch you know good Countriman."[21] And almost immediately afterwards, the king encounters Williams wearing his glove in his cap, and sets in train his capture by Fluellen. Earlier in the same scene, Fluellen draws a parallel between Alexander the Great and Henry V – and semi-humorously puts down Alexander – calling him Alexander the pig – in order elevate his king, Harry of Monmouth. He also reminds his audience that "as *Alexander* kild his friend *Clytus*, being in his Ales and his Cuppes; so also *Harry Monmouth* being in his right wittes, and his good judgements, turn'd away the fat Knight [Sir John Falstaffe]." Shakespeare develops this theme by engineering a quarrel between Fluellen and Pistol (who is a surrogate character for Falstaff) – resulting in Pistol being forced to eat the leek that Fluellen wears in his cap.[22]

This complicated and somewhat contorted series of events – humorously inappropriate for the context – is wholly characteristic of Shakespeare when he is exploring themes not directly relevant to the narrative structure of the play, but which have personal significance. There is a 'low' transformation in this series of events, equivalent to the "prince to prentice" incident, but with the additional significance that the 'low' emblem that Hal identifies himself with is – *a glove*. And through the symbolism of the leek/glove, the apparent nobility of the Welsh Tudor dynasty is reduced to the biographical world of Falstaff and the glover, John Shakespeare. Not only is Pistol humiliated through the eating of Fluellen's leek, but Falstaff is symbolically killed off by Alexander the pig/Harry of Monmouth.

The killing off of Falstaff is associated with the hunting of deer, described in *The Merry Wives Of Windsor*, and referred to indirectly in the other autobiographical plays. Shakespeare was caught and punished for poaching Sir Thomas Lucy's deer – this is a theme which will be dealt with at length later – and there is evidence that Shakespeare satirized Lucy in the form of Justice Shallow. In *The Merry Wives Of Windsor*, Shallow accuses Falstaff: "Knight, you haue beaten my men, kill'd my deere, and broke open my Lodge" (Act 1, Scene 1, Folio Edition – in the Quarto Edition this becomes, "Sir John, Sir John, you have hurt my keeper, kild my dogs, stolne my deere.") Master Page, father of young William, entertains Falstaff to venison pasty as the play opens – and inadvertently exacerbates Shallow's injuries by thanking him for a present of the venison, which has actually been poached by Falstaff.

In Shakespeare's mind, there was an indelible link between Falstaff and deer: in *The Merry Wives Of Windsor* Falstaff is pursued and hunted – he refers to himself as "a Windsor Stagge", is called "my Deere", "My male-Deere" by Mrs Ford, and confesses of himself that "when night dogs run, all sorts of deer are chased" – and in the final scene enters disguised as Herne "with a buck's head upon him". In *2, Henry IV*, when Prince Hal discovers Falstaff apparently dead, he exclaims: "Deathe hath not strucke so fat a Deere today" (5, iv), and in the epilogue to the play, Shakespeare promises his audience that "if you be not too much cloid with Fat Meate, our humble Author will continue the Story (with Sir John in it)." Falstaff's attempt to cuckold Ford and Page redounds against him, and he is justly punished by being made to wear horns. Shakespeare interweaves the theme of the hunting of Falstaff with his rejection by Prince Hal. In the Quarto version of *The Merry Wives Of Windsor*, as Falstaff begins to discover how he has been tricked into dressing as a buck-deer, he is made to say: "What hunting at this time of night? Ile lay my life the mad Prince of Wales/ Is stealing his fathers Deare." We see here a direct link between Shakespeare's own poaching activities and those of Prince Hal, but in the context of his rejection of wildness, embodied in the figure of Falstaff, who is hunted and eventually killed off.

Hal's wildness as a young man is described in the appropriate historical plays. In *Richard II* (5, iii), Bolingbroke complains of his son's behaviour: "Can no man tell me of my vnthriftie Sonne?/ 'Tis full three monthes since I did see him last . . . Enquire at London, 'mongst the Tauernes there:/ For there (they say), he dayly doth frequent,/ With vnrestrained

loose Companions . . . " In *2, Henry IV* (4, v), Bolingbroke (now king), reiterates this complaint: "For the Fift *Harry*, from curb'd License pluckes/ The muzzle of Restraint; and the wilde Dogge/ Shall flesh his tooth in euery innocent." He cross-examines Prince Humphrey and Prince Clarence, Hal's brothers, on the latter's whereabouts:

> "*King* . . . where is the Prince, your Brother?
> *Prince Humphrey* I thinke hee's gone to hunt (my Lord) at Windsor...
> *King* And how accompanyed? Canst thou tell that?
> *Prince Clarence* With *Pointz,* and other his continuall followers.
> *King* . . . when his head-strong Riot hath no Curbe,/ When Rage and hot-Blood are his Counsailors,/ When Meanes and lauish Manners meete together;/ Oh, with what Wings shall his Affections flye/ Towards fronting Perill, and oppos'd Decay?" (4, iv)

This speech foreshadows Hal's abandonment of his "wild youth", and Warwick predicts that the "Prince will, in the perfectnesse of time,/ Cast off his followers: and their memorie/ Shall as a Patterne, or a Measure, liue . . . Turning past euils to advantages." (4, iv) Hal's transformation is described in *Henry V* (1, i):

> "... his addiction was to Courses vaine,/ His Companies vnletter'd, rude, and shallow,/ His Houres fill'd vp with Ryots, Banquets, Sports;/ And neuer noted in him any studie,/ Any retyrement, any sequestration/ From open Haunts and Popularitie . . . The breath no sooner left his Fathers body/ But that his wildnesse, mortify'd in him,/ Seem'd to dye too: yea, at that very moment,/ Consideration like an Angel came/ And whipt th'offending *Adam* out of him . . . Neuer was such a sodaine Scholler made:/ Neuer came Reformation in a Flood ..."

This is associated with Hal's rejection of Falstaff, the misleader of youth, a figure that represents all that is dissolute, disorderly and wild. But he is more than an abstract "lord of misrule" – his relationship with Hal is highly personal, and although essentially a comic character, at the end he becomes something of a tragic figure, albeit in absurd form. His rejection is foreshadowed in act two, scene four of *1, Henry IV*, where Falstaff playfully says, in the third person, to Hal: "banish not him thy *Harryes* companie, banish not him thy *Harryes* companie; banish plumpe *Jacke*, and banish all the World." The actual rejection occurs at the end of *2, Henry IV:*

> "*Falstaff* But to stand stained with Trauaile, and sweating with desire to see him, thinking of nothing else, putting all affayres in obliuion, as if there were nothing els to bee done, but to see him ...
> *Enter King Henrie the Fifth, Brothers, Lord Chief Justice.*

> *Falstaff* Save thy Grace, King *Hall*, my Royall *Hall* ...
> *King* My Lord Chiefe Justice, speake to that vaine man.
> *Chief Justice* Haue you your wits? Know you what 'tis you speake?
> *Falstaff* My King! my Joue! I speake to thee, my heart.
> *King* I know thee not, old man: Fall to thy Prayers:/ How ill white haires become a Foole, and Jester?/ I haue long dream'd of such a kinde of man,/ So surfeit-swell'd, so old, and so profane:/ But, being awake, I do despise my dreame./ Make less thy body (hence) and more thy Grace,/ Leaue gormandizing; Know the Graue doth gape/ For thee, thrice wider then for other men."[23]

There is something very moving about this rejection, and it is anything but comical. Falstaff uses the language of love: "sweating with desire to see him . . . I speake to thee, my heart"; Hal's rejection, although justified by his position and situation, is one of cruelty, and the reader's sympathy cannot but be with Falstaff. Shakespeare makes it abundantly clear that this is no superficial or ephemeral relationship: the scene above is charged with a great intensity of feeling. The jesting and humour fall away to reveal a deeply serious relationship; the rejection of Falstaff culminates in his death – as he is about to die, the hostess tells his followers: "the King has kild his heart."[24]

The elimination of Falstaff at this stage of the play has always been something of a puzzle. In the epilogue of *2, Henry IV*, Shakespeare states his intention of using Falstaff in the sequel, *Henry V*, and there is no obvious reason why he should eliminate such an outstandingly successful comic character. A clue to the solution of this puzzle lies in the repentance shown by Falstaff as he dies – crying out "God, God, God". Falstaff's repentance is mirrored by the shift in John Shakespeare's attitude to religion during the period that these plays were written. In 1595 Shakespeare's father bought a book from a collection of four, the other three of which are known to have been prayer-books.[25] At some point between his non- attendance at church in 1592 and his leaving a Catholic religious will in about 1601, John Shakespeare was converted to Catholicism. The association between Falstaff and John Shakespeare explains the elimination of Falstaff from *Henry V:* at about the time the play was written (it was first published in 1599), John Shakespeare was undergoing a religious conversion that made him no longer a suitable subject for satire. It is not too fanciful to imagine that as he neared death, Shakespeare's father was afflicted with the kind of illness attributed to Falstaff at his end – changes which radically affected Shakespeare's attitude towards his comic creation.

CHAPTER 6:
THE FALL OF JOHN SHAKESPEARE

We are now in a position to address ourselves directly to the question posed at the beginning of this book: what is the explanation for John Shakespeare's fall from prosperity and status in the 1570's? Falstaff hints at a solution to this riddle in act two, scene two of *1, Henry IV*. He complains of Poins: "I am accurst to rob in that Theefe company . . . I haue forsworn his company hourley any time this two and twenty yeare, & yet I am bewitcht with the Rogues company. If the Rascall haue not given me medicines to make me loue him, Ile behang'd; it could not be else: I have drunke Medicines . . . And 'twere not as good a deed as to drinke to turne True- man ..." The first quarto of *2, Henry IV* was published in 1598 – "two and twenty years" previous to this takes us back to 1576, the year in which the dramatic deterioration in John Shakespeare's fortunes took place. There was a Master Poins living in the Stratford area at that time, although virtually nothing is known about him.[1] However, the importance of this passage is that it points us in the direction of an explanation of John Shakespeare's fall.

That Falstaff is virtually synonymous with drink and riotous living is commonplace; his addiction to sack is reflected in a long monologue extolling the virtues of alcohol: "A good Sherris-Sack hath a two-fold operation in it: it ascends me into the Braine, dryes me there all the foolish, and dull, and crudie Vapours, which enuiron it: makes it aprehensiue, quicke, forgetiue, full of nimble, fierie, and delectable shapes; which deliuered o're to the Voyce, the Tongue, which is the Birth, becomes excellent Wit. The second propertie of your excellent Sherris, is, the warming of the Blood; which before (cold and settled) left the Liuer white, and pale ..." (*2, Henry IV*, 4, iii).

There are echoes of Falstaff's character in a number of other plays; Christopher Sly in *The Taming Of The Shrew* is addicted to drink, and

although heavily satirized – there are suggestions of the Lambert family: "Am I not Christopher Sly, old Sly's son of Burton Heath ...?" – there are hints that he too once had led a sober life: "Oh how we joy to see your wit restor'd/ Oh that once more you knew but what you are:/ These fifteene yeeres you haue bin in a dreame." (Induction, ii). Sir Toby Belch in *Twelth Night*, is perhaps the character nearest to Falstaff, but not only is he a minor figure, he lacks the substance and depth, and does not come alive in the way that Falstaff does. The question of drink and drunkeness is explored very widely by Shakespeare in a number of the plays – for example, *2, Henry VI*, 2, iv, *Anthony And Cleopatra*, 2, vii, and *The Tempest*, 2, ii – but none of these, or the passages already quoted from *Henry IV*, do justice to a change which was as dramatic as that which took place in John Shakespeare's life. This would reverberate much more powerfully in his son's writings, and indeed we find just such a potent reverberation:

"*HAMLET, Act 1, Scene 4.*

Hamlet The ayre bites shroudly, it is very colde.
Horatio It is a nipping, and an eager ayre.
Hamlet What houre now?
Horatio I thinke it lackes of twelfe.
Marcellus No, it is strooke.
Horatio Indeede; I heard it not, it then drawes neere the season,/ Wherein the spirit held his wont to walke.
(A florish of trumpets and 2 peeces goes of.)
What does this meane my Lord?
Hamlet The King doth wake to night and takes his rowse./ Keepes wassell and the swaggring vp-spring reeles:/ And as he draines his drafts of Rhennish downe,/ The kettle drumme, and trumpet, thus bray out/ The triumph of his pledge.
Horatio Is it a custome?
Hamlet I marry ist/ But to my minde, though I am natiue heere/ And to the manner borne, it is a custome/ More honoured in the breach, then the obseruance./ This heavy headed reueale east and west/ Makes vs tradust, and taxed of other nations,/ They clip vs drunkards, and with Swinish phrase/ Soyle our addition, and indeede it takes/ From our achieuements, though perform'd at height/ The pith and marrow of our attribute,/ So oft it chaunces in particuler men,/ That for some vicious mole of nature in them/ As in their birth wherein they are not guilty,/ (Since nature cannot choose his origin)/ By their ore-grow'th of some complextion/ Oft breaking downe the pales and forts of reason,/ Or by some habit, that too much ore-leauens/ The forme of plausiue manners, that these men/ Carrying I say the stamp of one defect/ Being Natures liuery, or Fortunes starre,/ His vertues els be they as pure as grace,/ As infinite as man may vndergoe,/ Shall in the

> generall censure take corruption/ From that particular fault : the dram of eale/ Doth all the noble substance of a doubt/ To his own scandle.
>
> *Enter Ghost.*"[2]

The solution to the enigma of John Shakespeare's fall is revealed in this passage: "This heauy headed reueale east and west/ Makes vs tradust, and taxed of other nations,/ They clip vs drunkards, and with Swinish phrase/ Soyle our addition, and indeede it takes/ From our achieuements, though perform'd at height." Due to alcoholism, John Shakespeare fell in economic and social status, even though he had reached a pinnacle of success in 1576. The result was a stigma with traumatic impact on Shakespeare; much of his determination to succeed derived from a reaction to this failure. The rejection of Falstaff is a symbolic representation of the turning away from the world of John Shakespeare, with all its failure and social humiliation. The experience influenced his work, not only through the autobiographical plays which involved the Falstaff/ John Shakespeare connection, but also through the later, more tragic writing, particularly *Hamlet*.

This writing was not of course merely a reflection of autobiographical experience. Jenkins has pointed out that the passage quoted above from *Hamlet* was heavily influenced by Nashe's *Pierce Penniless*, which describes the Danes as "bursten-bellied sots", refers to "the quick-witted Italians . . . [who] detest this *swinish* generation", and categorizes the reveller at home in contrast to the reveller abroad as "the *heavy-headed* gluttonous house-dove."[3] He goes on to cite a passage which has many similarities to the one in *Hamlet;* referring to drunkeness, Nashe tells us

> "A mightie deformer of mens manners and features, is this vnnecessary vice of all other. Let him be indued with neuer so many vertues, and haue as many goodly proportion and fauer as nature can bestow vppon man: yet if hee be thirstie after his own destruction, and hath no ioy nor comfort, but when he drowning his soul in a gallon pot, that one beastly imperfection will vtterlie obscure all that is commendable in him; and all his good qualities sinke like lead down to the bottome of his carrowsing cups, where they will lie, like lees and dregges, dead and vnregarded of any man."[4]

Undoubtedly, Shakespeare was influenced by this passage, as well as the other references to drunkenness in Nashe's work. But the above passage refers to the behaviour of Englishmen, and drunkenness is singled out as one of their most prevalent vices. Shakespeare himself confirms the reputation of Englishmen for drunkenness through one of Iago's speeches – referring to a song he has just sung: "I learn'd it in

England: where indeed they are most potent in Potting. Your Dane, your Germaine, and your swag-belly'd Hollander, (drinke hoa) are nothing to your English . . . Why, he drinkes you with facillitie, your Dane dead drunke. He sweates not to ouerthrow your Almaine; he giues your Hollander a vomit, ere the next Pottle can be fill'd."[5]

There is no incompatibility between Nashe's influence on Shakespeare, and the autobiographical nature of the passage about drunkenness in *Hamlet*. "Perception is functionally selective", and material which has strong personal resonance, is likely for that reason, to be remembered and used at a later date. But this raises the question as to what led John Shakespeare to turn to drink in such disastrous fashion in 1576? A clue to the answer might lie in *The Rape Of Lucrece;* discussing Tarquin's motivation, Shakespeare tells us:

> "Those that much covet are with gain so fond/ That what they have not, that which they possess/ They scatter and unloose it from their bond,/ And so, by hoping more, the profit of excess/ Is but to surfeit, and such griefs sustain/ That they prove bankrupt in this poor-rich gain ... So that in vent'ring ill we leave to be/ The things we are for that which we expect;/ And this ambitious foul infirmity,/ In having much, torments us with defect/ Of that we have; so then we do neglect/ The thing we have and, all for want of wit,/ Make something nothing by augmenting it."[6]

Shakespeare reveals more about the loss of money and wealth in *Timon Of Athens;* Timon's loyal steward Flavius, tells us how Timon has become bankrupt: "When all our Offices hauve been oppresst/ With riotous Feeders, when our Vaults haue wept/ With drunken spilth of Wine, when euery roome/ Hath blaz'd with Lights, and braid with Minstrelsie,/ I have retyr'd me to a wastefull cocke/ And set mine eyes at flow."[7] Flavious elaborates on this theme in a further speech: "No care, no stop, so senselesse of expence/ That he will neither know how to maintaine it/ Nor cease his flow of Riot."[8] The language of riot pervades the accounts of Timon's fall – and the counter-pointing between surfeit, excess and reaction is a strong part of Timon's fate. There is something in the excess of Timon that is reminscent of Falstaff – "so surfeit swelled": his genorosity, his "drunken spilth of wine", and "minstrelsie." And as Shakespeare knew, the reaction of the puritan to the frustrations of constraint led to "gravity's revolt to wantoness". Although there is evidence that like Tarquin, John Shakespeare did "prove bankrupt in this poor-rich gain", there was a suddenness in his fall that requires special explanation. Part of the explanation probably lies in his life as an

individual trader. Everitt has described the dangers and vulnerabilities that traders were particularly suseptible to, and this is so central, that it will be quoted fully:

> "There were no banks, and a man's 'credit' could only be what he was taken to be worth in his local community. The very word 'credit' was synonymous with a man's 'estate', 'worth', or 'standing' in local society; but when factors and drovers were travelling all over the country such a conception became nugatory. It was only through the highly personal links of the wayfaring community that it retained any meaning. The problem was really twofold. First, there was a strong temptation for those with much address or little acumen to engage in dealings beyond their means or worth. All went well till some customer defaulted or the harvest failed; then disaster ensued, perhaps involving a score of other victims, unaware of their client's shaky status when they dealt with him. Secondly, there was the temptation to inflict injury on the credit of one's opponent. The dread of 'losing reputation' was not simply a fear of losing caste. It might mean the end of one's business and the impoverishment of one's family. When a wool dealer of Warwick caused his former partner John Lee, glover, 'to be arrested in Cirencester in the county of Gloucester in the open market there, where (Lee's) most credit was, by writ of *capias ad satisfaciend*,' that partner found himself greatly damnified and 'almost utterly undone'. The reputation of Humphrie Grigg of Beaudesert in Warwickshire, as a result of 'unlawful and unconscionable proceedings' of Robert Wheeler of Tanworth in Arden, was 'called into question and his estate descanted upon, so far as whereas before such time . . . his word and credit was current and would pass in the country with and amongst his neighbours . . . now they make it very scrupulous to take (his) word or promise, yea scarcely his bond for a matter of five pounds': so that he is 'prejudiced and damnified exceedingly not only in his reputation and credit but also in his private estate;' his creditors calling on him 'for their money faster than (he) can provide . . . threatening (him) with arrests and suits, so as (he) dare not stay at home in his own house or in the country, but is forced to fly and obscure himself from his own house, wife, and family, to his exceeding great loss ..."[9]

There are remarkable resonances in this passage: a wool dealer of Warwick, ruining his former partner, John Lee, glover; and Humphrie Grigg of Beaudesert, Warwickshire, daring not to stay at home, for fear of arrest of debt – it all has a highly familiar ring. And there is some evidence that John Shakespeare's reputation had been damaged in the way that Everitt has outlined; not only was he pursued for debt, but in his 1597 submission in the Lambert dispute he stated that "John Lamberte ys of greate wealthe and abilitie, and well frended and alied amongest gentlemen and freeholders of the countrey in the saide countie of

Warwicke, where he dwelleth, and your said oratours are of small wealthe and verey fewe frendes and alyance in the saide countie."[10] This of course might have been exaggeration to elicit the sympathy of the court, but it is compatible with John Shakespeare having lost "credit-worthiness". Everitt also quotes evidence about the rapidity of the rise and fall of traders, which echoes the passage quoted above on Tarquin. Robert Reyce of Suffolk described the kind of trader who "in short time climbeth to much credit and wealth" and "such is the world, which is nothing but a shop of all change, that too often it falleth out for these riches thus hastily gotten, which can abide no enduring continuance, (that) they receive in exchange first decay, which when willingness in suretyship hath laid open too manifestly, then poverty too, too quickly remedyless seizes them."[11]

However, there is no direct evidence that John Shakespeare suddenly lost his credit-worthiness, although there was a very rapid deterioration in his economic position. In about 1576 he applied for a coat of arms and in order to achieve this, the applicant had to "live without manuell labour, and therefore to . . . beare the port, charge and countenance of a gentleman." We are describing a shift from the protestant ethic of the earlier period, when John Shakespeare was building his family fortunes, to this gentlemanly way of life. John Wesley understood this process better than most: "I fear, wherever riches have increased, the essence of religion has decreased in the same proportion. Therefore I do not see how it is possible, in the nature of things, for any revival of true religion to continue long. For religion must necessarily produce both industry and frugality, and these cannot produce riches. But as riches increase, so will pride, anger, and love of the world in all its branches."[12]

Protestantism has always engendered its own particular brand of vulnerability: stripped of the protective rituals, beliefs and practices of traditional Roman Catholicism, Protestants were thrown back on their own psychological resources. Where it was impossible to alleviate a sense of guilt through institutionalised procedures such as the confessional, psychological vulnerabilities were likely to acquire the status of "tragic flaws". That this should take the form of alcoholism in John Shakespeare's case, was partly due to his work as an individual trader; Everitt has described the hazards of working in this way: "When so much bargaining was undertaken in alehouses, inns, and farm-kitchens all manner of abuses might occur. The case of one Richard Snellinge of

London may be taken as typical. Employed by a poulterer of Gracechurch Street 'in journeying and riding to country markets to buy and provide poultry wares,' Snellinge was despatched by his master with two horses 'worth twenty nobles apiece' to Potton in Bedfordshire. Meeting there in 'riotous manner with two country chapmen' from whom he apparently brought poultry, he went to the Sign of the George in Potton and there continued drinking until they were all drunk . . . slept in the stable of the said inn until seven o'clock at night."[13] The element of riot in Richard Snellinge's behaviour is reminiscent of Falstaff and his companions, but we do not have to interpret this too literally to see that John Shakespeare's work as an individual trader, exposed him constantly to alcoholic drink and its effects.

Additionally, there were hazards in living and working in Stratford, particularly for members of the corporation. We saw earlier that Stratford's chief manufacturing activity was the production of malt; this was reflected in the number of its alehouses, taverns and inns. On the 29th April, 1552 there were listed no less than forty-five tipplers (running alehouses), four innkeepers, and two vintners[14] – representing about one tavern or inn to every thirty people (men, women and children) living in Stratford. From council records, there appears to have been no diminution in the numbers of alehouses and inns during the latter half of the sixteenth century – and this was at a time when home brewing was almost universal. The effect on local life was sufficiently disturbing for the Stratford Corporation on the 8th December, 1592 to totally ban all innholders, alehouse keepers and tipplers from serving "any pore Artificer, day laborer, mens servantes or prentises . . . by day or by night (excepte in the tyme of Christmas) upon payne of Imprisonment by the space of thre dayes & thre nightes of them that be the householders & never to sell Any more Ale or Bere wthin this Boroughe."[15] Six years later things had deteriorated to a point whereby the corporation passed a formal motion deploring the effect of drink on the life of the town: "On 27 September [1598] . . . a strong resolution was passed with respect to the 'great disorder' in the borough caused by the Tipplers (otherwise alehouse keepers), 'through their unreasonable strong drink, to the increase of quarrelling and other misdemeanours in their houses, and the further and greater impoverishment of many poor men haunting the said houses, when their wives and children are in extremity of begging ...' "[16]

But the effects of drink were not confined to the poor; the corporation banned the latter from drinking in ale-houses, but they themselves

continued to lavishly entertain visiting magistrates, officials, preachers and players with wine and sack – and of course, while John Shakespeare was a member of the council, he would have participated in these entertainments. The corporation used two inns for this purpose: the Bear and the Swan – both located at the end of Henley Street. And we can most aptly conclude this chapter, by quoting from Halliwell-Phillips's description of the Bear based on an inventory taken in about 1603: "The Bear near the foot of the bridge possessed its large hall, its nominated rooms such as the Lion and Talbot chambers, an enormous quantity of house linen, a whole pipe of claret, two butts of sack, and, among its plate, 'one goblet of silver, parcel gilt' "[17] – reminiscent of Mistress Quickly's admonishment of Falstaff in *2, Henry IV:* "Thou didst sweare to mee upon a parcell of gilt Goblet, sitting in my Dolphin-chamber."

CHAPTER 7:
THE DEATH OF JOHN SHAKESPEARE AND THE WRITING OF HAMLET

Hamlet was the key play for revealing the secret behind the enigma of Shakespeare's father's life. Why then was this secret revealed there and not in one of the earlier autobiographical plays? If as Freud suggested, *Hamlet* was written in reaction to John Shakespeare's death, questions of significance and meaning would have been part of that response. The dating of *Hamlet* in relation to Shakespeare's father's death is also important because it could tell us a great deal about the psychological context of the play, as well as possibly explaining the shift in tone and content of Shakespeare's later work – the beginning of the "dark period", which ushered in the great tragedies of the 1600s.

But what is the evidence that *Hamlet* was written after September 1601, when John Shakespeare died? The dating of *Hamlet* is a highly contentious issue, much discussed by Shakespeare scholars. The only certain date is that on which it was first registered, 26th July 1602, when it was listed as "A booke called the Revenge of Hamlett Prince Denmarke as yt was latlie Acted by the Lord Chamberlayne his servantes."[1] One of the most important pieces of evidence for dating *Hamlet* is Gabriel Harvey's note in the 1598 Speght edition of Chaucer, which mentioned Shakespeare's "tragedie of Hamlet, Prince of Denmarke". Jenkins has succinctly summarized the evidence as follows:

> "When exactly, after his purchase of the book in 1598, Harvey wrote this note can only be inferred from other allusions in it; and these are strangely conflicting. The inclusion among our 'florishing metricians' of Spenser, who died on 16 January 1599, is apparently not indicative since the same list oddly includes Watson, who died in 1592; but a statement of what 'the Earle of Essex commendes' and what 'the Lord Mountioy makes the like account

of' points to a time not only before Mountjoy was made Earl of Devonshire in July 1603 but before Essex met his death on 25 February 1601."[2]

Jenkins concludes that the balance of evidence is for a date before 25 February 1601, but notes with some perplexity that Harvey also made reference to "Owens new Epigrams", and these were not published until 1606.[3] A number of scholars have pointed out that the reference to the Earl of Essex in the present is not conclusive: it is possible to quote a dead man's critical opinions in the present tense, a point supported by Harvey's description of the long-dead Watson as a 'florishing metrician'. Even the reference to Lord Mountjoy is not conclusive, for as Kirschbaum argued, Elizabethans were not always careful about the use of titles.[4] Given the degree of conflict of evidence, and the confusion about language and chronology, no firm conclusion about the dating of *Hamlet* can be reached from Harvey's note.

The other major evidence for dating the play is the famous "little eyases" passage, which appeared in the Folio edition. The following is a dialogue between Hamlet and Rosencrantz & Guildenstern about the players who have just arrived at the Danish court:

"*Hamlet* . . . what Players are they?
Rosencrantz Euen those you were wont to take delight in the Tragedians of the City.
Hamlet How chances it they trauaile? their residence both in reputation and profit, was better both wayes.
Rosencrantz I thinke their Inhibition comes by the meanes of the late Innouation.
Hamlet Doe they hold the same estimation they did when I was in the City? Are they so follow'd?
Rosencrantz No indeed, they are not.
Hamlet How comes it? doe they grow rusty?
Rosencrantz Nay, their indeauour keepes in the wonted pace; but there is Sir an ayrie of Children, little Yases, that crye out on the top of the question; and are most tyrannically clapp't for't: these are now the fashion, and so be-ratled the common Stages (so they call them) that many wearing Rapiers, are affraide of Goose-quils, and dare scarce come thither.
Hamlet What are they Children? Who maintains 'em? How are they escoted? Will they pursue the Quality no longer than they can sing? Will they not say afterwards if they should grow themselues to common Players (as it is like most if their meanes are no better) their Writers do them wrong, to make them exclaim against their own Succession.
Rosencrantz Faith there ha's bene much to do on both sides: and the Nation holds it no sinne, to tarre them to Controuersie. There was for a while, no money bid for argument, vnless the Poet and the Player went to Cuffes in the Question.

Hamlet Is't possible?
Guildenstern Oh, there ha's beene much throwing about of Braines.
Hamlet Do the Boyes carry it away?
Rosencrantz I that they do my Lord, *Hercules* & his load too."[5]

It is generally recognized that this is a reference to the "war of the theatres", reflecting a dispute mainly between Jonson on the one hand, and Marston and Dekker on the other, which involved a secondary conflict between the private children's theatres and the public playhouses. The dispute appears to have started with Marston's play *Histriomastix*, written in 1599, possibly for the Children of Paul's.[6] Marston portrayed Jonson in the play, and the latter in turn parodied some of Marston's language in his *Every Man Out Of His Humour*, produced at the end of 1599 or the beginning of 1600. Marston's response was *Jack Drum's Entertainment*, acted by the Children of Paul's in about August 1600; in turn, Jonson replied with *Cynthia's Revels*, which was staged in about December 1600 by the Children of the Revels in their newly opened Blackfriars theatre. Marston probably rejoined with *What You Will* in the early spring of 1601, again acted by the Children of Paul's. Jonson's final response was *The Poetaster* – staged by the Children of the Revels – and as this is the key play for understanding the "little eyases" passage in *Hamlet*, it requires careful consideration.

According to the title page of the 1640 Folio edition of *Poetaster*, the play was "First Acted in the yeare 1601. By the then Children of Queene ELIZABETHS CHAPPELL." In the induction to the play, Jonson (who was a notoriously slow writer) tells us that it was written in anticipation of an attack on him in a play yet to be published: "these fifteene weekes/ (So long as since the plot was but an *embrion*)/ Haue I, with burning lights, mixt vigilant thoughts,/ In expectation of this hated play."[7] Although ostensibly set in Rome, virtually the whole of Act 3, Scene iv is devoted to a satirical attack on the writers and players of the public playhouses, which therefore broadens what was a personal dispute between Marston and Jonson, into a "war of the theatres". Jonson appears to have had the Chamberlain's Men particularly in mind (this may have been partly because of the lack of success of his *Every Man Out Of His Humour* with that company), and contrives the following dialogue between Tucca and Histrio, the latter intended probably as a satire on one of the Chamberlain's players:

"TUCCA . . . I would faine come with my cockatrice one day, and see a play; if I knew when there were a good bawdie one: but they say, you ha'

> nothing but *humours*, *reuells*, and *satyres*, that girde, and fart at the time, you slaue.
> HISTRIO No, I assure you, Captaine, not wee. They are on the other side of *Tyber:* we haue as much ribaldrie in our plaies, as can bee, as you would wish, Captaine: All the sinners, i' the suburbs, come, and applaud our action, daily.
> TUCCA I heare, you'll bring me o' the stage there; you'll play me, they say: I shall be presented by a sort of copper-lac't scoundrels of you: life of PLVTO, and you stage me, stinkard; your mansions shall sweat for't, your tabernacles, varlets, your *Globes*, and your *Triumphs*.
> HISTRIO Not we, by PHOEBVS, Captaine: doe not doe vs imputation without desert.
> TUCCA I wu' not, my good two-penny rascall: reach mee thy neufe. Do'st heare? What wilt thou giue mee a weeke, for my brace of beagles, here, my little point-trussers? you shall ha' them act among yee."[8]

Jonson here is satirically contrasting his own work being performed at the Blackfriars north of the Thames, with the Chamberlain's plays performed at the Globe south of the river, set amongst the "stews" and other places of "disreputable" activity. His reference to "your Globes" indicates that it was the Chamberlain's men that he had in mind, and Tucca's threat that his "little point-trussers" will "act among yee", appears to be a clear reference to the commercial success of the Children of the Revel's company. Jonson probably included Shakespeare in this satire through the figure of Pantolabus, although this mainly showed Shakespeare in a favourable light. The tone of Jonson's satire at times bordered on the offensive – for example, he refers at one point to the players as "punkes decai'd i' their practice"[9] – and there is no doubt that this whole conflict seriously damaged the Chamberlain's Men. Towards the end of Act 4, Scene iv, Jonson puts the following words into the mouths of Tucca and Histrio, concerning Demetrius, a satire on Dekker:

> "HISTRIO . . . hee is . . . one DEMETRIVS, a dresser of plaies about the towne, here; we haue hir'd him to abuse HORACE, and bring him in, in a play, with all his gallants ...
> TUCCA And: why so, stinkard?
> HISTRIO O, it will get vs a huge deale of money (Captaine) and wee haue need on't; for this winter ha's made vs all poorer, then so many staru'd snakes: No bodie comes at vs; not a gentleman, nor a – "[10]

Horace is a reference to Jonson himself, and if this passage is to be believed, Jonson's satirical work for the Children of the Revels had seriously undermined the fortunes of Shakespeare's company. What had previously been a dispute between individual playwrights, had become a

serious commercial battle between the private children's companies and the public theatres. Jonson justified his attack on the latter in a special "apologeticall Dialogue: which was only once spoken upon the stage":

> "... three yeeres,/ They did prouoke me with their petulant stiles/ On euery stage: And I at last, vnwilling,/ But weary, I confesse, of so much trouble,/ Thought I would try, if shame could winne vpon 'hem . . . Now, for the Players, it is true, I tax'd 'hem,/ And yet, but some; and those so sparingly . . . If it gaue 'hem meat,/ Or got 'hem clothes. 'Tis well. That was their end."[11]

Although apologetical in tone, Jonson had successfully predicted (through the speech of Histrio) that the Chamberlain's men were going to stage a play attacking him, and that they were going to do this partly to attract back the audience lost to the children's theatres. If we are to believe the "apologeticall Dialogue" this was successful, for "it gaue 'hem meat" and "got 'hem clothes". This presumably was achieved through Dekker's play *Satiromastix*, which was performed in 1601 by the Chamberlain's Men and by the Paul's Children.[12] The play is widely recognized as a reply to Jonson *Poetaster*, as there are many references to the latter which are counters to Jonson's satire.

Shakespeare almost certainly had Jonson's *Poetaster* in mind when writing the "little eyases" passage: "they berattle the common stages" and "their writers do them wrong to make them exclaim against their own succession", i.e. against the adult players of the public theatre. Jonson's play was the major vehicle for this attack on the players, all the more galling because of coming out of the mouths of the "little point- trussers". But Shakespeare also had in mind Dekker's counter- attack : "there has been much to do on both sides." This refers not just to the dispute between Jonson and Marston – both writing for children's theatres – but between Jonson and Dekker, concentrating on Jonson's satire of the public players. The Chamberlain's Men revived their fortunes through the attack on Jonson in *Satiromastix*, evidenced by both Jonson and Shakespeare.

Poetaster was written and first performed in 1601, probably in the Spring of that year ("this winter ha's made vs all poorer, than so many staru'd snakes.") *Satiromastix* followed in response, and presumably took Dekker at least some weeks, if not months, to write; Fredson Bowers, the modern editor of Dekker's works, believes that *Satiromastix* was not completed until after 14 August, on the grounds that it made reference to "*the Whipping a'th Satyre*", a verse satire which was not

entered into the Stationer's Register until that date.[13] Shakespeare referred to the "war of the theatres" in the past tense – "There was for a while no money bid for argument unless the poet and the player went to cuffs in the question" – and this almost certainly includes the Chamberlain's Men's attack on Jonson in Dekker's *Satiromastix*. Given the evidence on the date of writing of the latter, it is likely that *Hamlet* was written some while after August 1601.

There is some evidence that the play was written before the Christmas holiday period of 1601. The students of St John's College, Cambridge put on a play known as *The Return from Parnassus*, Part 2, for the Christmas holiday, and in that play, there is a scene where two graduates apply for jobs as writers with the Chamberlain's Men; one of their managers, Kempe, expresses his scepticism as follows: "Few of the vniuersity men pen plaies well, they smell too much of that writer *Ouid*, and that writer *Metamorphoses*, and talke too much of *Prosperina & Jupiter*. Why heres our fellow *Shakespeare* puts them all downe, I and *Ben Jonson* too. O that *Ben Jonson* is a pestilent fellow, he brought vp *Horace* giuing the Poets a pill, but our fellow *Shakespeare* hath giuen him a purge that made him betray his credit."[14]

Horace giving the poets a pill, refers to *Poetaster*, where Horace (Jonson) gives pills to Crispinus (Marston) to make him vomit, but the reference to Shakespeare giving Jonson a purge, has somewhat puzzled Shakespeare scholars. However, on the present reading of the "war of the theatres", the purge referred to is the "little eyases" passage of *Hamlet;* Shakespeare has Hamlet say of the Children's Companies: "Will they not say afterwards if they should grow themselues to common Players (as it is like most if their meanes are no better) their Writers do them wrong, to make them exclaim against their owne Succession?" Here we have a weighty and justified moral attack on Jonson, the purge referred to in the *Parnassus* play. Jonson in his "apologeticall Dialogue" printed in the Quarto of *Poetaster*, registered on the 21st December 1601, refers to his critics but singles out one of them for special comment: "Onely amongst them, I am sorry for/ Some better natures, by the rest so drawne,/ To rune in that vile line" – presumably a reference to Shakespeare and his criticism of Jonson. Thus on this evidence, *Hamlet* must have been written before the end of December, 1601.

One reason why scholars have resisted a date for *Hamlet* in late 1601, is the reference to the "late innovation", which some have interpreted as relating to the abortive Essex rebellion of February 1601. Essex's

supporters paid Shakespeare's company, the Lord Chamberlain's Men, for a special performance of *Richard 2* (in which King Richard is deposed and killed), in order to encourage support for the rebellion. As a result, Augustine Phillips was required to explain the Chamberlain's Men's role in the affair[15], and some have suggested that this led to the suppression of the company and its performance of plays in London. But no action was taken against the company, and on the 24th February – the day before Essex's execution – they played before the Queen at Whitehall; it is therefore difficult to give any credence to the argument that the rebellion was the innovation that Shakespeare referred to. That the innovation was a reference to the child actors, is strongly supported by the first Quarto of *Hamlet;* in that edition, the lengthy "little eyases" passage quoted above is severely truncated, and the reason for the players travelling is explicitly given as follows: "noueltie carries it away,/ For the principall publike audience that/ Came to them, are turned to priuate playes,/ And to the humour of children."[16]

The balance of all the evidence suggests that *Hamlet* was written in late 1601, and probably after John Shakespeare's death in September, and before the Christmas holiday of that year. *Hamlet* is the beginning of an entirely new phase in Shakespeare's work, and represents a radical departure from previous writing. It was the beginning of the period in which Shakespeare wrote most of his great tragedies; on the present argument, it was John Shakespeare's death which was responsible for this radical shift in tone and content. The intensity of Shakespeare's reaction to his father's death is indicated in *Hamlet:* "Remember thee?/ I, thou poore Ghost, while memory holds a seate/ In this distracted Globe: Remember thee?/ Yea, from the Table of my Memory/ Ile wipe away all triuial fond Records,/ All sawes of Bookes, all formes, all pressures past,/ That youth and obseruation coppied there;/ And thy Commandment all alone shall liue/ Within the Booke and Volume of my Braine,/ Vnmixt with baser matter."[17] What clearer manifesto for the ensuing "dark period" could we find? The depth of Hamlet's grief is indicated by both the Queen his mother and Claudius, his uncle King:

> "*Queen* . . . Do not for euer with thy veyled lids/ Seeke for thy Noble Father in the dust;/ Thou know'st 'tis common, all that liues must dye,/ Passing through Nature, to Eternity ...
> *King* 'Tis sweet and commendable in your Nature *Hamlet*,/ To giue these mourning duties to your Father:/ But you must know, your Father lost a Father,/ That father lost his, and the Suruiuer bound/ In filiall Obligation,

> for some terme/ To do obsequious Sorrow. But to perseuer/ In obstinate Condolement, is a course/ Of impious stubbornnesse. 'Tis vnmanly greefe . . . a fault to Nature . . . whose common Theame/ Is death of Fathers, and who still hath cried,/ From the first Coarse, till he that dyed to day,/ This must be so."[18]

That this is not exaggeration used by the King and Queen to undermine Hamlet, is confirmed by Hamlet's own admission to Guilderstern: "... I have of late, but wherefore I know not, lost all my mirth, forgone all custome of exercise; and indeed, it goes so heauily with my disposition, that this goodly frame the Earth, seemes to me a sterill Promontory ..."[19] Grief at the death of a father is a general theme of the play: Hamlet, Ophelia and Laertes all lament the death of their fathers. Orphelia is said to have gone mad as a result of her father's death – "it springs/ All from her father's death" – and perhaps the following song is an appropriate lament for the death of John Shakespeare:

> "They bore him bare fac'd on the Bier,/ Hey non nony, nony, hey nony:/ And on his graue raines many a teare . . . And will he not come againe,/ And will he not come againe:/ No, no, he is dead, go to thy Death-bed,/ He neuer wil come againe./ His Beard as white as Snow,/ All Flaxen was his Pole:/ He is gone, he is gone, and we cast away mone: Gramercy on his Soule."[20]

We can make no better conclusion to the main argument of this section of the book than quote from *Hamlet*, as an epitaph on John Shakespeare: "Alas poore *Yoricke*, I knew him *Horatio*, a fellow of infinite jest, of most excellent fancie, hee hath bore me on his backe a thousand times, and now how abhorred in my imagination it is: my gorge rises at it. Heere hung those lyppes that I have kist I know not howe oft, where be your gibes now? your gamboles? your songs, your flashes of merriment ..."[21]

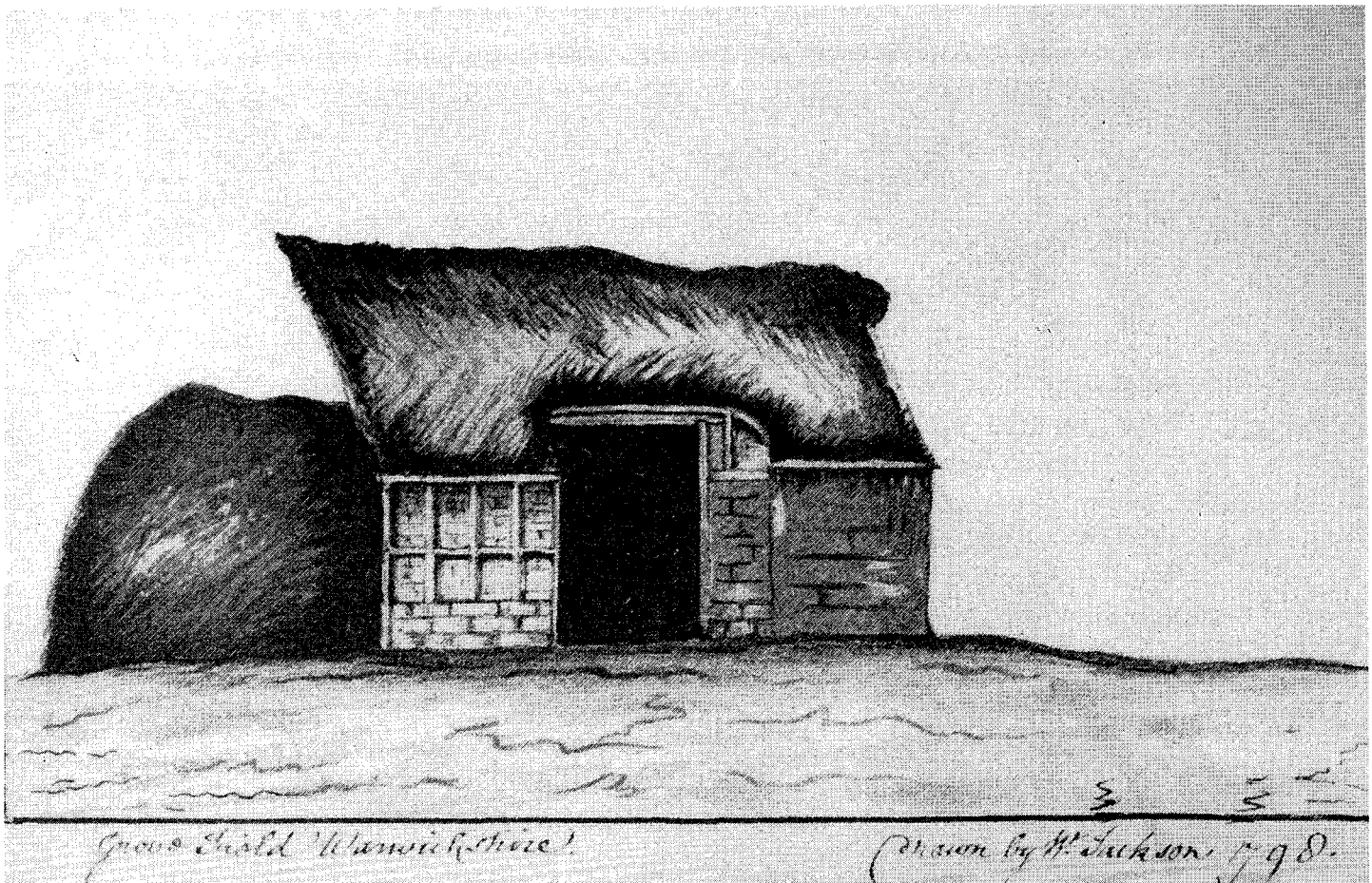
SECTION II

"Stratford Bridge" (From Samuel Ireland, Picturesque Views on the Warwickshire Avon *[1795])*

"The House in which Shakespeare was Born" (From William Smith, A New and Compendious History of the County of Warwick *[1830])*

Barton-On-The-Heath, Warwickshire

"View of Charlecote Deer Park" (From William Smith, A New and Compendious History of the County of Warwick *[1830])*



"The Deer Barn" (Drawn by W. Jackson in 1798).

"Daisy Hill Farm House" (From Samuel Ireland, Picturesque Views on the Warwickshire Avon *[1795])*

"View of Weston Hall" (From William Smith, A New and Compendious History of the County of Warwick *[1830])*

The Bust of William Shakespeare, in Stratford Parish Church

CHAPTER 8:
THE DEER POACHING TRADITION

As we have seen, one of the most autobiographical of the plays is *The Merry Wives Of Windsor*, and there are a number of passages which have been generally recognized as applying to Shakespeare himself. The comical cross-examination of Mistress Page's son William on his Latin grammar by Parson Hugh Evans, has suggested to most commentators Shakespeare's own experience with the Stratford schoolmaster Thomas Jenkins – and Mistress Page's comment to Sir Hugh that "my husband saies my sonne [William] profits nothing in the world at his Booke", has echoes of the tradition that Shakepeare was prematurely withdrawn from school because of his father's economic difficulties.[1] Likewise the character of Gentle Master Fenton is consistent with what we know of Shakespeare as a young man; as the host in the play describes him: "He capers, he dances, he has eies of youth: he writes verses, hee speakes holiday, he smels April and May." At the end of the play, Fenton runs away with Ann Page and marries her very much against her parents' wishes – reminiscent of Shakespeare's hurried marriage to Anne Hathaway. But most important of all, is the wildness of Fenton's youth: "The Gentleman is of no hauing, he kept companie with the wilde Prince, and *Poïntz:* he is of too high a Region, he knows too much" – an accusation which is admitted by Fenton himself, confessing to his "Riots past, my wilde Societies."

This brings us to the most important of all the autobiographical themes in *The Merry Wives Of Windsor:* the poaching of deer from Justice Shallow, reputedly based on the local magistrate, Sir Thomas Lucy. The two main accounts of this story are those told by Richard Davies and Nicholas Rowe. Davies wrote at the end of the seventeenth century: [Shakespeare was] "much given to all unluckinesse in stealing venison & rabbits particularly from Sr [Thomas] Lucy who had him oft whipt &

sometimes imprisoned & at last made him fly his native country to his great advancement. But his reveng was so great that he [Lucy] is his Justice Clodplate [Shallow] and calls him a great man & yt in allusion to his name bore three lowses rampant for his Arms."[2] Rowe wrote a somewhat fuller version of the story in his biography of Shakespeare, published in 1709:

> "He [Shakespeare] had, by misfortune common enough to young fellows, fallen into ill company; and amongst them, some that made a frequent practice of Deer-stealing, engag'd him with more than once in robbing a park that belong'd to Sir Thomas Lucy of Cherlecot, near Stratford. For this he was prosecuted by that Gentleman, as he thought, somewhat too severely; and in order to revenge that ill usage, he made a ballad upon him. And tho' this, probably the first essay of his poetry, be lost, yet it is said to have been so very bitter, that it redoubled the prosecution against him to that degree, that he was oblig'd to leave his business and family in Warwickshire, for some time, and shelter himself in London."[3]

The fullest account of this ballad is that published by Edward Capell; Capell described how this version came into his hands:

> "One stanza of it, which has the appearance of genuine, was put into the editor's hands many years ago by an ingenious gentleman (grandson of its preserver) with this account of the way it descended to him. – Mr Thomas Jones, who dwelt at Tarbick [Tardebigge] a village in Worcestershire a few miles from Stratford-on-Avon, and dy'd in the year 1703 aged upwards of ninety, remember'd to have heard from several old people at Stratford the story of Shakespeare's robbing Sir Thomas Lucy's park; and their account of it agreed with Mr Rowe's, with this addition – that the ballad written against Sir Thomas by Shakespeare was stuck upon his park gate, which exasperated the knight to apply to a lawyer at Warwick to proceed against him: Mr Jones had put down in writing the first stanza of this ballad, which was all that he remember'd of it, and Mr Thomas Wilkes (my grandfather) transmitted it to my father by memory, who also took it in writing, and his copy is this: 'A parliamente member a justice of peace,/ At Home a poore Scarecrow at London an asse./ If Lowsie is Lucy as some Volke miscalle it/ then Lucy is Lowsie whatever befalle it/ He thinkes himself greate/ Yet an asse is his state/ we allowe by his eares but with asses to mate/ If Lucy is Lowsie as some Volke miscalle it/ Sing (O) Lowsie Lucy whatever befalle it'."[4]

Two of these three accounts of the poaching incident either state or imply that it took place at Sir Thomas Lucy's park at Charlecote. Towards the end of the eighteenth century, a new tradition emerged which relocated the incident to a neighbouring deer park at Fulbrook. The first person to put this tradition into print was John Jordan, a self-educated

local wheelwright, who collected a number of stories about Shakespeare. Jordan's version of the incident, written in 1790, is as follows:

> "[Sir Thomas Lucy] had at the time also another park at a place called Fullbroke, two miles distant from the other; and there tradition reports it was that Shakespeare and his companions made a practise of following their favourite diversion; which they did so often, that the knight's resentment was raised, and he commenced a prosecution against them, but desisted upon their making an abject submission; but which so hurt the high spirit of our poet that he could not repress his idignation. A satirical song went abroad, which inflamed Sir Thomas to the utmost pitch, and he renewed the prosecution with redoubled vigour. His power was too great for poor Shakespeare to contend with, and he now saw, perhaps with horror, that his youthful levity obliged him to quit his father, his fond wife, his prattling babes, and his native place."[5]

In 1788, the Rev. S. Cooper, of Loxley, had written to Jordan, mentioning a farm "at Ingon containing now about two hundred acres of land adjoining to the old park, now deparked, from which it is said William Shakespeare stole Mr Lucy's deer."[6] This may be a reference to Fulbrook, but will be discussed when an evaluation is made of the historical accuracy of the poaching tradition. A further claim that the poaching took place at Fulbrook was made by Samuel Ireland in 1795:

> "It was in this [Fulbrook] park our bard is said to have been, in a youthful frolic, engaged in stealing deer, and thereby to have drawn upon himself a prosecution from the then owner, Sir Thomas Lucy . . . Within this park is now standing, on a spot called Daisy Hill, a farm house, which was antiently the keeper's lodge. To this lodge it is reported our Shakespeare was conveyed, and there confined at the time of the charge, which is supposed to have been brought against him."[7]

Sir Walter Scott is thought to have referred to Fulbrook when he made a note in his diary in 1828 at the time of a visit to Charlecote:

> "April 8 . . . Charlecote is in high preservation, and inhabited by Mr Lucy, descendant of the worshipful Sir Thomas . . . He told me the park from which Shakespeare stole the buck was not that which surrounds Charlecote, but belonged to a mansion at some distance, where Sir Thomas resided at the time of the trespass. The tradition went that they hid the buck in a barn, part of which was standing a few years ago, but now totally decayed. This park no longer belongs to the Lucys."[8]

According to Lady Alice Fairfax-Lucy, George Lucy who had told Scott the above, "had busied himself with collecting a vast mass of notes on Lucy genealogy."[9] A separate reference to the deer barn was given by W.Jackson, who had made a drawing of it in 1798; under his engraving –

which was headed "GROVE FIELD, WARWICKSHIRE – THE DEER BARN" – he wrote:

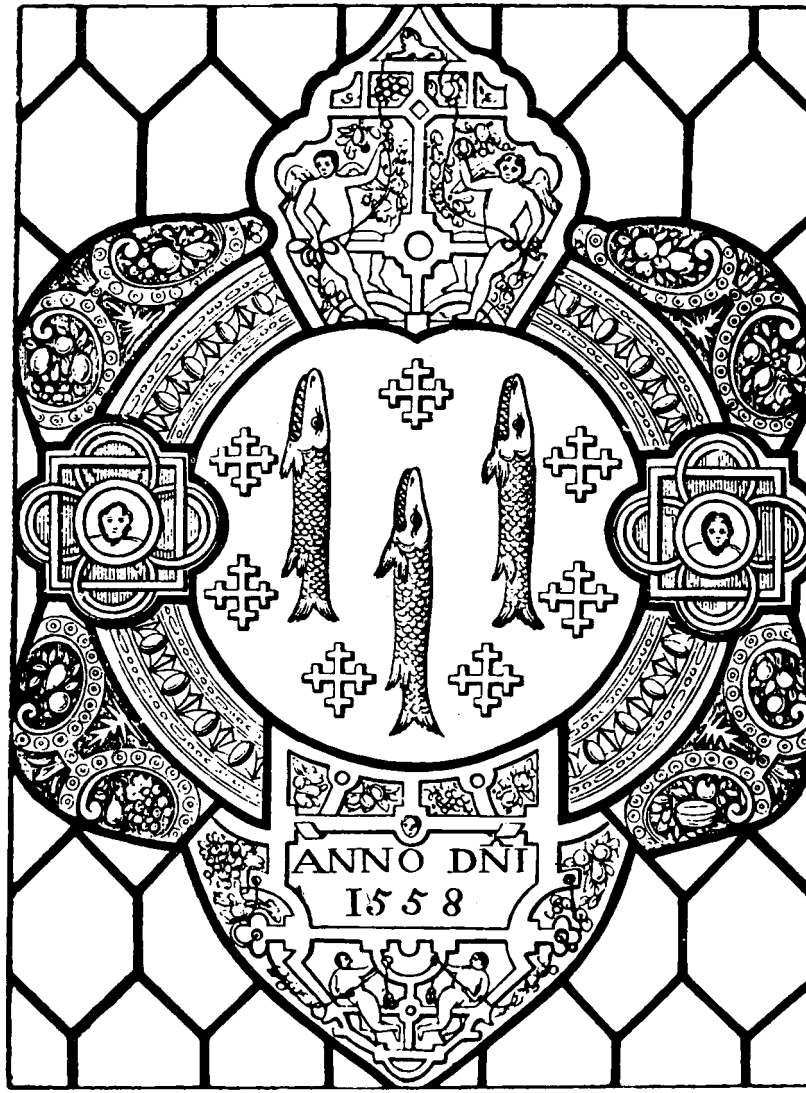
> "This barn was originally appropriate to the use of foddering the deer belonging to the park which formerly surrounded this place of antiquity. Here it was that the immortal Shakespeare secreted himself after he had stolen the deer. Charlecote is the name of the family seat of the Lucys, and there belongs to it a park well stocked with deer; but there remains a doubt in my mind whether it ever belonged to the park where this barn stands, as the river Avon runs close by the extremities of the present park, and that which originally [was a park] is now converted into farms."[10]

Jackson was confused about the location of the deer barn in relation to the park from which the deer were poached, but we will see later that this confusion was understandable given the number of parks that the Lucy family were linked with in the sixteenth century. According to the local historian of Stratford-on-Avon, R. B. Wheeler, writing at the beginning of the nineteenth century, "the uniformly believed opinion and tradition [in the locality] is that it [the poaching incident] happened at Charlecote."[11] This statement will be evaluated in discussion of a review of the historical and documentary evidence. There were other references to the deer-poaching tradition in the eighteenth and nineteenth centuries, but they add little to the accounts quoted above, with the exception of one unusual twist to the story quoted by Sir Richard Phillips in 1818: "At Stratford, the family maintain that Shakespeare stole Sir Thomas Lucy's buck, to celebrate his wedding day, and for that purpose only ..."[12]

We therefore see that there was a large body of local and oral evidence in favour of the poaching tradition, and given that some of the evidence is from independent sources, we would normally expect it to be widely accepted. Some scholars have rejected the tradition partly on the basis of the argument that it is simply an embellishment of Falstaff's raiding of Justice Shallow's park in *The Merry Wives Of Windsor*. But there are a number of flaws in this line of argument: there was a cony warren at Charlecote and although there is no mention of rabbit poaching in *The Merry Wives Of Windsor*, Davies mentions Shakespeare poaching rabbits as well as deer. Also Davies was unfamiliar with the details of *The Merry Wives Of Windsor* for he calls Shallow "Justice Clodplate"; his account of the story remained in manuscript form and was not known to Rowe when he wrote his version. Rowe derived his account from local tradition (this was collected by the Shakespearian actor, Betterton, who according to Rowe made a special journey into Warwickshire some time during the

second half of the seventeenth century to collect information on Shakespeare.) Where Rowe's information can be checked against independent documentary evidence it has turned out to be accurate – the major example of this being his statement that John Shakespeare was a dealer in wool.

The multiplicity of separate sources for the poaching story would in itself suggest that it was genuine. Sir Thomas Lucy was a Member of Parliament and in March 1585 had charge of a bill "for the preservation for grain and game" – which is certainly consistent with the traditional accounts of his reaction to the poaching of his deer. The association between Justice Shallow and Lucy is suggested by their similar coat of arms; Shallow had "a dozen white Luces", whereas Sir Thomas Lucy had three white luces (see Plate 1) – although on at least one occasion his coat is known to have been "quartered" [reminiscent of Slender's remark on Shallow's coat: "I may quarter (Coz)."], producing a dozen white luces.[13] Most scholars have been prepared to accept that the traditional testimony for the poaching story is strong, but the major difficulty in its acceptance has been the absence of any firm evidence for a deer park at Charlecote or Fulbrook at the relevant period. However, the lack of such evidence is largely due to the failure of Shakespeare scholars to take the poaching tradition seriously, and this evidence will now be considered in some detail.



WINDOW AT CHARLECOTE WITH LUCY ARMS

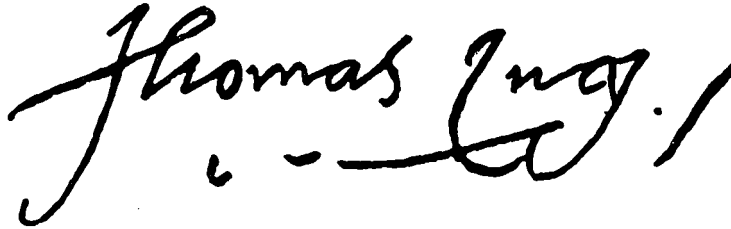


Plate 1: The Arms Of Sir Thomas Lucy (From Edgar I. Fripp, Shakespeare's Haunts New Stratford *[1929])*

CHAPTER 9:
SIR THOMAS LUCY'S DEER PARKS

The formal legal history of licensed deer parks only provides limited information on their actual existence. Jane Croom, the historian of medieval deer parks in Warwickshire, has summarised the position very succinctly for the period 1100-1530:

> "The Patent and Charter Rolls record royal licences to create parks, but only seven [out of a total of seventy-nine] were issued for Warwickshire, so for the majority of parks the actual date of emparking is not known, and we have only the date of the earliest reference . . . However, it seems unlikely that it was always necessary to obtain a royal licence to empark. Only 290 licences are known for the whole of England ..."[1]

The first mention of a park at Charlecote was in 1486, when Rous wrote his account of the enclosure and depopulation of villages in Warwickshire; he stated that almost all of Charlecote was emparked and that the population had fallen from forty-two people in the seventh year of the reign of Edward I to seven people in 1486.[2] In 1510 a Sir Thomas Lucy (Shakespeare's Sir Thomas Lucy's grandfather) owned "20 acres of wood, one park, several fisheries in the water of the Avon . . . in the Manor of Charlecote."[3] In the same year he was granted, as one of the King's sewers, the keepership of Fulbrook Park, and fifteen years aftwerwards in 1525 there is record of his ownership of "unus parcus".[4] Given his ownership of a park in Charlecote in 1510, this almost certainly again refers to Charlecote, and in June of 1525 his steward, Richard Cocks, wrote from Charlecote to Lucy's wife in London, sending her venison, adding that a "buck killed for Master Nethermill had an ill liver" and he "doubted of more".[5] In 1557, Shakespeare's Sir Thomas Lucy acquired the manors of Bishop's Hampton and Hatton, which included what had previously in 1549 been known as Hampton Park.[6] In 1594, legal note was made of the existence of a warren at Charlecote [7], whereas

in 1618 Sir Thomas Lucy's grandson obtained a patent to impale on his estates, " the *parcum vocatum* Charlecote Park *in Comitatu Warwicensi*" and "the *parcum vocatum* Sutton Park *in Comitatu Wigorniensi*"[8], the latter being a deer park in Worcestershire that was inherited by Lucy through marriage during the middle of the sixteenth century.

Although useful as backround, the legal history of the status of Sir Thomas Lucy's parks and warrens does not in itself answer the question that we are mainly interested in: the existence of a park/warren at the time of Shakespeare's alleged poaching activities – some time during the middle of the 1580's. The existence of a legal right does not mean that it was necessarily exercised, and conversely Elizabethans invariably ignored the law when it suited their convenience – witness John Shakespeare's moneylending and woolbroking activities. We therefore need to examine the evidence for the existence of a park/warren beyond the legal record in some detail. Our starting point must be the Lucy Household Accounts Book, which covers the period 1573-1587.[9] The first quarterly entry in 1573 (for the second quarter of the year) includes the following listing of names and wages paid:

"Tho. Eden keper at Char. warren -------- x s.
Jo. Morrysse, keper there lykwyse -------- xiii s. iiij d.
Wm Hulett, keper at Hatton -------- x s.
Jo. Rowbery, keper there also --------- vj s. viij d."

There was no keeper mentioned for Sutton, although there was a "balyfe", and by implication, from the sequence of the listing, a "keper of horse" and a "fawlconer" at that place. The next quarter's listing was identical, as was the final quarter except that Richard Lawson replaced John Morrysse as under-keeper at Charlecote and a second "ffawlkoner" was apparently added at Sutton. The same listing continued into the first quarter of 1574, with the addition of an extra falconer, which was also carried over into the second quarter – although by then, at least one of the three – John Gowlde – was working at Charlecote, and possibly one of them was working at Hatton. In the third quarter, both the keepers listed for Hatton disappeared, as did one of the falconers – leaving two keepers (Thomas Eden and Richard Lawson) and one falconer (John Gowlde) at Charlecote, and one falconer and a keeper of horse at Sutton. This pattern of just the two keepers at Charlecote was maintained until the third quarter of 1575, when for the first time a "keper of Sutton parke" was listed.

The pattern of two keepers at Charlecote and one at Sutton was then maintained until 1578, in which year William Hullett, who had been one of the keepers at Hatton, appeared as head keeper at Charlecote. In the following year the two Charlecote keepers disappeared and did not re-appear until three years later during the second quarter of 1582, when Sir Thomas Lucy began to keep his own account book, stating that he had "payd to hidgcox keaper of my conynove ------ £iij xv s." This very large wage – something like seven times the normal wage of an ordinary head keeper – was paid to Hidgcox until the third quarter of 1583, when he suddenly disappeared from the record. He had been the only keeper at Charlecote during this one-and-a-half year period, and probably was responsible for the construction of a completely new warren at Charlecote, given Sir Thomas Lucy's reference to a "conynove".

During this whole period – 1575-1583 – only one keeper was maintained at Sutton, and this appears to have continued right through to the end of the account book in 1587. After Hidgcox left Charlecote in 1583, there were no keepers listed there for the last quarter of that year and the first quarter of the following year. But in the second quarter of 1584, "xj s. viij d." was "payd to robert mathew my keeper" at Charlecote, who was replaced by "wylliam mathew" in the following quarter. In the final quarter of 1584 a second "cony keeper" (Thomas Reynolds) was added to the list for Charlecote, and the pattern of two keepers (they were invariably referred to as "my keeper" and "my other keeper") was maintained right through to the end of the record in 1587.

Although the account book tells us a great deal about what was happening at Charlecote and elsewhere, in some ways it raises more problems than it solves. In particular, there are the very puzzling references to the two keepers at Hatton during the first two years of the account in 1573 and 1574. In 1551, Sir Thomas Lucy's father, William Lucy, had owned the manor of Shrewley in the parish of Hatton, Warwickshire, but this manor had been sold to Clement Throckmorton by 1561.[10] However, Sir Thomas Lucy had acquired the manor of Hatton (also known as Hayton), along with that of Bishop's Hampton, both in the parish of Bishop Hampton (afterwards known as Hampton Lucy), in 1557.[11] There is some confusion over the exact whereabouts of the manor of Hatton, the author of the *Victorian County History of Warwickshire* article on Hatton, maintained that "Lands in 'a field called Grove Field' in the tenure of William Lucy are specifically mentioned in the grant of the manor to Thomas Lucy in 1557."[12] The same author went on to argue

that Hampton Woods was probably identical with Hampton Park, which had been owned during the medieval period by the Bishop of Worcester, and therefore this park adjoined Grovefield. He reached this conclusion partly on the basis that a forester had a holding at the Grove in the late twelfth century, suggesting that this was the location for Hampton Park.[13]

The reader will perhaps remember the earlier reference to William Jackson's drawing of the deer barn in Grovefield, in which it was alleged that Shakespeare hid. The existence of a deer park at this site – even a few years earlier than the probable date of the poaching incident – would obviously be of very great interest in assessing the poaching tradition. However, it appears that the *V.C.H.* author was incorrect in his conclusions, although it is a matter we must examine in some detail. In 1557 the Crown sold to Thomas Lucy for £989.19s.6d. the "reversion and reversions of the aforesaid manor and manors of Hampton Episcopi and Hatton" which had lately been seized from the Duke of Northumberland on his attainder and conviction for high treason. This grant was to include "all those lands tenements meadows feeding pastures and hereditaments with appurtenances severally lying in the aforesaid Hampton Episcopi in a certain field called Grovefield in the parish of Hampton ... being and in our hands through purchase . . . from Ralph Sadler Knight lately belonging to the advowson . . . to the Rectory and Parish Church of Hampton Episcopi."[14] It would appear from this that Grovefield was not a part of the manor of Hatton, in that it was purchased quite separately.

A listing was made in 1602 of the recently deceased Sir Thomas Lucy's lands, which further clarifies the position; a reference was made to "Grovefield except woods and lands called Hampton Wood", implying that the latter was a part of the former. In the same listing a description of the legal basis of ownership and value of the various lands was given as follows:

> "Manor of Hampton Episcopi from the Queen in Chief value clear per annum £20 & lands called Hayton alias Hatton & pastures and meadows of which they are ignorant [value per annum] £5.2.0. and lands called Grovefield from the Queen as of the manor of Warwick in common socage value clear per annum £5.2.8 and the advowson & parsonage of the Church of Hampron Episcopi and a hereditament called Hampton Wood from the Queen as of Manor of Warwick in common socage value clear per annum £5.15.3."[15]

This list clearly states that neither Grovefield nor Hampton Woods were a part of Hatton Manor, but on the contrary were part of the manor

of Warwick. This also destroys the linkage of Hampton Park with Hampton Woods, as the former was a part of the manor of Bishops Hampton, whereas the latter was a part of the manor of Warwick. The location of Hampton Park therefore remains something of a mystery. When it was transferred from the Bishop of Worcester to the Duke of Northumberland in 1549, it was still referred to as Hampton Park, but as a part of "the bishop's woods in Warwickshire called Hampton Parke and Busshe Woods."[16] It is possible that Hampton Park was located in Hatton, as the "descent of the manor of HATTON follows that of the main manor [of Hampton Lucy] throughout,"[17] and although Hampton Park is listed as being a part of the manor of Bishop's Hampton, it may be that there was some confusion over this. However, there is no indication of any deer park in Hatton at any point in its history – it is not mentioned for example in Croom's study of Warwickshire parks – and the likely solution lies elsewhere: in the highly complicated and obscure history of Fulbrook Park.

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The manor of Hatton was described in a special act of Parliament confirming title of certain of Sir Thomas Lucy's lands in 1585, as "all those closes pastures, meadowes and heredytamentes in the saide parishe of Bushoppes Hampton and commonly called of knowen by the name of or names of Hayton or Hatton Leasowes."[18] Although there is no description of the location of Hatton at this time, we are fortunate in having a very detailed survey and plan of the area for the year 1736.[19] This was prepared by James Fish, a surveyor from Warwick, and his work enables us to study in some detail the question not only the location of Hatton, but its relationship to neighbouring Fulbrook Park.(See Plate 2) Hatton stretched from Ox Close adjoining the river, across to Great Ground, south of Hobbins Ground, on the right-hand side. This latter part of Hatton was known as Blackhills Farm, and directly adjoined the area of Fulbrook designated by Fish as The Old Park. Long Close and Highway Close were a part of The Old Park, and were listed by Fish as "next Black Hills", and so Blackhill Farm in Hatton was directly adjacent to The Old Park in Fulbrook.

Fish locates "Fullbrook Parke" in the centre of the area designated by him as The Old Park – and this is exactly the location of Daisy Hill Farm, the lodge that Shakespeare is alleged to have been taken to after

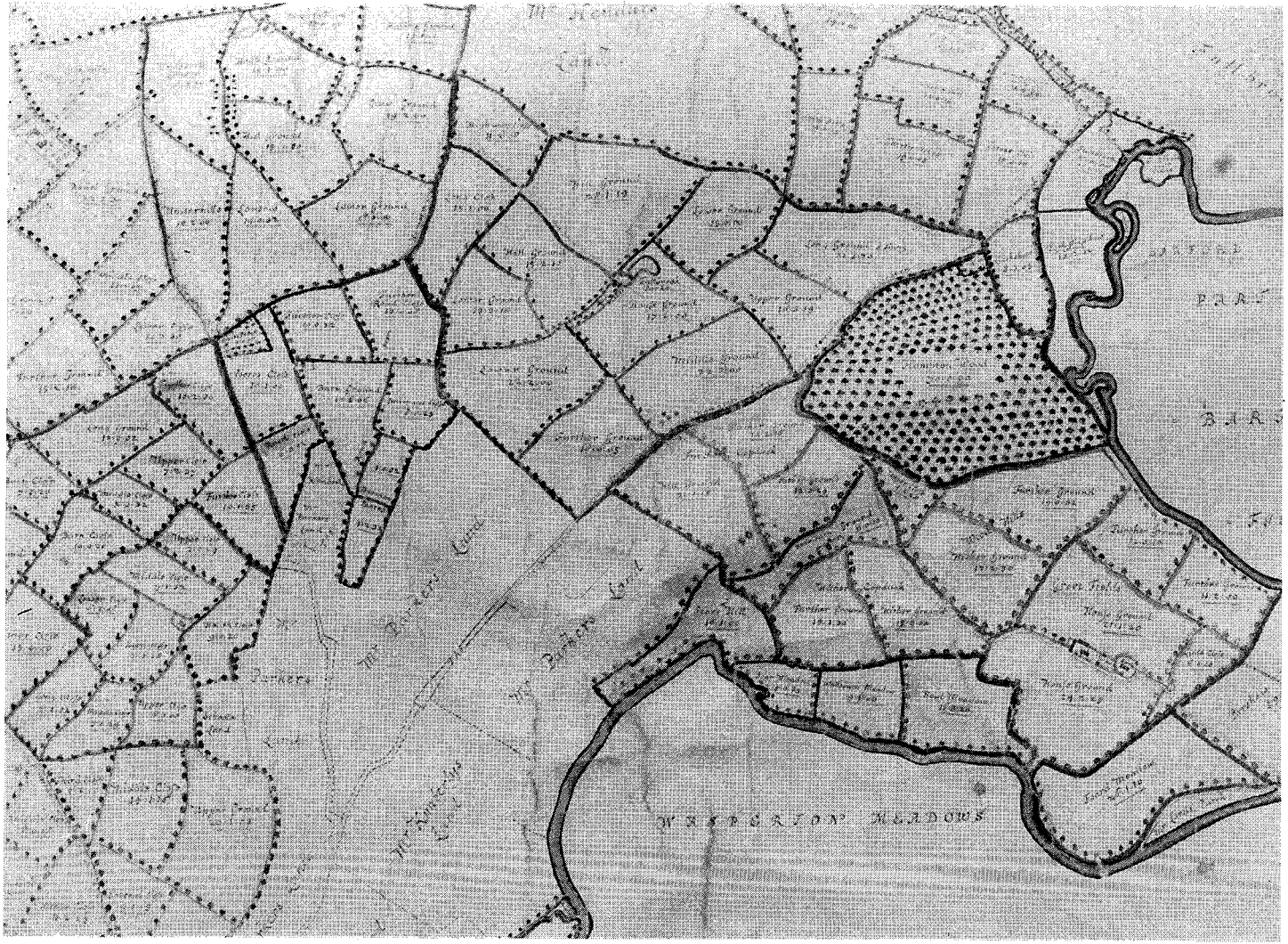

Plate 2: Fulbrook Park and Surrounding Area (From James Fish, The Survey Book of Charlecote etc *[1736] in Warwick C.R.O.)*

capture.(See Plate 2) But why should Sir Thomas Lucy place his keepers in an area adjacent to Fulbrook, and why did he withdraw them in 1574? In order to answer this question, we must now review what is known about the history of Fulbrook Park. The best introduction to the subject is the *The Victorian County History of Warwickshire's* summary:

> "According to Rous, the Warwick antiquary, the once celebrated park of Fulbrook, near Warwick, was inclosed early in the reign of Henry VI by John, Duke of Bedford, brother of Henry V. On the accession of Edward IV the manor and park passed to the crown, and . . . Henry VIII, at the beginning of his reign, granted the custody of the park to Thomas Lucy, to hold during pleasure. Leland, writing towards the end of this reign, mentions 'the fayre parke called Fulbroke', adding that 'the praty castle made of stone and bricks was an eyesore to the Earles that lay in Warwick castle, and was cause of displeasure between each Lord,' until Sir William Compton, as keeper of Fulbrook castle and park, seeing it go to ruin, helped it forward and took much of it to build his house at Compton Wyniates. In the days of James I the park, which had been purchased by Sir Thomas Lucy, was much enlarged."[20]

The author of another article on Fulbrook in the *Victorian County History* adds some additional detail to this account:

> "The boundaries of the . . . park inclosed by John, Duke of Bedford, [in] about 1421 . . . [are] known to have extended from the river up to the Stratford [to Warwick] road and Rous [writing before 1486] complains bitterly that the inclosing of the park converted a formerly safe highway into a notorious haunt of robbers, who lay in wait for their victims behind the newly erected palings."[21]

After the park passed to the crown, its rights of keepership were granted to a number of different favourites of the king; as we have previously seen, Thomas Lucy, one of the king's sewers, was appointed keeper of the park "during pleasure" in the year 1510.[22] Sixteen years later, in 1526, it was granted to William Corpson, one of the Yeoman of the Guard [23]; and in 1534, it was granted to Thomas Ogle, one of the Riders of the King's Horse, as a reversion on Corpson's death.[24] In the late 1530's, Leland gave his account of a visit to Fulbrook as follows: "I roade from Warwicke to Bareford Bridge of 8 fayre arches a 2 miles [from Warwicke]. Here I sawe halfe a mile lower upon Avon on the right ripe by northe a fayr parke caullyd Fulbroke."[25]

So when it was bought by John Dudley, Duke of Northumberland from the crown in 1547[26], it was still "a fayr parke". In 1557 it was said that "the premisses before the late Duke of Northumberland's time were 'A parke replenyshid with deare and by him disparked and all the woods sold and

the pale taken awaye.' "[27] On the attainder of the Duke of Northumberland in 1553, Fulbrook was granted to the Catholic Sir Francis Englefield by the new Queen Mary; Englefield was appointed Keeper of Fulbrook Park for life, "with all fees and profits pertaining to that office and profits and revenues of Manor of Fulbrooke."[28] On the 22nd December 1557, Englefield bought Fulbrook, along with lands in Cholsey, Berkshire, for £254.15s.; and he was granted: "... the reversions of the said capital mansion and lands in Cholsey and of the said herbage and pannage of Fulbrooke Park . . . lands called 'Fulbroke Parke' in Fulbroke, co. Warwick, and divers lands (*described*) within or without the bounds of the said park . . . now disparked . . . Yearly value . . . £14.12s.6d. . . . [and for Englefield] To hold [Cholsey and] Fulbroke Park in chief by the service of one- fortieth part of a knight's fee ..."[29]

With the accession of Elizabeth, Englefield's Catholic sympathies brought him into immediate conflict with the crown, and there ensued a series of confused and complicated events which threw title of possession of Fulbrook into great doubt. The *Dictionary of National Biography* gives a summary of events which provides a useful background:

> "Sir Francis Englefield . . . Being a firm adherent of the catholic religion, he fled abroad in 1559, soon after the accession of Elizabeth, and retired to Valladolid. His lands and goods were seized to the queen's use in consequence of his disobedience in not coming home after the queen's revocation, and for consorting with her enemies . . . Subsequently he was attainted and convicted of high treason in parliament on 29 Oct.1585, and all his manors, lands and vast possessions were declared to be forfeited to the crown . . . Englefield had however, by indenture dated in the eighteenth year of the queen's reign (1575-6), settled his manor and estate of Englefield on Francis, his nephew, with power notwithstanding of revoking the grant if he should deliver or tender a gold ring to his nephew. Various disputes and points of law arose as to whether the Englefield estate was forfeited to the queen. After protracted discussions in the law courts the question remained undecided, and accordingly the queen in the ensuing parliament (35th Eliz.) had a special statute passed to confirm the attainder and to establish the forfeiture to herself. After tendering by her agents a ring to Englefield, the nephew, she seized and confiscated the property . . . alienated and transferred to the crown."[30]

In fact, the above is a somewhat simplified account of what happened to Englefield's property. The crown had seized Englefield's lands in Berkshire and Wiltshire in 1563, but had allowed his wife, Catherine, the income from some of this property, as well as allowing Englefield's sister-in-law Margaret (wife to his brother John) £40 a year for maintenance.

Englefield himself had been allowed to keep £300 a year and more, which had been frozen to £300 in 1571 because of his continuing refusal to return to England.[31] (The seizure of lands on the one hand and yet allowance of income on the other, was all part of a long political struggle which took place between Elizabeth and the Catholic opposition.) The position on Fulbrook was described on the 7th October 1573 in a commission, conducted by Sir Thomas Lucy and others, "to enquire concerning the death of Francis Englefield overseas":

> "Inrots say that Francis Englefield was seized as of fee of 1 messuage 20 acres of meadow 350 acres pasture and 100 acres woods called Fulbroke Park in Fulbrook in Warwickshire value £40 per annum . . . by an indenture of 6 May 1 Elizabeth granted the same to Edward Fytton Ralph Egerton Kt John Mayor Esq John Corbet gent and Richard Bosne yeoman to be held for the use of Francis Englefield during the life of John Englefield Esq deceased the brother of Francis and after the death of John for the use of Margaret his wife, sister of Edward Fytton for her life and after her death for the use of Francis Englefield and his heirs . . . Francis had licence to stay overseas for 2 years but stayed after that time and John Engerfield died 3 April 9 Elizabeth . . . and Margaret is alive at Englefield in Berkshire and William Compson is in occupation of the premises at Fulbroke for an unknown term ..."[32]

In fact Sir Francis Englefield did not die until about 1596 (this "mistake" about his death may or may not be significant), and on the face of it, the facts appear fairly straightforward. The crown had allowed Englefield's friends to act as trustees to ensure that the rental income from the property was used for Englefield himself during his brother's lifetime, and then for his sister-in-law on his brother's death. The land was presumably under lease to William Compson for an unknown term, and if this were the case, he would pay the £40 a year rent which would meet Margaret Englefield's annuity. In 1572 the crown began directly collecting the rent from Fulbrook, and this may have represented yet a further hardening of the relationship between the crown and Englefield.[33] We know how the earlier arrangement worked through a complaint that Lady Englefield made about another property of hers that was let under lease; in about 1576, she gave her version of what had happened to the Englefield property at Compton, Berkshire, which had been leased to one Stafford. Lady Englefield complained that although Stafford had obtained a lease on the property for £30 a year, paying the rent to her friends, he had "much absented himself . . . whereby the house ran much to decay" (having his own main residence elsewhere in

the county) and "he also let out the arable land and grounds to others, to the spoil thereof."[34] Presumably a similar arrangement operated at Fulbrook, i.e. the estate was let out on lease and then possibly sub-let to a third party; but in its case, the crown was responsible for collecting rent after 1571. All the more surprising then, to find the following entry in the Calendar of Patent Rolls for 27 April 1573: "Licence for Edward Graunt to alienate lands in Fulbroke, co.Warwick, to Thomas Lucy, Knight, John Somervyle and Henry Rogers to the use of Edward and Anne his wife and his heirs and assigns. For 32s. in the Hanaper." This licence is so important that we must quote it in some detail:

> "The Queen to all to whom etc. Greeting . . . Know that we of our special grace and for thirty shillings paid into our hanaper, granted and gave licence . . . to our beloved Edward Grant, gentleman, that he one messuage, twenty acres of land, forty acres of meadow, three hundred acres of pasture and ten acres of woodland, with appurtenances in Fulbrook in our County of Warwick which are held from us in chief so that he can give and grant, alienate, enfeoff or take cognizance of by fine or by recovery in our court of our Justice of the Bench at Westminster or any other way whatsoever at his will, to beloved and faithful Thomas Lucy Knight and our beloved John Somerville, Esquire and Henry Rogers, Gentleman ..."[35]

There was a total of 370 acres included in this licence. It is not known whether it was activated: a search of the Feet of Fines for Warwickshire has not turned up a confirmation of the sale, although when Grant died in 1596, there was no mention of Fulbrook in the post-mortem inquiry – presumptive evidence that the land had been transferred out of his hands. Grant owned an estate at Northbrook in the parish of Fulbrook, but an examination of the relevant estate papers reveals that Northbrook contained only 179 acres, and even if we add extra land owned by Grant at Brierly in Fulbrook, this only gives a total of about 220 acres – 150 acres short of the amount described in the licence.[36] Fulbrook Park was described in an inquisition on the land carried out by Sir Thomas Lucy and others on 19 June 1573, as "1 messuage, 20 acres of meadow, 350 acres of pasture and 1 acre of woodland"[37]: 371 acres of land – nearly identical to the 370 acres described in the licence. There is therefore no doubt that the licence was referring to a transfer of Fulbrook Park to Sir Thomas Lucy and others. However, there are certain features of these transactions which are very puzzling: for example, why was Edward Grant's name not mentioned in the October 1573 inquiry? A part of the solution to this puzzle might lie in the fact that Edward Grant was a member of a prominent Catholic family, and may have been acting in

concert with the Englefields to protect their land. He was certainly capable of acting outside of the law: he is the same Edward Grant who sold wool illegally to John Shakespeare two years previously, and his family later became involved in the Somerville conspiracy to kill the Queen. The *Victorian County History* describes Northbrook as follows:

> "At Northbrook . . . [is] the . . . manor house of the Grants, a centre of Catholic disaffection in the reigns of Elizabeth and James I. Edward Grant of Northbrook was described in 1564 as 'an adversary of true religion'. He married Anne Somerville of Edstone and the house was searched in November 1583 after the discovery of his nephew John Somerville's plot against the Queen. His grandson was drawn into the Gunpowder Plot . . . Northbrook was the scene of frequent meetings of the conspirators during 1605."[38]

Edward Grant could have acted in concert with the Englefields to prevent the crown from seizing their land. When Sir Francis Englefield's nephew and heir, Francis Englefield, was examined by Sir John Perrott about his uncle's lands, and "willed to tell the truth, he said the truth is not to be told at all times."[39] The Englefields had tried to save their lands through internal transfer; these evasive devices were sufficiently successful to force the crown to resort to special act of parliament. But although it is credible to believe that Edward Grant acted in concert with the Englefields, it is difficult to see how an ardent Protestant like Sir Thomas Lucy would have done so. However, the partners in the proposed purchase of Fulbrook in the 1573 licence were John Somerville and Henry Rogers – and John Somerville was one of Warwickshire's most prominent Catholics (it was his son John Sommerville who was later executed for plotting against the Queen.[40]) Lucy was clearly friendly with Somerville – he appeared on the commission post-mortem on his lands when he died in 1578[41] – and it is possible that local gentry families acted in concert to protect their lands when threatened by outside forces, like the crown. However, the licence quite clearly referred to Fulbrook being held by Grant from the crown, and therefore there is no direct evidence of any concealment in these arrangements. One alternative explanation of the anomaly of dates, is that there was a mistake in the date of the 1573 licence, and that this was issued after 1574/1575 when the crown stopped collecting rent from Fulbrook. This puzzle can only be settled through future research.

The other party to the proposed purchase of Fulbrook – Henry Rogers – is of great interest in his own right. Rogers was a lawyer and not only was

he Sir Thomas Lucy's steward but he was listed in Lucy's account book for the year 1580 as "ffyrst retayner"; he was also town clerk and steward to the Stratford Corporation for the period 1570-86[42] – covering John Shakespeare's time of high office. Additionally, Rogers was one of John Shakespeare's business partners[43], and therefore formed a direct link between Sir Thomas Lucy, and John Shakespeare's son, William. (He has the additional interest for Shakespeare scholars of being the coroner at Katherine Hamlet's inquest – she was drowned in 1579 and may have been a model for Ophelia.) But the web of connections ran even further; Fripp, writing of the Sommerville plot in 1583, tells us:

> "On 25 October [1583] John Somerville of Edstone, a young squire, late at Oxford, now married Margaret Arden, a kinswoman of Shakespeare's mother, daughter of Edward and Mary Arden of Park hall, fired by the fanaticism of the Ardens and their priest, Hugh Hall, set out from his house, six miles north of Stratford, for London, with the intention of shooting the heretical Queen. He was arrested next day . . . [and] conveyed to the Tower . . . on the 2 November the Clerk of the Privy Council, Thomas Wilkes, arrived at Charlecote and took Master and Mistress Arden prisoners. Thence, with Lucy . . . he proceeded to Edward Grant's house, Northbrook, in Snitterfield . . . Henry Rogers, who lived in Sherburn, Town Clerk of Stratford and agent of Sir Thomas Lucy, assisted the latter and Wilkes in their search for incriminating 'books and writings'."[44]

Fripp was wrong in stating that Northbrook was in Snitterfield: although adjoining it, it was in the parish of Fulbrook. It is highly ironic that having had a business connection with Grant regarding the purchase of Fulbrook, Lucy and Rogers should have raided his Northbrook house in search of incriminating Catholic literature: particularly as their business partner – John Sommerville – was the father of the main culprit in the conspiracy. This web of connections can only be clarified through further work.

Given that Lucy was listed as one of the purchasers of Fulbrook in the 1573 licence, and his family's involvement with the park, it is probable that he did acquire an interest in the estate. There is however, uncertainty as to what happened to Fulbrook after the 1570's. The crown appear to have ceased collecting rent from the estate after about 1575, although they continued to collect the rents from other land owned by Englefield.[45] A special act of parliament was passed in 1593, "An Acte for explanation and confirmaytion of the Queens Maiestes title to the lands and tenements late Sir Francis Englefields Knight attainted for high treason", and included in the confirmed lands was "the reversion and remainder of

the parke called Fullbroke parke in the sayde Countie of Warwike."[46] The phrase "reversion and remainder" may refer to Margaret Englefield's interest in the property, for when it was leased to Nicholas Ffaunt, one of the Clerks of the Signet, in 1607, it was stated that the lease was "to begin after the death of Margaret Englefield who hath an estate in the said lands for term of her life."[47]

However, when the land was sold in 1610 to Sir William Willoughby and William Brock by the crown, it was sold in fee simple, but, "on composition for defective title".[48] What the basis of this "defective title" was, is unknown; it may refer to the chequered entanglements between the Englefields and the crown, but it also might relate to other transactions – such as that in the 1573 licence – involving the land previous to its sale in 1610. The Englefields had contested the right of the crown to their lands through the device of internal transfer, and this would have affected any grants the crown made of these lands – including the transfer of Fulbrook to Sir Thomas Lucy – before the special act of parliament in 1593.

There is evidence that Fulbrook was re-emparked at some date before 1588 (this will be discussed in the next chapter), and this seems to have taken place during the year 1573. In 1557, Fulbrook was described as "now disparked", with "all the woods sold and the pale taken awaye"; it consisted of 21 acres of meadow, 80 acres of pasture, and 400 acres of heath and wasteland.[49] In the inquisition of 19 June 1573, it was described as consisting of "1 messuage, 20 acres of meadow, 350 acres of pasture and 1 acre of woodland there called Fulbrook Park at £10 p.a.", whereas four months later on the 17th October it had expanded to "one messuage, twenty acres of meadow, 350 acres of pasture and 100 hundred acres woods called Fulbroke Park . . . value £40 per annum."[50] This latter description was used in all legal transactions right up to that used for the sale of the property to Willoughby and Brock in 1610, and the inclusion of woods in the property and the evidence to be considered in the next chapter, suggest it was being used as a deer park throughout most of this period. Sir Thomas Lucy's withdrawal of his two keepers from Hatton in the middle of 1574, indicates that the deer from Fulbrook ceased to roam over neighbouring ground, presumably because the estate had been re-emparked. (This might also explain his interest in acquiring the estate in April 1573.)

Fulbrook was only a mile or so away from Snitterfield, as was Ingon – both places with direct Shakespeare connections. Shakespeare's uncle,

Henry, farmed land at both Snitterfield and Ingon (his children were baptised in Hampton Lucy church), and John Shakepseare had in 1570 leased fourteen acres of land in Ingon, possibly that farmed by his brother Henry. But all these facts are irrelevant unless we can establish that there were deer parks at Fulbrook and Charlecote during the period that Shakespeare is alleged to have poached deer from Sir Thomas Lucy. What we need is a map – a map which shows deer parks in the mid-1580's – and to our great good fortune, such a map does exist, giving detail of parks and the area in which they were set.

CHAPTER 10:
THE DEER PARK AND CONY WARREN AT CHARLECOTE

At some time in the middle of the sixteenth century, William Sheldon, a prosperous landowner in Warwickshire and Worcestershire, set up a tapestry-weaving industry on his estates at Beoley and Barcheston in Warwickshire.[1] In 1570, Sheldon left the weaving business to his son Ralph, along with money and grants to continue it. In Sheldon's will, there is mention of Richard Hicks, his manager at Barcheston, who may well have been trained in tapestry weaving and design in the Low Countries.

In 1588, Ralph Sheldon built himself a new mansion at Weston, near Long Compton in Warwickshire, and commissioned a set of four tapestry maps covering the counties of Warwickshire, Worcestershire, Oxfordshire with Berkshire, and Gloucestershire. According to the seventeenth century antiquary, Anthony Wood, these hung in the dining room of Weston, and were probably woven for Ralph Sheldon's new house. Not all the maps in this original series have survived, and a second set were woven some time after 1647 (one of them included the arms of Ralph Sheldon the younger impaling those of his wife whom he married in 1647), probably to replace the worn first set. In this second set was included a map of Warwickshire, bearing the date 1588; it was obviously not woven at this date because it incorporated Ralph Sheldon the younger's coat of arms, and a mid-seventeenth century style of borders. But given that the cartographical portions of the maps common to both series are identical, and that the second set all bear the date of 1588, it is clear that the second set is a copy of the first.

In fact, both editions of the map of Worcestershire have survived and are currently hanging in the *Victoria and Albert Museum*. This particular

map includes that part of Warwickshire covering the Charlecote/ Fulbrook area, and the map of the first series, although not dated, has the name of Richard Hicks as its designer, and has a typical Elizabethan border for its surrounds. This map, although not complete, is in excellent condition where it has survived, and unlike the Warwickshire map itself, is laid out on a correct geographical basis, i.e. it was designed and woven so that north faced north, east – east etc. And this map seems remarkably accurate as far as distances and locations are concerned, as the reader will see from comparing Plate 3 with Plate 4. As Humphreys has observed in his book on the Sheldon tapestries, "the tapestry maps . . . suggest a personal knowledge of the county by the designer of each map, for usually the churches are correctly represented, with or without the spire."[2] This can be seen for example in Stratford : the two churches of that town are very accurately portrayed (see Plates 5 and 6). Barcheston, where the map was woven, is about ten miles from Charlecote, and there was a particular reason why the Sheldon weavers would have been familiar with Charlecote: William Sheldon had leased "the manors of Hampton Lucy and Hatton" from Sir Thomas Lucy, and the lease had only been re-assigned back to Lucy in 1573.[3]

The Sheldon tapestry map of Worcestershire bears very close scrutiny. The reader will notice that there is a paling attached to Charlecote, and that this is bounded on one side by the river Avon. What is not clear from a first reading of this map is the location of Fulbrook Park; there appears to be a park at "Wasburton" just east of Snitterfield, but no sign of Fulbrook. In fact what has happened is that the tapestry weavers took the location of Wasperton (Wasburton) from Saxton's 1576 map of Warwickshire, as did a number of subsequent mapmakers – rather than on the left-hand side of the river it should be on the right. (See Plate 4) Given that the rest of the Sheldon map is so accurate in detail – for example both the churches at Bishops Hampton and Wasperton are correctly depicted – it is surprising that this mistake was made. Saxton's map had such authority with contemporaries, that the Sheldon mapmakers preferred to believe that rather than the evidence of their own eyes.

If Wasperton is relocated in its correct position on the right side of the river, a park emerges exactly where Fulbrook was located. (See Plate 7) The Warwick to Stratford road runs alongside its paling, where Rous's robbers lay in waiting, and it is precisely where it should be according to the various descriptions of its location already discussed – in particular Leland's account already quoted : "I roade from Warwicke to Bareford

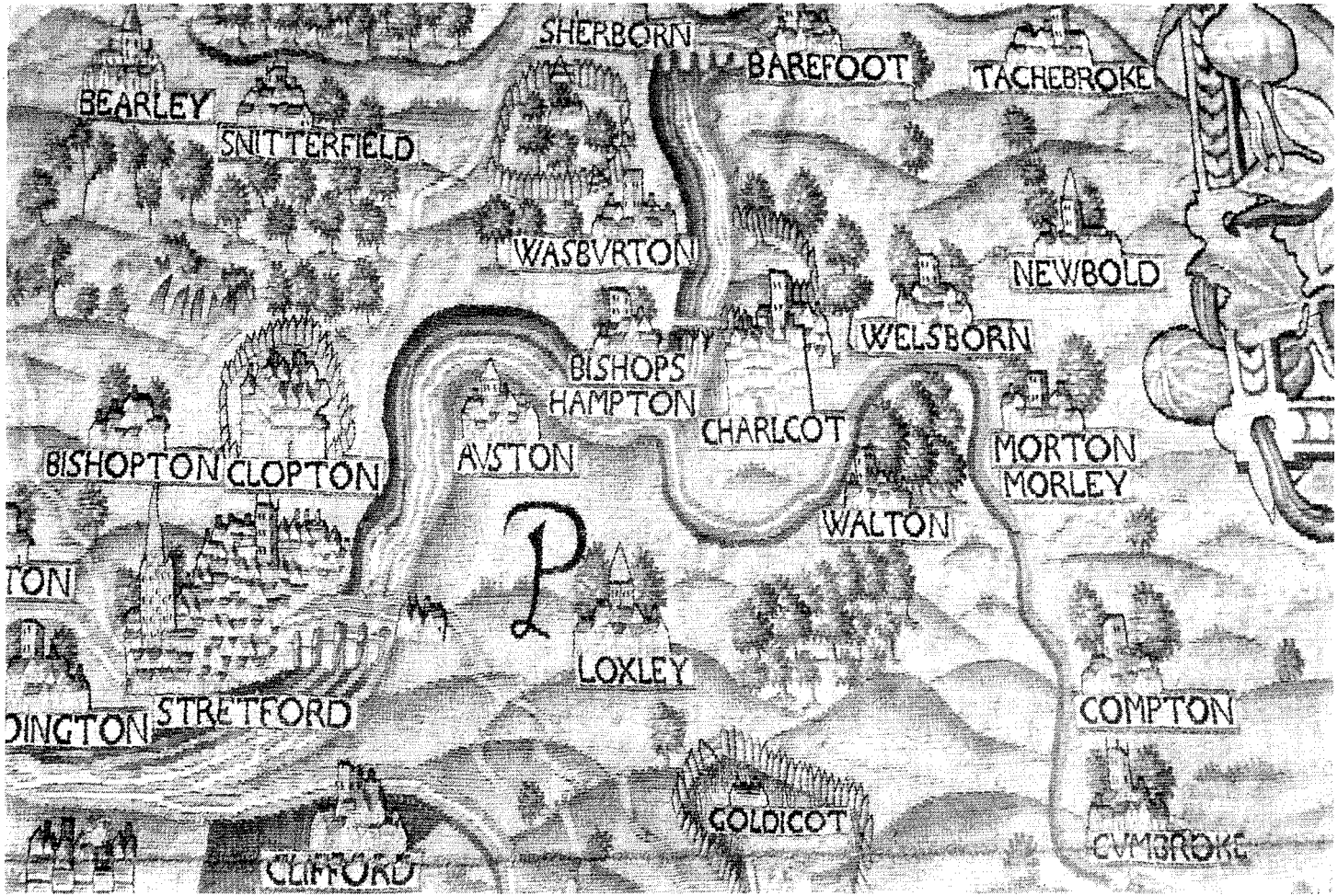

Plate 3: Charlecote and Surrounding Area (From Sheldon Tapestry Map of Worcestershire in The Victoria and Albert Museum)

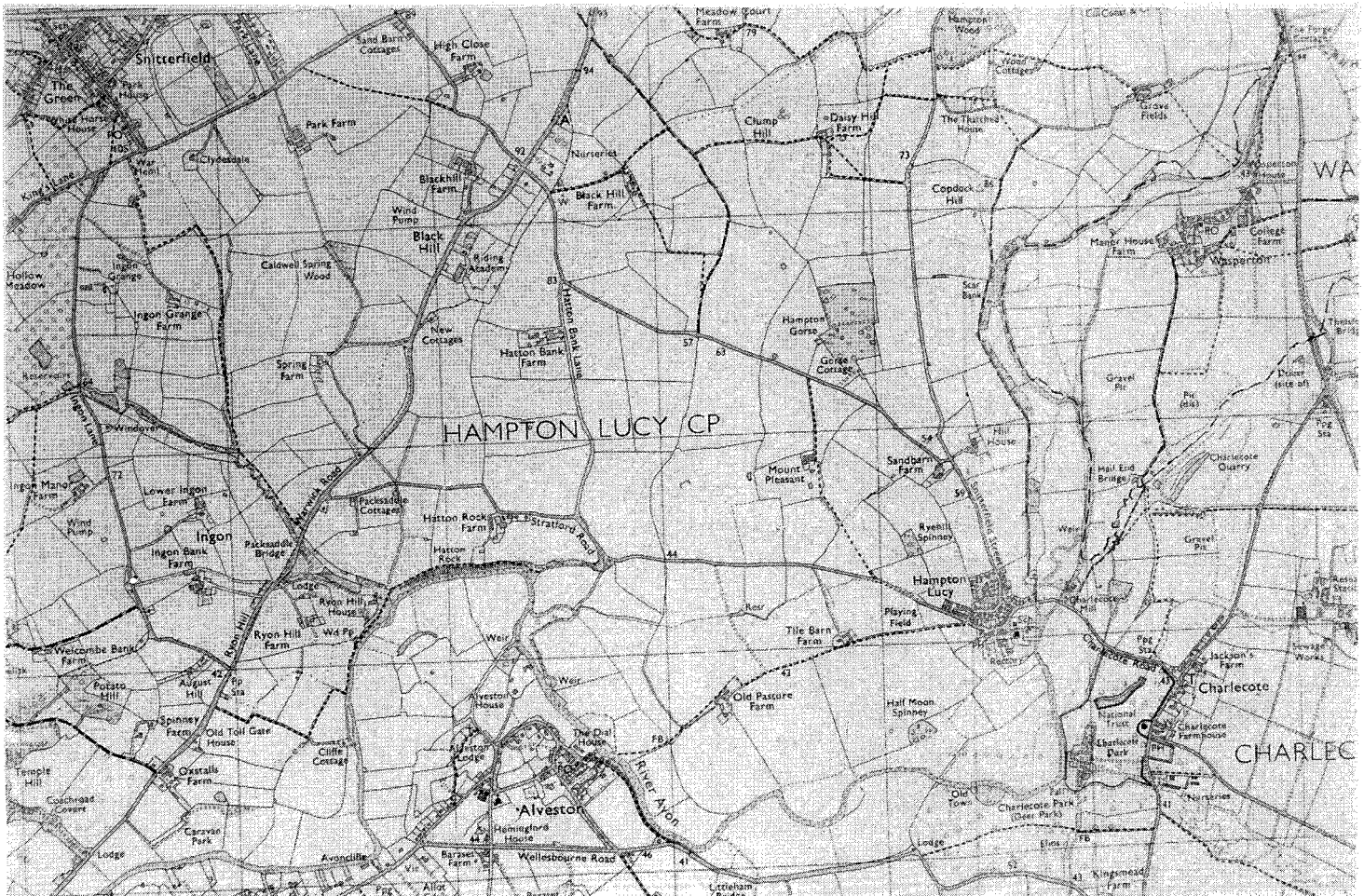

Plate 4: Charlecote and Surrounding Area, Ordnance Survey Map

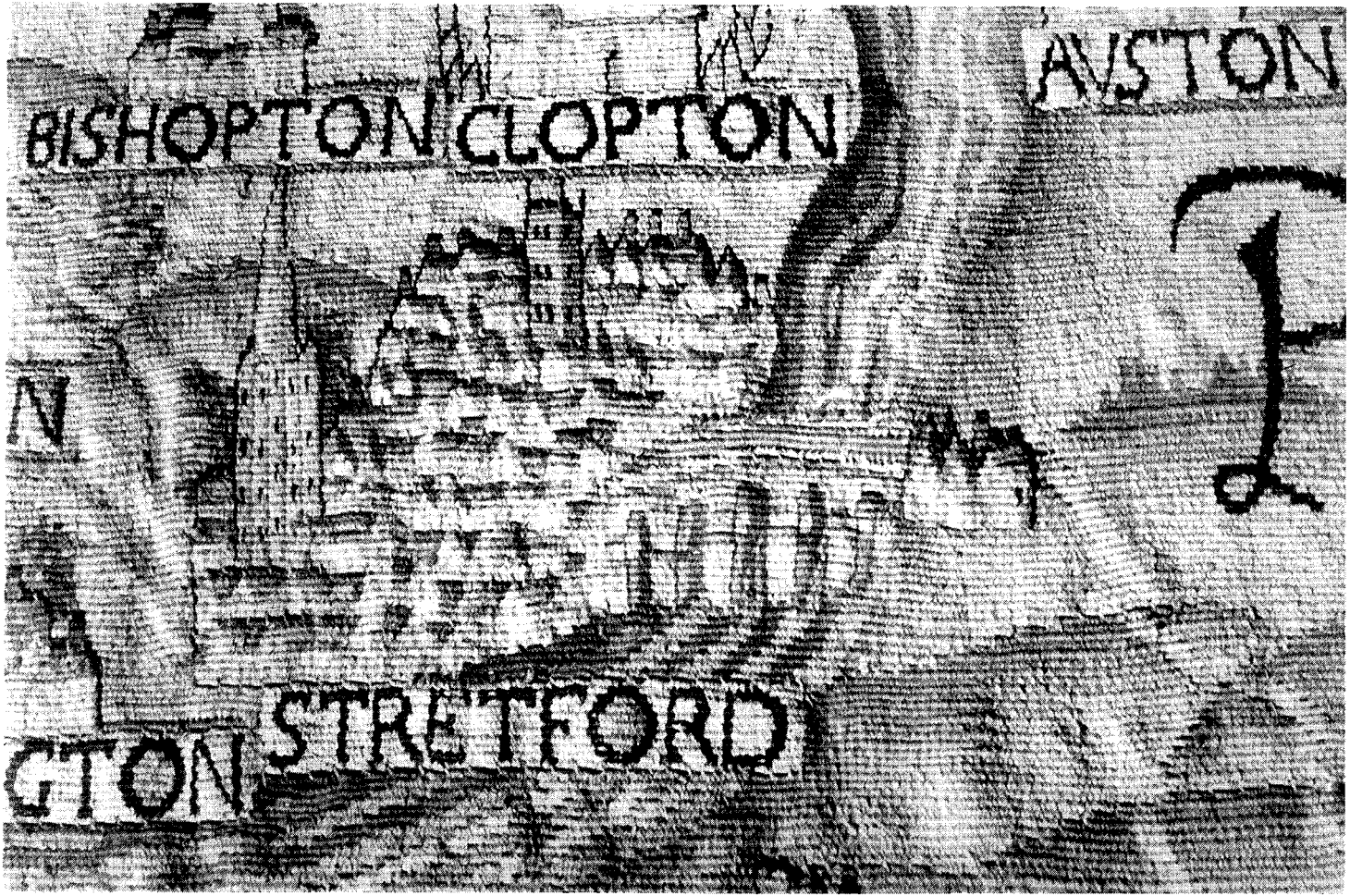

Plate 5: Stratford-On-Avon (From Sheldon Tapestry Map of Worcestershire in The Victoria and Albert Museum)

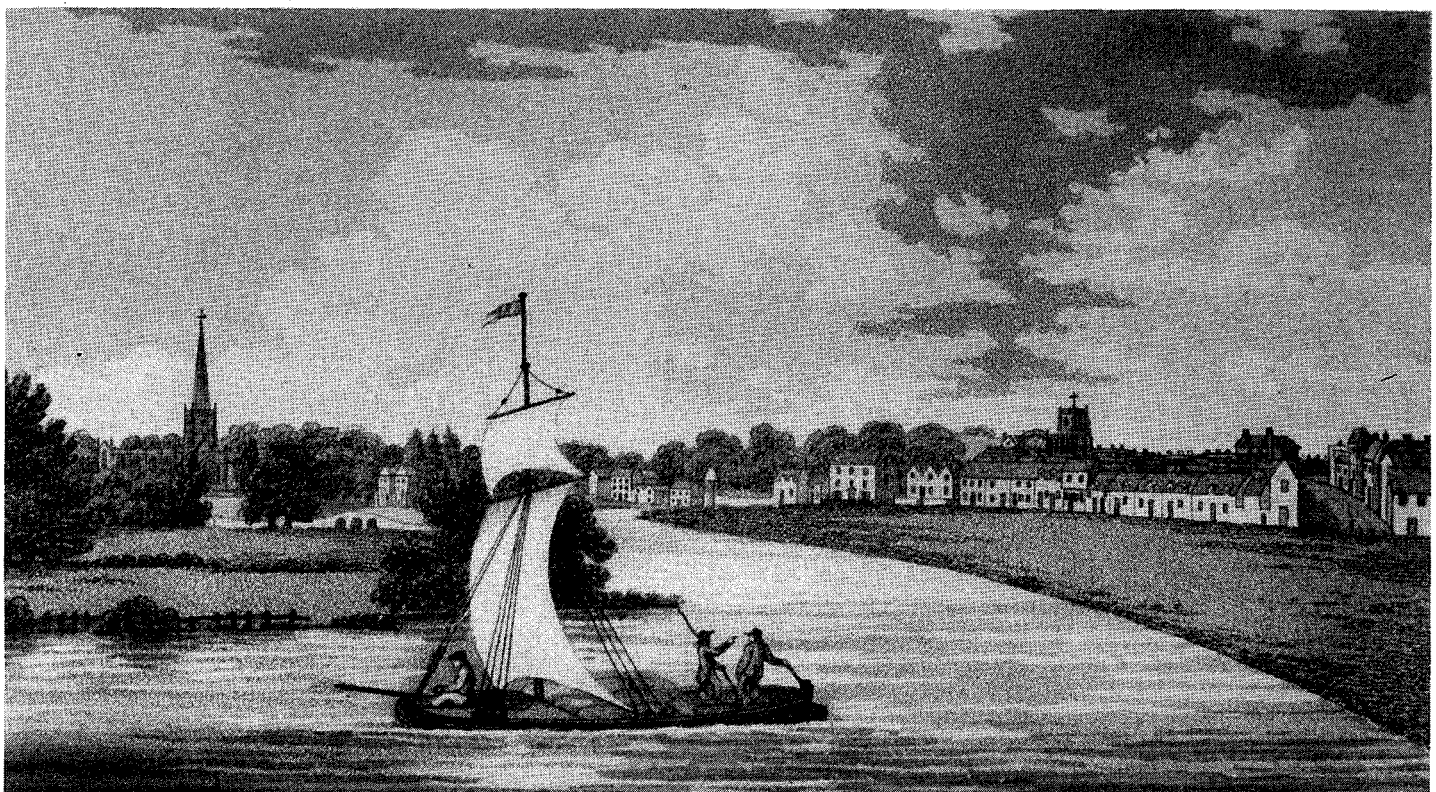

Plate 6: "View of Stratford-On-Avon" (From R. B. Wheeler, History and Antiquities of Stratford-On-Avon *[1806]).*

Plate 7: Fulbrook Park (From Sheldon Tapestry Map of Worcestershire in The Victoria and Albert Museum)

Bridge of 8 fayre arches a 2 miles [from Warwicke]. Here I sawe halfe a mile lower upon Avon on the right ripe by northe a fayr parke caulled Fulbroke." Also the detailed map of the area drawn up by Fish and and reproduced in Plate 2 confirms the identity of this park as Fulbrook. And so in 1588, about the time that Shakepeare is said to have poached deer and rabbits from Charlecote and deer from Fulbrook, we have documented evidence for the existence of parks in those places. The evidence for Fulbrook has been considered in some detail, and all we need to note at this point is the Sheldon map's confirmation of the proximity of the park to Snitterfield and Ingon where Shakespeare's Uncle Henry farmed land.

The picture for Charlecote is more complicated and I hope to be able to show that it was highly likely that there were deer in Sir Thomas Lucy's Charlecote park. The first thing to note is that the paling at Charlecote ran from the bridge at Bishops Hampton (Hampton Lucy) to Charlecote House, and then round in a semi-circle for about a mile up to the river. (See Plate 8) Bracebridge in his book about Shakespeare and his deer-stealing activites (written in 1862), tells us that "Sir Thomas Lucy, who in 1558 rebuilt the manor house of Charlecote as it now stands, imparked a considerable tract around it, on the left bank of the Avon in 1563 ...[which] ran along the bank of the Avon for nearly a mile."[4] This description fits perfectly with the Sheldon map – the Charlecote paling is shown ending just opposite Wasperton, which is about a mile north of Hampton Lucy. The only scholar to previously notice the significance of this paling was Fripp. Unfortunately he relied on the copy of the less accurate Warwickshire map, which shows a paling between the Avon and the Dene, a stream running between Charlecote and the village of Wellesbourne. He observed:

> "In the interesting tapestry-map of Warwickshire, woven at Barcheston in 1588, all that is visible of a park at Charlecote is on the left bank of the Avon, marked by a paling on the north-east of the house connecting the Avon with the Dene, the stream from Wellesbourne. Such an enclosure might serve for mares and foals – not for deer, which would soon be over the water inflicting damage on crops and pasture."[5]

Fripp did not seem to be aware of the Worcestershire map which showed the area in more accurate geographical detail; if he had examined this carefully, and walked over the territory opposite the Charlecote enclosure, he would have observed "Scar Bank" – a ridge running from Hampton Lucy bridge, right up to the bend in the river adjoining Grove

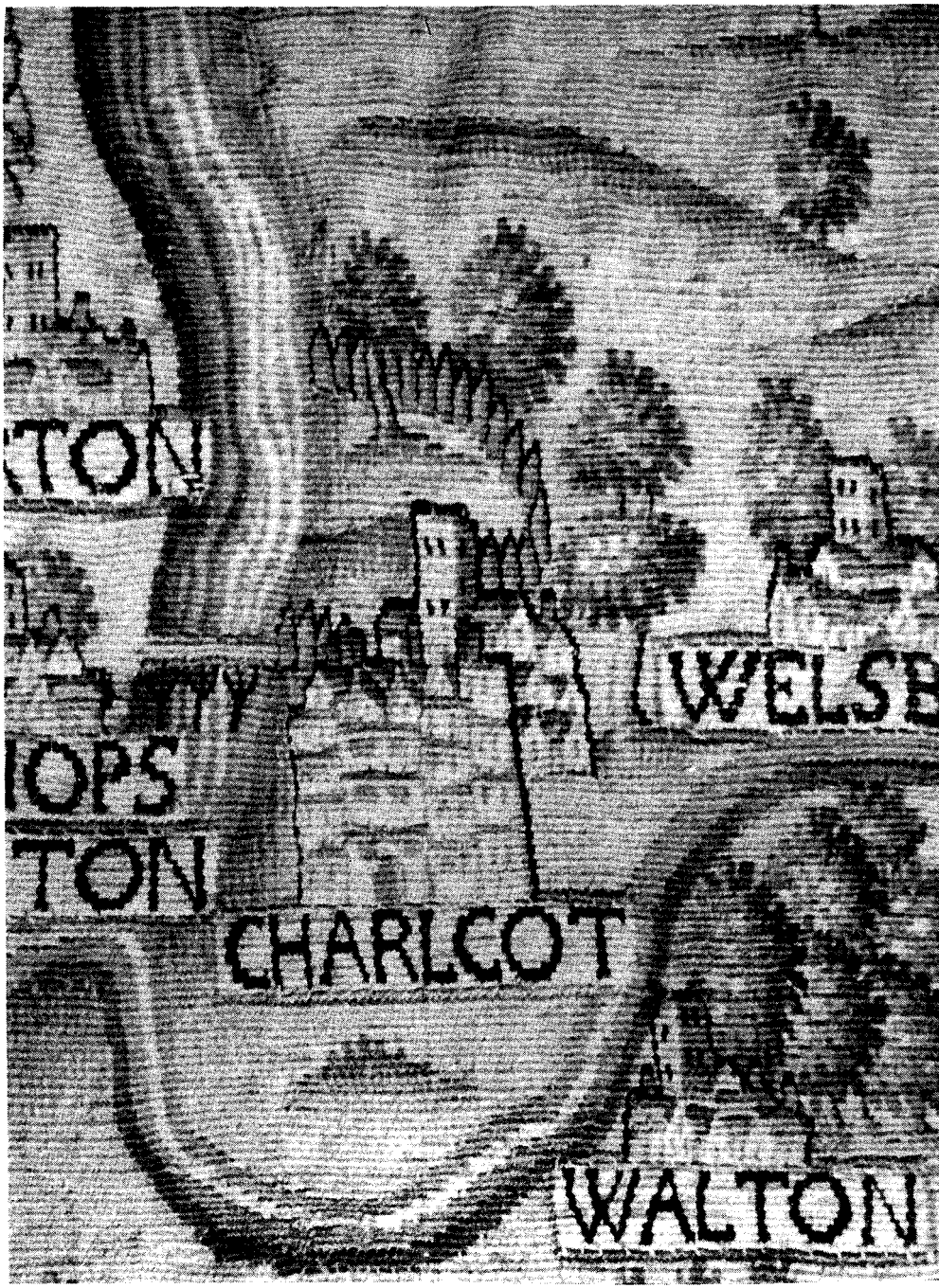

Plate 8: Charlecote Park (From Sheldon Tapestry Map of Worcestershire in The Victoria and Albert Museum)

Field. (See Plate 9). This bank forms a natural barrier on the other side of the river, and is fronted on both sides of the river by ground that even today is likely to flood, and probably in Shakespeare's day was largely marshland. Croom has observed of such a habitat: "Where the local topography allowed, natural boundaries such as a river or marshy ground might circumscribe the park."[6] In fact, the Worcestershire map shows a number of parks using a river as a part of its boundary: see for example the park at Strensham in Plate 10.

The river at Charlecote would have functioned partly to keep poachers out of the park, but also, depending on the ground, as a deterrent against the deer escaping the park grounds. Although deer swim readily, they will only do so if there is an incentive for good feed, and where there is not difficult marshy ground to traverse. We have already noticed that landowners only very rarely registered their deer parks, and Croom has observed that frequently "a small landowner received a grant of a free warren years before enclosing a deer park."[7] She noted that "the main purpose of a deer park was for hunting both for sport and for a supply of fresh meat . . . parks were also stocked with smaller game, including hares, rabbits, partridges and pheasants, which were an additional source of meat for the household."[8] (See Plate 11).

Sir Thomas Lucy always used the term "keeper", not "warrener" for the men responsible for the game in his park; and it is revealing that William Hullett had come as keeper from Hatton to head-keeper at Charlecote, suggesting deer-keeping was one of his main responsibilities. But undoubtedly Lucy's park was used for storing rabbits, and it is likely that these were hunted along with the other game in the enclosure. As one authority on rabbits has noted, "Hawking is . . . a very satisfactory way of taking rabbits . . . [and] twenty or more rabbits can be taken in a day" through this method.[9] As we have previously noted, Sir Thomas Lucy kept at least one falconer at Charlecote and it is likely that a part of their duties was the hunting of rabbits.

It was not uncommon for parks to be stocked with both deer and rabbits; for example, during the survey of Richard Norton's lands at Ripon in 1570, it was noted that "within half a mile of his house, he has a park of 1 miles, well stored with deer and conies, which are now nearly spoiled."[10] We have already seen that Charlecote was "imparked" at the end of the fifteenth century, and that there is evidence that deer were kept there in about 1520. The enclosure depicted in the Sheldon tapestry map, and described by Bracebridge, was far too extensive to be used merely for

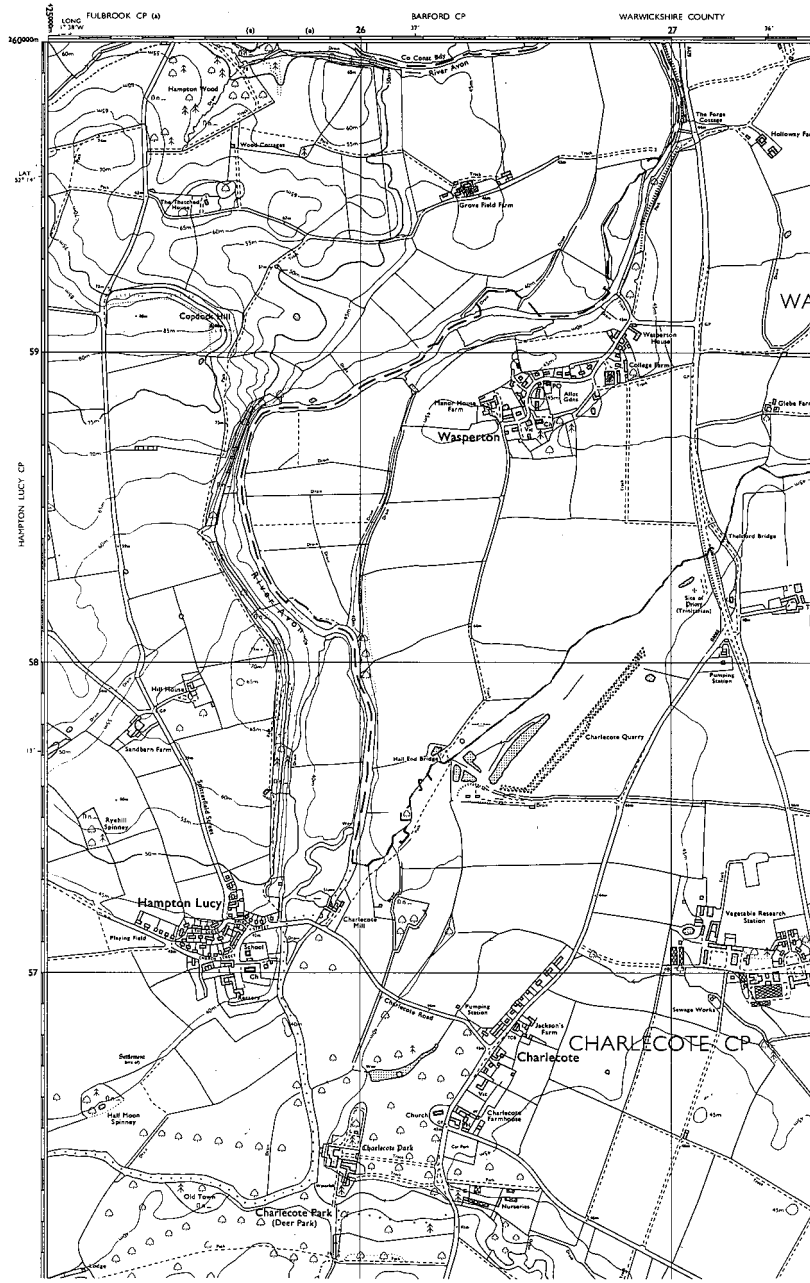

Plate 9: Ordnance Survey Map of Charlecote and Surrounds

Plate 10: Strensham Park (From Sheldon Tapestry Map of Worcestershire in The Victoria and Albert Museum)

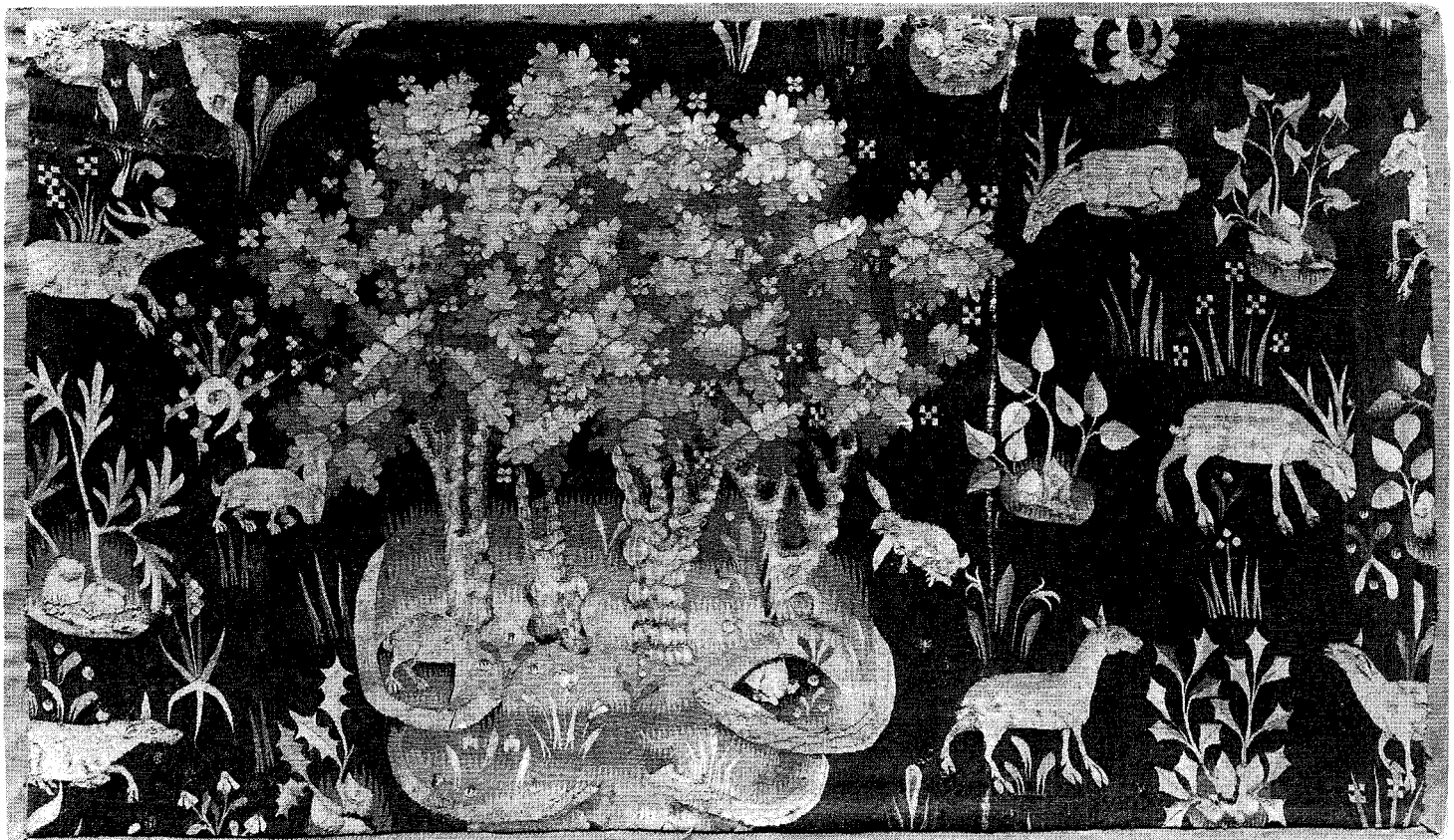

Plate 11: "Deer Park and Cony Warren" (Tapestry, in The Victoria and Albert Museum)

the stocking of rabbits. Evidence that it was used as a park as well as a warren is provided by two leases made for ground in Charlecote in the mid-seventeenth century. In March 1663 a lease was granted by Richard Lucy to Richard Hinton and Robert Sandys, "husbandmen . . . of Charlecote", to farm land described as follows:

> "All that pasture ground or ground inclosed commonly called the parke situate lieing and being in Charlecote aforesayd & adjoyning to the Mansion house of the sayd Richard Lucy... except . . . All trees whatsoever . . . with full power & authority [for Lucy] with his . . . servants . . . to fell cut downe and carry away the same . . . [but the tenants to maintain] the gates railes hedges ditches mounds & fences."[11]

The lease was to run for four years, at a rent of £40 a year. This was exactly the same rental value that Fulbrook Park was valued at during the late sixteenth and early seventeenth century, which gives an idea of the extent of the old Charlecote Park. The land was further leased in 1679, with some additional detail in its description; the lands leased contained "the two partes of the parke wherein the pooles are situate and being in the Parish of Charlecott . . . [except] Woods and Underwoods, timber trees and other trees hedges and hedged Bowds whatsoever now standing growing . . . or being upon the said Demised Premises."[12] There are virtually no woods left now in this area, although from the descriptions in the leases, there seems to have been extensive woodland during the first half of the seventeenth century. (In 1598, Abraham Sturley, in a letter to Richard Quiney, referred to Sir Thomas Lucy as one of the "knightes of the woodland".) The park as described ran north from Charlecote House up to Charlecote Quarry; it was contained within the parish of Charlecote and so would not have been as extensive as the old park depicted in the Sheldon Tapestry Map. The latter ran up to the river – presumably where the river and Scar Bank joined – which is almost exactly opposite Wasperton, the location indicated by the tapestry map. Presumably Sir Thomas Lucy rented this extra ground from the owner of the manor of Wasperton, in whose jurisdiction it lay.

It should be noted that the join of the river with Scar Bank is adjacent to Grovefield and very near to the site of the old deer-barn; the latter lies between Fulbrook Park and the old Charlecote Park, so it is possible that Shakespeare hid here after poaching from either of these parks. However, he would have had to have crossed the river to get into Charlecote at this location – possible but more difficult than entering Fulbrook, which lay directly adjacent to the deer barn.

There is the question of the punishment that was meted out to Shakespeare as a result of his poaching activities; scholars have previously noted that as Charlecote was a warren rather than a park, Shakespeare could not have been punished for poaching deer from there. (This objection would obviously not apply to Fulbrook, which was a registered deer park). In order to meet this critical point, we must examine the relevant legislation of the period. In 1563 it was enacted that "if any person or persons . . . break or enter into any park impaled, or any other several ground closed with wall, pale or hedge, and used for the keeping, breeding, and cherishing of deer, and so wrongfully hunt, drive or chase out, or take, kill, or slay any deer . . . shall suffer imprisonment . . . of three months, and shall yield & pay to the party grieved his treble damages." But the Act then went on to stipulate that it did not apply "to any park or enclosed ground heereafter to be made and used for deer, without the grant or licence of our sovereign lady the Queen."[13] The Act gave protection to an enclosure which was used as a deer park by "immemorial prescription", and this was crucial, because as we have seen, less than ten per cent of deer parks were licenced. Also the distinction made by the law between parks and warrens was a very ambiguous one, as was shown by legislation enacted in 1540:

> "if any p[er]son . . . wrongefully take kill or slee any Deere within any parke or closed ground used for Deere, or with any hay or other nette or with any fyrett or pursenett any of the conyes or rabettis being within any parke or ground closed for maynten'nce or keping of Deere, or in any place being laufull warren . . . shalbe adjudged and taken felony; and that thoffendeer and offendours therein being thereof laufully attaynted shalhave and suffre suche paynes of death and shall lose and fofaicte theer goodis and cattalle landis and tenements . . . Provided always that this acte or any thing therein conteyn'd extend not to any park or inclosed grounde used for Deere, heretofore had or made without the graunt or lycence . . . or otherwise not allowed or approved of recorde; nor to any grounde herafter to be impark'd or inclosid for Deere by any of the Kinges subjectis, nor to any groundes not nowe usid as a warren for or of any conyes at this present tyme."[14]

The legislation of 1563 made no reference to the 1540 Act, and so the provisions of the latter, where they were not superceded by the former, were still in force by Shakespeare's time. In particular, the provisions relating to warrens – including the very severe punishments – would have applied to the poaching of rabbits. The 1540 Act was rather ambiguous in its definition of a park or a warren: it certainly protected rabbits kept in a

deer park; but it is less clear whether deer kept in a licenced warren were also protected. The Act appears to have envisaged parks and warrens as overlapping in their functions, which is entirely consistent with what we know of the history of Charlecote.

Davies in his account of the poaching tradition states that Shakespeare was "oft whipt & sometimes imprisoned", and although legislation stipulated imprisonment, nowhere does it mention whipping. However, it was notorious during Shakespeare's time that magistrates were a law unto themselves; for example, Lambard complained in 1582 "that justices of the peace . . . arrogate unto themselves authority to use their discretion, and to play, as it were, the Chancellor in every cause that cometh before them."[15] Sir Robert Cecil was even more specific in a letter he wrote in 1600: "for my deare that are killed, what I can do by law I will prove, but otherwise I will reveng myself by no other meanes under color of authority being in myne owne case"[16] – exactly the position of Sir Thomas Lucy, justice of the peace.

That whipping was seen by contemporaries as a minor form of punishment, is indicated by one author's satirical observation of the effects of a free-school education: "I must needs come short of their experience that are bred up in free-schools, who, by plotting to rob an orchard,etc, run . . . under no higher penalty than a whipping."[17] All in all, contemporary legislation and authority of magistrates, gave Sir Thomas Lucy all the power that he needed to very harshly punish the young Shakespeare. All the ingredients of the poaching tradition are to be found in the historical record: two areas of enclosed parkland, a registered deer park (Fulbrook) and cony warren (Charlecote), a gate-house, estate game-keepers, the presence of both deer and rabbit in at least one of Sir Thomas Lucy's parks, and the existence of legislation stipulating severe punishment for poaching. The weight of evidence therefore supports the various traditional accounts of Shakespeare's deer and cony-stealing exploits – sometimes in great detail – and that by breaking into the park, he incurred the wrath of Sir Thomas Lucy and was forced to flee to London to avoid further punishment.

CHAPTER 11:
THE WILD YOUTH

In all, there are mentions of deer-hunting and cony-catching in eighteen of Shakespeare's plays and two in his narrative poems: *The Comedy Of Errors*, *The Winter's Tale*, *Cymbeline*, *King Henry VI*, *As You Like It*, *Love's Labour's Lost*, *Coriolanus*, *Much Ado About Nothing*, *The Merry Wives Of Windsor*, *Anthony And Cleopatra*, *Hamlet*, *Macbeth*, *Julius Caesar*, *The Taming Of The Shrew*, *Twelth Night*, *Troilus And Cressida*, *Titus Andronicus*, *Romeo And Juliet*, *Venus And Adonis*, and *The Rape Of Lucrece*. If interpreted correctly, these references can make a significant contribution to Shakespeare biography. A passage from *The Winter's Tale* enables us to estimate the approximate date that Shakespeare was involved in the poaching incident. The old shepherd, hearing his sons hunting, laments:

> "I would there no age betweene ten and three and twenty, or that youth would sleep out the reste; for there is nothing (in the betweene) but getting wenches with childe, wronging the Auncientry, stealing, fighting, hearke you now: would any but these boylde-braines of nineteene, and two and twenty hunt this weather?"[1]

Shakespeare was born approximately in April 1564 and his younger brother, Gilbert, in October 1566. In the summer of 1586, William would have been twenty-two, and his brother nineteen; the shepherd tells us that the wildness of youth ends at the age of twenty-three. Although purely speculative, if this passage were read to apply to Shakespeare, this would give a date of 1587 for the year he was caught poaching Lucy's deer. Echoes of the relationship between the shepherd and his two sons are found in the relationship between Belarius and his two adopted sons Guiderius and Arviragus, in *Cymberline*. Arviragus laments his impoverished and narrow provincial circumstances:

> "What thing is't, that I neuer/ Did see man dye, scarse euer look'd on blood,/ But that of Coward Hares, hot Goats , and Venison?/ Neuer bestrid a Horse saue one, that had/ A Rider like my selfe, who ne'er wore Rowell/ Nor Iron on his heele? I am asham'd/ To looke vpon the holy Sunne, to haue/ The benefit of his blest Beames, remaining/ So long a poore vnknowne."[2]

It is possible to see here expressions of what Shakespeare called in Sonnet 121 "my sportive blood". There is sufficient detail in the plays to give us some idea of the setting and the method used by Shakespeare in his poaching activities. In one of his first plays, *King Henry VI*, "park'd and bounded in a pale – A little herd of England's timorous deer", where the King stood "close to steale the Bishop's Deere"[3], the scene is set in "A chase in the north of England":

> "*Enter Sinklo, and Humfrey, with Crosse-bowes in their hands.*
> *Sinklo* Vnder this thicke growne brake, wee'l shrowd our selues:/ For through this Laund anon the Deere will come,/ And in this couert will we make our Stand,/ Culling the principall of all the Deere.
> *Humfrey* Ile stay aboue the hill, so both may shoot.
> *Sinklo* That cannot be, the noise of thy Crosse-bow/ Will scarre the Heard, and so my shot is lost:/ Heere stand we both, and ayme we at the best ..."[4]

This scene is reminiscent of that in *Love's Labour's Lost*, where standing in the park, the forester tells the princess in order to kill the deer, she must place herself "vpon the edge of yonder Coppice,/ A Stand where you may make the fairest shoote."[5] The actual death of the deer is described in *As You Like It*, in the setting of the Forest of Arden:

> "Vnder an oake, whose anticke roote peepes out/ Vpon the brooke that brawles along this wood,/ To the which place a poore sequestred Stag,/ That from the hunter's aime had tane a hurt,/ Did come to languish; and indeed my Lord,/ The wretched animall heau'd forth such groanes/ That their discharge did stretch his leatherne coat/ Almost to bursting, and the big round teares/ Cours'd one another downe his innocent nose/ In pitteous chase: and thus the hairie foole,/ Much marked of the melancholie *Jacques*,/ Stood on th' extremest verge of the swift brooke,/ Augmenting it with teares."[6]

This is reminiscent of Benedick's comment on the Count in *Much Ado About Nothing*: "I found him heere as melancholy as a Lodge in a Warren."[7] That Shakespeare was familiar with conies is shown by his references to them in *Coriolanus* – "they will out of their Burroughes, (like Conies after Raine)"[8] – and *King Henry VI* – "So doth the Connie struggle in the Net."[9] And there is a touch of his own experience in his satire on Master Slender: "he is as tall a man of his hands, as any is

betweene this and his head: he hath fought with a Warrener."[10] That all forms of game could be hunted in one area of parkland is revealed in one of Shakespeare's first published writings, *Venus And Adonis*:

> "But if thou needs wilt hunt, be rul'd by me;/ Uncouple at the timorous flying hare,/ Or the fox which lives by subtlety,/ Or at the roe which no encounter dare . . . And when thou hast on foot the purblind hare,/ Mark the poor wretch . . . Sometimes he runs . . . where earth-delving conies keep,/ To stop the loud pursuers in their yell;/ And sometimes sorteth with a herd of deer."

Shakespeare was very familiar with the terms used by sportsmen for the different categories of deer; in *Love's Labour's Lost*, he makes the following play on words: "The prayfull Princesse pearst and prickt a prettie pleasing Pricket,/ Some say a Sore, but not a sore till now made sore with shooting./ The Dogges did yell, put ell to Sore, then Sorell jumps from thicket:/ Or Pricket sore, or else Sorell, the people fall a hooting./ If Sore be sore, then ell to Sore, makes fiftie sores O sorell:/ Of one sore I an hundred make by adding but one more L."[11] A pricket is a stag in its second year, a sorell is one in its third year, and a sore one in its fourth year.

Shakespeare invariably uses the language of the hunt metaphorically. Anthony laments Caesar's fate in just such a fashion: "Heere was't thou bay'd braue Hart . . . O World! thou wast the Forrest to this Hart . . . How like a Deere, stroken by many Princes,/ Dost thou heere lye."[12] Similarly, after lamenting the fate of the wounded stag, Jacques exclaims: "Sweepe on you fat and greazie Citizens,/ 'Tis just the fashion:/ wherefore doe you looke/ Vpon that poore and broken bankrupt there?"[13] As well as a metaphor of violence, Shakespeare frequently uses the pursuit and hunting of deer as a sexual metaphor: "Oh thus I found her straying in the Parke,/ Seeking to hide herselfe as doth the Deare/ That hath receiued some vnrecuring wound."[14] And the winning of woman and the stealing of deer were directly juxtaposed:

> "Why, mak'st thou it so strange?/ Shee is a woman, therefore may be woo'd,/ Shee is a woman, therefore may be wone . . . Then why should he dispaire that knowes to court it/ With words, faire lookes, and liberality:/ What hast not thou full often strucke a Doe,/ And borne her cleanly by the Keepers nose?"[15]

Sometimes the language is playfully possessive: "Within the circuit of this ivory pale,/ I'll be a park, and thou shalt be my deer . . . Then be my deer, since I am such a park;/ No dog shall rouse thee, though a thousand

bark."[16] Other times the language is more violent, as in the *Twelfth Night:* "O when mine eyes did see *Olivia* first,/ Me thought she purg'd the ayre of pestilence;/ That instant was I turn'd into a Hart,/ And my desires like fell and cruell hounds,/ Ere since pursue me."[17] Hunting and sexuality were inextricably linked in Shakespeare's mind, and of the many quotes we could select to illustrate this, perhaps the most apt – although with a touch of Oedipus – is from one of his early narrative poems:

> "She hearkens for his hounds and for his horn./ Anon she hears them chant it lustily./ And all in haste she coasteth to the cry./ And as she runs, the bushes in the way/ Some catch her by the neck, some kiss her face,/ Some twine about her thigh to make her stay;/ She wildly breaketh from their strict embrace,/ Like a milk doe whose swelling dugs do ache/ Hasting to feed her fawn hid in some brake."[18]

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Underlying Shakespeare's treatment of violence and sex, there lies a general theme of the wildness of youth. In *Hamlet*, Polonius complains of "such wanton, wild, and vsual slips,/ As are Companions noted and most knowne/ To youth and liberty."[19] This wildness was seen by Shakespeare as mainly physical: "Young bloud doth not obey an old decree"[20] – although, "The bloud of youth burns not with such excesse/ As grauity's reuolt to wantons be."[21] Wildness was linked with rebellion: "Naturall rebellion, done i'th blade of youth,/ When oyle and fire, too strong for reasons force,/ Ore-beares it, and burnes on."[22] But "the aimes, and ends of burning youth" had to be curbed by custom and law: "We haue strict Statutes, and most biting Laws,/ The needful bits and curbes to headstrong weedes."[23]

This rebellious wildness was often sexual: "In generall Riot, melted downe thy youth/ In different beds of Lust, and neuer learn'd/ The Icie precepts of respect, but followed/ The sugred game before thee."[24] Much of this can be related to Shakespeare's own experience; he was only eighteen when he married – his wife was eight years older – and his first child, Susanna was born less than six months after the date of his marriage.[25] Although there may have been a pre-marital contract between Shakespeare and Anne Hathaway, this type of contract had become rare by the 1580's.[26] This was reflected in the pattern of marriage in Stratford during this period: of 109 marriages which took place

between 1580 and 1599, only 11.9% resulted in children born less than six months after marriage. (The figure increased to 17.4% for under eight months, and 29.3% for under eight-and-a-half months.)[27] Thus it was only a minority of marriages where the wife was pregnant at marriage, and this was particularly the case for brides who had been pregnant for three months or more.

Through recent research, it is also possible to throw light on the question of age of marriage: the mean age of first marriage for women in Stratford ranged between 21.1 and 25.6 years in the period 1580-1624, and for men the equivalent figures were 25.8 and 28.2. The samples for the earlier period are too small to be reliable, and if we aggregate them to reasonable sizes, we arrive at the following figures: the average age for women in 1580-1609 was 23.0 (based on a sample of 111 women), and men in 1590-1609 was 27.16 (a sample of 71).[28] This was very similar to what it was in the two neighbouring rural parishes of Wellesbourne and Alveston: for women 24.1 in 1595-1629, and men 27.8 – although Skipp found in his Forest of Arden parishes for the period 1575-99, mean ages of marriage of 26.3 for women and 29.7 for men.[29] Anne Hathaway came from a rural parish, and so perhaps the latter averages are more relevant to her. The conclusion from these various figures is that Anne Hathaway at twenty-six may have been a little older than the average woman when married, but not very much older – and certainly not the "older" woman that many previous biographers have labelled her. It is rather the the reverse which is true: Shakespeare was a lot younger than most of his contemporaries, who on average married ten years later than he did. Marrying at the age of eighteen must have been very rare (we do not have the exact figures), and probably was the result of Anne Hathaway being pregnant at the time of marriage.

Shakespeare was deeply ambivalent about pre-marital sexuality: on the one hand some passages in the plays indicate a belief in the morality of the pre-marital contract, whereas others, indicate strong moral disapproval. In *All's Well That Ends Well* (Act 5, Scene iii), Diana demands of Bertram, "Ask him vpon his oath, if hee do's thinke/ He had not my virginity" – to which Bertram replies : "certaine it is I lyk'd her,/ And boorded her i'th wanton way of youth." And Prospero warns Ferdinand against similar treatment of Miranda:

> "Worthily purchas'd, take my daughter: But/ If thou do'st breake her Virgin-knot, before/ All sanctimonious ceremonies may/ With full and holy right, be ministred,/ No sweet aspersion shall the heauens let fall/ To make

> this contract grow; but barraine hate,/ Sower-ey'd disdaine, and discord, shall bestrew/ The vnion of your bed, with weedes so loathly/ That you shall hate it both: Therefore take heede,/ As Hymens Lamps shall light you."[30]

This was written towards the end of Shakespeare's life, and probably his attitude had hardened as he got older. But the reflection of his own experience was to be found in Caliban, the embodiment – to use Jung's phrase – of Prospero's "shadow" ("this Thing of darknesse, I/ Acknowledge mine"[31]): "Thou most lying slaue,/ Whom stripes may moue, not kindnes: I haue vs'd thee/ (Filth as thou art) with humane care, and lodg'd thee/ In mine owne Cell, till thou didst seek to violate/ The honor of my childe."[32]

The change in Shakespeare's attitude is reflected in the clown's song in *Hamlet:* "In youth when I did loue, did loue,/ me thought it was very sweete:/ To contract O the time for a my behoue,/ O me thought there was nothing meete . . . But Age with his stealing steps/ hath caught me in his clutch:/ And hath shipped me intill the Land,/ as if I had neuer beene such."[33] But the idea of a linear shift of attitude is too simple; Shakespeare's characters declaim varying attitudes in different plays, with no obvious chronology. In one of the earliest plays, *Love's Labour's Lost*, the Princess protests by her "maiden honor, yet as pure/ As the vnsallied Lilly"[34], whereas in a play of the middle period, *Twelth Night*, reference is made to "A Contract of eternall bond of loue,/ Confirm'd by mutuall joynder of your hands,/ Attested by the holy close of lippes,/ Strengthned by enterchangement of your rings,/ And all the Ceremonie of this compact."[35]

The ambiguity of attitude is reflected directly in a number of passages; when Claudio casts aspersions on Hero's honour as "a maid", Leonato rejoins, "Deere my Lord, if you in your owne proofe,/ Haue vanquisht the resistance of her youth,/ And made defeat of her virginitie" – to which Claudio interrupts: "I know what you would say: if I haue knowne her,/ You will say, she did imbrace me as a husband,/ And so extenuate the forehand sinne."[36] And perhaps the young Shakespeare's impatience can be seen in Rosalind's speech in *As You Like It:* "Marry he trots hard with a yong maid, between the contract of her marriage, and the day it is solemnizd: if the interim be but a sennight, Time's pace is so hard, that it seemes the length of seuen yeare."[37]

The play which deals mainly with the issue of the morality of pre-marital conception is *Measure For Measure*. The theme is treated in such a complex fashion that it is impossible to do justice to it here as part of a

more general discussion. Suffice it to say, that the same ambiguity and ambivalence noted elsewhere, is also to be found in this play. This can be illustrated by quoting from Act 1, Scene 2; Claudio is to be sent to prison "for getting Madam *Julietta* with childe". Claudio engages in dialogue with Lucio about the morality of the issue

> "*Claudio* Thus can the demy god (Authority)/ Make vs pay downe, for our offence, by waight/ The words of heauen; on whom it will, it will/ On whom it will not, (soe) yet still 'tis just.
> *Lucio* Why how now *Claudio:* whence comes this restraint.
> *Claudio* From too much liberty, my *Lucio* Liberty/ As surfet is the father of much fast,/ So euery Scope by the immoderate vse/ Turnes to restraint: Our natures do pursue/ Like rats that rauyn downe their proper Bane,/ A thirsty euill; and when we drinke, we die.
> *Lucio* . . . what's thy offence, *Claudio?* . . . Lecherie?
> *Claudio* Call it so . . . vpon a true contract/ I got possession of *Julietas* bed./ You know the Lady, she is fast my wife,/ Save that we doe the denunciation lacke/ Of outward Order. This we came not to,/ Onely for propagation of a Dowre/ Remaining in the Coffer of her friends, From whom we thought it meet to hide our Loue/ Till Time had made them for vs. But it chances/ The stealth of our most mutuall entertainment/ With Character too grosse, is writ on *Juliet.*"

What was the result of this ambiguity of attitude in Shakespeare's own life? The passage quoted earlier from *The Tempest* – "If thou do'st breake her Virgin-knot, before/ All sanctimonious ceremonies . . . barraine hate,/ Sowerr-ey'd disdaine, and discord shall bestrew/ The vnion of your bed ..." – suggests that Shakespeare's marriage was not a happy one. We will be looking later at other evidence in support of this conclusion – in particular the sonnets – but the fact that he mainly worked in London after he came to maturity, that he left his wife and children in Stratford, and only visited them, according to tradition, once a year[38], is certainly consistent with it.

Looking at his writings for a reflection of his attitude towards marriage, we see at the beginning a period of optimism: "To the best Bride-bed will we,/ Which by vs shall blessed be:/ And the issue there create/ Euer shall be fortunate:/ So shall all the couples three/ Ever true in louing be."[39] And during this early period, the "new married Wife about her Husbands Necke, [is] hardly to be shooke off"[40], and as long as marriage is rightly ordered, "peace it boads, and loue, and quiet life . . . And to be short, what not, that's sweete and happie."[41]

But very quickly things change: "men are Aprill when they woe, December when they wed"[42], and as one wife complains about husbands,

"when you haue our Roses/ You barely leave our thornes to pricke our selues,/ And mocke vs with our barenesse."[43] And this is echoed in *Twelth Night* (2, iv), in a situation very similar to Shakespeare's own, of a very young man marrying an older woman. Addressing remarks to a young man/boy (Viola in disguise), the Duke advises: "Let still the woman take/ An elder than her selfe; so weares she to him;/ So swayes she leuell in her husbands heart:/ For boy, howeuer we do praise our seiues,/ Our fancies are more giddie and vnfirme,/ More longing, wauering, sooner lost and worne,/ Then womens are . . . Then let thy Loue be yonger then thy selfe,/ Or thy affection cannot hold the bent:/ For women are as Roses, whose fair flowre/ Being once displaid, doth fall that verie howre."

The play in which the theme of the internal disintegration of marriage is depicted, is *The Comedy Of Errors*. Through the device of mistaken identity, Shakespeare is able to indirectly explore tensions within marriage. The nature of the marriage between Antipholus and Adriana, although treated as a part of a comic plot, is revealed through the dialogue between the Abbess and Adriana:

> "*Abbess* Hath he not lost much wealth by wrack of sea,/ Buried some deere friend, hath not else his eye/ Stray'd his affection in vnlawfull loue,/ A sinne preuailing much in youthfull men/ Who giue their eies the liberty of gazing./ Which of these sorrowes is he subject to?
> *Adriane* To none of these, except it be the last;/ Namely, some loue that drew him oft from home.
> *Abbess* You should have for that haue reprehended him.
> *Adriane* Why so I did.
> *Abbess* I but not rough enough.
> *Adriane* As roughly as my modestie would let me.
> *Abbess* Haply in priuate.
> *Adriane* And in assemblies too.
> *Abbess* I, but not enough.
> *Adriane* It was the copie of our Conference./ In bed he slept not for my vrging it/ At boord he fed not for my vrging it:/ Alone, it was the subject of my Theame:/ In company I often glanced it:/ Still did I tell him, it was vilde and bad.
> *Abbess* And thereof came it, that the man was mad./ The venome clamors of a jealous woman,/ Poisons more deadly then a mad dogges tooth./ It seemes his sleepes were hindred by thy railing,/ And thereof comes it that his head is light./ Thou saist his meat was sawcd with thy vpbraidings,/ Vnquiet meales make ill digestions;/ Thereof the raging fire of feauer bred;/ And what's a Feauer, but a fit of madnesse?/ Thou sayest his sports were hindred by thy bralles./ Sweet recreation barr'd, what doth ensue/ But

moodie and dull melancholly,/ Kinsman to grim and comfortlesse dispaire."[44]

Antipholus's unfaithfulness is suggested earlier in the play: "My wife is shrewish when I keepe not howres . . . I know a wench of excellent discourse,/ Prettie and wittie; wilde, and yet too gentle;/ There we will dine . . . To her will we to dinner."[45] Luciana, Adriana's sister, upbraids Antipholus for his unfaithfulness: "Euen in the spring of Loue, thy Loue-springs rot? . . . if you like else-where, doe it by stealth,/ Muffle your false loue with some shew of blindnesse:/ Let my sister not read it in your eye . . . Be secret false: what need she be acquainted?/ . . . 'Tis double wrong to truant with your bed/ And let her read it in thy lookes at boord . . . Though others have the arm, show vs the sleeue."[46]

The wife's bitterness at this treatment is described through Emilia's speech in *Othello:* "Let Husbands know/ Their wiues have sense like them: They see, and smell,/ And haue their Palats both for sweet, and sowre/ As husbands haue. What is it that they do/ When they change vs for others? Is it Sport? I thinke it is: and doth Affection breed it?/ I thinke it doth. Is't Frailty that thus erres?/ It is so too. And haue we not Affections,/ Desires for Sport? and Frailty, as men haue?/ Then let them use vs well: else let them know,/ The illes we do, their illes instruct vs so."[47]

We should note that in this passage, as elsewhere, Shakespeare uses a sporting metaphor for sexual unfaithfulness. That the theme of unfaithfulness was not merely fictional, is indicated by the sonnets. His love for the dark lady and his fair friend are too familiar to warrant any extensive discussion. There are however sonnets which do illuminate the themes under discussion, and these will be briefly considered. In sonnet 152, Shakespeare confesses his unfaithfulness: "In loving thee thou know'st I am forsworn./ But thou art twice forsworn, to me love swearing;/ In act thy bed-vow broke, and new faith torn/ In vowing new hate after new love bearing./ But why of these two oaths' breach do I accuse thee,/ When I break twenty?" Earlier in sonnets 141 and 142, he was even more explicit: "Only my plague thus far I count my gain,/ That she that makes me sin awards me pain./ Love is my sin . . . those lips of thine . . . Robb'd others' beds' revenues of their rents."

And the sonnet following, sonnet 143, is perhaps the most self-revealing of all those written by Shakespeare. It is not within the brief of this book to analyse psychological motivation in detail – this has been undertaken by Freud, Jones and other psycho-analytical writers – but it is

appropriate to briefly consider the relevance of some of this material. Sonnet 143 runs:

> "Lo as a careful huswife runs to catch/ One of her feathered creatures broke away,/ Sets down her babe, and makes all swift dispatch/ In pursuit of the thing she would have stay;/ Whilst her neglected child holds her in chase,/ Cries to catch her whose busy care is bent/ To follow that which flies before her face./ Not prizing her poor infant's discontent;/ So run'st thou after that which flies from thee,/ Whilst I thou babe chase thee afar behind;/ But if thou catch thou hope, turn back to me,/ And play the mother's part, kiss me, be kind./ So will I pray that thou mayst have thy Will,/ If thou turn back and my loud crying still."

The sonnet reveals a strong oedipal theme: the child/man is rejected by the mother/woman, leading to a desperate and dependent pleading for love – and it does not require a great deal of psychological understanding to see that the almost inevitable consequence is rejection, hurt and anger. Shakespeare's bitterness towards the dark lady had tinges of both sadism and masochism. The latter is dominant, and certainly when she appears as a figure in the plays, she usually is presented in that guise. In *Love's Labour's Lost*, Rosaline is described "as blacke as Ebonie . . . [and] Blacke is the badge of hell,/ The hue of dungeons, and the Schoole of night."[48] And likewise, in *Romeo And Juliet*, Mercutio, referring to another Rosaline, describes how "that same pale hard-harted wench, that *Rosaline*/ torments him so, that he sure will run mad."[49] And perhaps it is no co-incidence, that a character with virtually the same name – Rosalind – is, in *As You Like It*, associated with another dark lady, Celia: the shepherd falls in love with Celia's "foulnesse", and Celia falls in love with Rosalind's "anger".[50] Three dark ladies with the name of Rosaline/ Rosalind – is it possible that in real life, that Shakespeare's dark lady carried that name?

That Shakespeare was capable of dramatising his feelings towards the dark lady in inverted form is illustrated by the scene between Demetrius and Helena in *A Midsummer's Night Dream*. Helena declaims: "I am your spaniell, and *Demetrius*,/ The more you beat me, I will fawne on you./ Vse me but as your spaniell; spurne me, strike me,/ Neglect me, lose me; only giue me leave/ (Vnworthy as I am) to follow you./ What worser place can I beg in your loue,/ (And yet a place of high respect for me)/ Then to be vsed as you doe your dogge?"[51] In view of this, it is perhaps not surprising that Shakespeare at times adopted a very misogynistic viewpoint, expressed most vividly in *King Lear:* "though Women all aboue: but to the Girdle do the gods inherit, beneath is all the Fiends.

There's hell, there's darkenes, there is the sulphurous pit; burning, scalding, stench, consumption."[52] A similar theme is expressed in sonnet 144, perhaps most aptly, immediately following 143 quoted above

> "Two loves I have, of comfort and despair,/ Which like two spirits do suggest me still;/ The better angel is a man right fair,/ The worser spirit a woman colour'd ill./ To win me soon to hell, my female evil/ Tempteth my better angel from my side,/ And would corrupt my saint to be a devil,/ Wooing his purity with her foul pride ..."

The denigration of the dark lady, and idealisation of his fair friend, is the process of "splitting", projecting all the unacceptable erotic and hostile impulses onto the woman, and all that is noble and virtuous onto the man. It has become commonplace to see Shakespeare's attitude towards his friend as being of a homosexual nature, although the idealised tone of the sonnets would suggest that this was latent rather than actual. This is confirmed by Sonnet 20, in which Shakespeare explicitly repudiates homosexuality: "And for a woman wert thou first created:/ Till nature, as she wrought thee, fell adoting./ And by addition me of thee defeated/ By adding one thing to my purpose nothing./ But since she prick'd thee out for women's pleasure,/ Mine be thy love, and thy love's use their treasure."

In Sonnet 62 Shakespeare explains his love for his young friend as an expression of his own narcissism: "Sin of self- love possesseth all mine eye,/ And all my soul, and all my every part;/ And for this sin there is no remedy,/ It is so grounded in my heart./ Methinks no face so gracious as is mine,/ No shape so true, no truth of such account,/ And for myself mine own worth do define/ As I all other in all worths surmount./ But when my glass shows me myself indeed,/ Beated and chopt with tann'd antiquity,/ Mine own self-love quite contrary I read;/ Self so self-loving were iniquity./ 'Tis thee, my self, that for myself I praise,/ Painting my age with beauty of thy days."

However, in addition to the sonnets, there are a number of the plays, where male characters fall in love with women dressed as men, in which there is a distinct homosexual undertone.[53] The nearest that Shakespeare came to explicit homosexual imagery is in *Venus And Adonis;* at the end of the poem, the encounter between the boar and Adonis is described as follows: "But this foul, grim, and urchin-snouted boar,/ Whose downward eye still looketh for a grave,/ Ne'er saw the beauteous livery that he wore:/ Witness the entertainment that he gave./ If he did see his face, why then I know/ He thought to kiss him, and hath kill'd him so . . . And

nuzzling in his flank, the loving swine/ Sheath'd unaware the tusk in his soft groin." Shakespeare's "flight from woman" can be linked, as we saw earlier, to his attitude to marriage. In the play that deals with this theme, *The Comedy Of Errors*, the flight of the husband from his wife is described by Adriana:

> "Whil'st I at home starue for a merrie looke:/ Hath homelie age th' alluring beauty tooke/ From my poor cheeke? then he hath wasted it./ Are my discourses dull? Barren my wit,/ If voluble and sharpe discourse be mar'd,/ Vnkindness blunts it more than marble hard./ Doe their gay vestments his affections baite?/ That's not my fault, he's master of my state./ What ruines in me that can be found,/ By him not ruin'd? Then he is the ground/ Of my defeatures. My decayed faire/ A sunnie looke of his, would soone repaire./ But, too vnruly Deere, he breakes the pale,/ And feedes from home; poore I am but his stale . . . I know his eye doth homage other where,/ Or else, what lets it but he would be here?"[54]

We might say with Bertram, in *All's Well That Ends Well*, that "Warres is no strife/ To the dark house, and the detested wife."[55] And in the light of all the evidence considered in this chapter, perhaps the most apt conclusion about Shakespeare's attitude towards marriage lies in Dromio's comment: "As from a Beare a man would run for life,/ So fly I from her that would be my wife."[56]

CHAPTER 12:
BANISHMENT AND EXILE

According to Richard Davies, Shakespeare had been "oft whipt and sometimes imprisoned" for poaching Sir Thomas Lucy's deer. The theme of whipping and banishment is found in a number of the plays, but is most fully explored in *King Lear*. Poor Tom "is whipt from Tything to Tything, and stockt, punish'd, and imprison'd"[1] – a fate which cannot be escaped; as the Fool observes: "they'l have me whipt for speaking true: thou'lt haue me whipt for lying, and sometimes I am whipt for holding my peace."[2] Cornwall punishes Kent by placing him in the stocks, a method of punishment Gloucester considers socially demeaning: "your purpos'd low correction/ Is such as basest and contemned'st wretches/ For pilf'rings and most common trespasses/ Are punish'd with."[3]

In *All's Well That Ends Well*, the Countess playfully teases the clown: "Doe you crie O Lord sir at your whipping, and spare not me? Indeed your O Lord sir, is very sequent to your whipping: you would answere very well to a whipping if you were but bound too't."[4] Later in the play, Lafeu tells Parolles, "you were beaten in *Italy* for picking a kernell out of a Pomgranat"[5], and in the same scene, when the lords deny Helena, he tells her, "And they were sons of mine, I'de haue them whip'd." In *The Winter's Tale*, Autolycus's imaginary robber "was certainely Whipt out of the Court"[6] This, like the previous references, is essentially comic, but in *Anthony And Cleopatra* the tone changes. Anthony has Caesar's messenger whipped:

> "*Anthony* . . . Take hence this Jack, and whip him . . . Whip him Fellowes,/ Till like a Boy you see him crindge his face,/ And whine aloud for mercy . . . Tugge him away: being whipt/ Bring him againe . . . Is he whipt?
> *Enter a Seruant with Thyreus.*
> *Servant* Soundly, my Lord.

Anthony Cried he? and begg'd a Pardon?
Servant He did aske fauour."[7]

A similar tone is adopted by Romeo in describing his punishment: "Shut vp in prison, kept without my foode,/ Whipt and tormented."[8] There are a number of references to whipping for sexual offences: in *All's Well That Ends Well*, there is a hint of Shakespeare's own experience in Parolles description of Dumain: "I know him, a was a Botchers Prentize in *Paris*, from whence he was whipt for getting the Shrieues fool with childe."[9] (According to one tradition, Shakespeare was a butcher's apprentice.) In *Love's Labour Lost*, Costard proposes that "Hector be whipt for *Jaquenetta* that is quicke by him"[10], and in *Midsummers Night Dream*, Lysander proposes that he and Hermia marry at a place remote from Athens, so that "the sharpe Athenian Law/ Cannot pursue vs."[11]

In spite of his belief in a strict social order, Shakespeare displays a profound mistrust of the administration of justice. As Hamlet tells Polonious, "Vse euerie man after his desart, and who shall scape whipping?"[12] And the most famous speech on this subject is of course Lear's bitter diatribe: "See how yond Justice railes vpon yond simple theefe. Hearke, in thine eare: Change places, and handy- dandy, which is the Justice, which is the theefe . . . there thou might'st behold the great image of Authoritie, a Dogg's obey'd in Office. Thou, Rascal Beadle, hold thy bloody hand: why dost thou lash that Whore? Strip thy owne backe, thou hotly lusts to vse her in that kind, for which thou whip'st her."[13] It is difficult not to see in this speech a tinge of Shakespeare's bitterness at his treatment by Sir Thomas Lucy, although obviously it is a great speech on the universal theme of the injustice of the law.

Following punishment, comes banishment and exile. Romeo's reaction to his banishment to Verona, perhaps gives something of what Shakespeare felt at being made to flee Stratford: "Ha, banishment? be mercifull, say death:/ For exile hath more terror in his looke,/ Much more then death: do not say banishment . . . There is no world without *Verona* walles,/ But Purgatorie, Torture, hell it selfe:/ Hence banished, is banisht from the world,/ And worlds exile is death. Then banished,/ Is death, mistearm'd, calling death banished,/ Thou cut'st my head off with a golden Axe,/ And smilest vpon the stroke that murders me."[14]

There is clearly an element of dramatic exaggeration in all this, and it is possible to see exactly the opposite reaction in *Cymbeline*. Belarius and his two adopted sons, Guiderius and Arviragus have been banished to a poor province of Wales, living in the most primitive of circumstances. We

have seen earlier how Arviragus champs at being "so long a poor unknown", and this has particular significance because in reality, he is one of the King's lost sons. (This is a familiar theme in Shakespeare: the poor and despised unknown, who in reality is of royal blood.) Like many of Shakespeare's reactions, we can detect strong ambiguity in his attitude to his exile from Stratford: both a banishment and yet at the same time, an escape from the narrow limits of provincial life. But that Shakespeare felt his banishment to be basically a form of exile, is indicated in Sonnet 29:

> "When in disgrace with Fortune and men's eyes,/ I all alone beweep my outcast state,/ And trouble deaf heaven with my bootless cries,/ And look upon myself, and curse my fate,/ Wishing me like to one more rich in hope,/ Featur'd like him, like him with friends possess'd,/ Desiring this man's art, and that man's scope,/ With what I most enjoy contented least;/ Yet in these thoughts myself almost despising,/ Haply I think on thee, and then . . . I scorn to change my state with kings."

The two plays which deal with exile are *King Lear* and *Coriolanus*. Gloucester in his rage with Edgar, gives orders for his capture: "Let him fly farre:/ Not in this Land shall he remain vncaught/ And found; dispatch . . . All Ports Ile barer; the villaine shall not scape . . . besides, his picture I will send farer and neere, that all the kingdome/ May haue due note of him ..."[15] Although highly dramatised, the conditions endured in the flight after banishment, perhaps give us a sense of what Shakespeare endured:

> "Poor naked wretches, where so ere you are/ That bide the pelting of this pittilesse storme,/ . . . Such sheets of Fire, such bursts of horrid Thunder,/ Such groanes of roaring Winde, and Raine, I neuer/ Remember to haue heard. Mans Nature cannot carry/ Th' affliction, nor the feare . . . [so] Tremble thou Wretch/ That hast within thee vndivulged Crimes/ Vnwhipt of justice."[16]

In *Coriolanus*, the bitterness of exile is a central theme. When his wife, mother and son are presented to him, Coriolanus denies all ties: "out affection,/ All bond and priuiledge of Nature breake . . . Ile neuer/ Be such a Gosling to obey instinct: but stand/ As if a man were Author to himself/ & knew no other kin . . . Wife, Mother, Child, I know not . . . Long as my Exile, sweet as my Reuenge!"[17] Final humiliation takes place when Coriolanus enters the foreign city of Antium – perhaps not unlike Shakespeare's arrival in London? – and is refused entry into Aufidius's house:

> "*Coriolanus* A goodly House: The Feast smells well: but I appeare not like a Guest ...

1 Servingman What would you haue Friend? whence are you? Here's no place for you: Pray go to the door ...
2 Servingman Whence are you sir? Ha's the Porter his eyes in his head, that he giues entrance to such Companions? Pray get you out ...
3 Servingman What haue you to do here fellow? Pray you auoid the house.
Coriolanus Let me but stand, I will not hurt your Harth.
3 Servingman What are you?
Coriolanus A Gentleman.
3 Servingman A maru'llous poore one.
Coriolanus True, so I am.
3 Servant Pray you poore Gentleman, take vp some other station: Heere's no place for you . . . Where dwel'st thou?
Coriolanus Vnder the canopy."[18]

There are a number of stories about Shakespeare's arrival in London, but perhaps the most evocative is that told by Robert Shields in the middle of the eighteenth century. Shields claimed that his information originated with William Davenant: "Concerning Shakespear's first appearance in the play-house. When he came to London, he was without money and friends, and being a stranger he knew not to whom to apply, nor by what means to support himself. – At that time coaches not being in use, and as gentlemen were accustomed to ride to the playhouse, Shakespear, driven to the last necessity, went to the playhouse door, and pick'd up a little money by taking care of the gentlemens horses who came to the play; he became eminent even in that profession, and was taken notice of for his diligence and skill in it; he soon had more business than he himself could manage, and at last hired boys under him, who were known by the name of Shakespear's boys: Some of the players accidently conversing with him, found him so acute, and master of so fine a conversation, that struck therewith, they and recommended him to the house."[19]

There are other similar accounts of Shakespeare's employment with horses, written during the middle of the eighteenth century, and although these are too remote in time to be entirely reliable, they confirm what we know from other sources about Shakespeare's poverty and economic destitution during this period. Shields's story has resonance with the account of Coriolanus's reception into the city of Antium quoted above, a similar destitution expressed by Romeo after his banishment from Verona: "Famine is in thy cheekes,/ Need and opression starueth in thy eyes,/ Contempt and beggary hangs vpon thy backe,/ The world is not thy

friend, nor the worlds law:/ The world affords no law to make thee rich."[20]

All this is consistent with what we know about the Shakespeare family's economic circumstances during the 1580's. This barren period is reflected in a number of the plays: in *As You Like It*, Orlando complains of his oppression by his brother Oliver: "for my part, he keepes me rustically at home, or (to speak more properly) staies me heere at home vnkept."[21] Likewise, as we have seen, Arviragus laments his narrow and impoverished circumstances – but the seeds of ambition are contained within this narrowness : "Such wind as scatters yongmen throgh ye world,/ To seeke their fortunes farther then at home,/ Where small experience growes."[22] We will consider the fruits of this ambition in the next chapter.

CHAPTER 13:
THE RISE OF WILLIAM SHAKESPEARE

In one of the earliest biographical accounts of Shakespeare, John Aubrey described what he had been told at Stratford about Shakespeare's youth: "Mr William Shakespear was borne at Stratford vpon Avon, in the County of Warwick; his father was a Butcher, & I have been told heretofore by some of the neighbours, that when he was a boy he exercised his father's Trade, but when he kill'd a Calfe, he would doe it in a *high style*, & make a Speech. There wast that time another Butcher's son in this Towne, that was held not at all inferior to him in naturall wit, his acquaintance & coetanean, but dyed young."[1]

We have seen that John Shakespeare's main occupation was that of a glover, but he was also a dealer in wool, a money-lender and a trader in a number of other commodities. He controlled about 100 acres of land and probably butchered animals from these and other farms, skinning them to provide materials for his glove-making business. There is some independent confirmation of his trade as a butcher: when his religious testament was discovered at the end of the eighteenth century, a report appeared in the popular press for February 1790: "This testament is no farther remarkable, than in proving that John Shakespear was a Butcher, and that he bequeathed all his acquirements in that profession to his son William."[2] Unfortunately, although now accepted as genuine, this testament was not taken seriously at the time of its discovery; the original manuscript has been lost, and we are unable to check the reliability of this report.

Echoes of Shakespeare's apprenticeship to his father are found in Dowdall's description of his visit to Stratford in about 1693: "the clarke that shew'd me this Church is about 80 yrs old; he says that this *Shakespear* was formerly in this Towne bound apprentice to a butcher; but that he Run from his master to London, and there was Recd Into the

playhouse as a serviture."[3] It is likely that the butcher in question was John Shakespeare, and that the flight to London was the result of the deer-poaching incident. As well as mentioning Shakespeare's occupation as a butcher's apprentice, Aubrey also described how "he had been in his younger yeares a Schoolmaster in the Countrey."[4] There has been much speculation about Shakespeare's "lost years", but the only direct information we have comes from the oral tradition, and there is no reason to doubt its veracity on this issue. Shakespeare probably did practice as a country schoolmaster for a while, although he may well have also helped his father on legal matters associated with trading transactions. (His name certainly was linked to his father's in the Lambert dispute.) The most detailed account of these years is that given by Rowe:

> "His Father . . . could give him no better Education than his own Employment. He had bred him, 'tis true, for some time at a Free-School, where 'tis probable he acquir'd that little *Latin* he was Master of: But the narrowness of his Circumstances, and the want of his assistance at Home, forc'd his Father to withdraw him from thence . . . Upon his leaving School, he seems to have given intirely into that way of Living which his Father propos'd to him . . . 'till an Extravagance that he was guilty of, forc'd him both out of his Country and that way of Living which he had taken up."[5]

Therefore it is likely that he was taken away from school at an early age – fourteen? – because of John Shakespeare's difficulties, and that he was apprenticed to his father as a glover/butcher, helping where appropriate with the drafting of bonds and agreements in trading transactions. He also possibly practised as a schoolmaster in the Stratford area – his children were born in Stratford when he was eighteen and twenty-one – perhaps up to the time he left Stratford in his mid-twenties?

But in order to achieve fortune and success, Shakespeare had to renounce the wildness associated with his youth. We have seen how the main dramatic representation of this renunciation, is Hal's rejection of Falstaff, and how this was a metaphor for Shakespeare's rejection of his father's way of life. In *The Merchant Of Venice* and other plays, Shakespeare makes clear his condemnation of usury, in spite of his own father being a money-lender. Shylock embodies all that is morally repugnant and reprehensible, and Shakespeare handles the tension that arises out of this conflict, by creating Jessica's rejection of her father, Shylock: "Alacke, what hainous sinne is it in me/ To be ashamed to be my Fatheres childe/ But though I am a daughter to his blood,/ I am not to his manners."[6]

Although there was a repudiation of the morality of Shylock, there was not a rejection of the ethic of capitalism. Following Max Weber, we may say that Shakespeare renounced the "adventure capitalism" of his father in favour of the more sober, methodical capitalism of the Protestant ethic. He acquired substantial wealth during the 1590's, and carefully invested it in land, property, tithes, as well lending out modest sums at interest. As we have seen, some of this wealth may have come from the gift of one thousand pounds from his patron, the Earl of Southampton, but a substantial proportion undoubtedly accrued through his position as a sharer in both the company and theatre that he worked for. Shakespeare was clearly an astute businessman; and the application for a coat of arms in 1596 was a mark of the success he had achieved in a remarkably short period.

The restoration of the Shakespeare family's fortunes is most clearly delineated in *Cymbeline*. Posthumus reads in gaol of the restoration of Britain's fortunes which echoe Shakespeare's own experience: "When as a Lyons whelpe, shall to himselfe vnknown, without seeking finde, and bee embrac'd by a peece of tender Ayre: And when from a stately Cedar shall be lopt branches, which being dead many yeares, shall after reuive, bee joynted to the old Stocke, and freshly grow, then shall Postumus end his miseries, Britaine be fortunate and flourish in Peace and Plenty."[7]

But there was a negative side to this prosperity. In 1598 Shakespeare became involved in the most controversial of his investments: the hoarding of barley and malt. In order to understand the significance of this, we must appreciate the social changes which were taking place at this time. There had been a significant increase in social polarisation as a result of the demographic and economic changes discussed earlier; in Stratford this had resulted in a sharp rise in the number of poor: a corporation petition stated in 1601 that "our poor are in number seven hundred and odd, young and old" – something like forty per cent of the total population.[8] This increase in poverty was not confined to Stratford: it was a general phenomena throughout England, and perhaps the most vivid illustration of this is the statement made by Phelps-Brown and Hopkins in their study of builders' real wages during the period 1264-1954: "the lowest point we record in seven centuries was in 1597, the year of *Midsummer Night's Dream*."[9]

The problem was compounded in Stratford by the malting industry: barley was used both for malting and for the making of bread, and barley bread was the main food of the poor. As one early seventeenth century

observer stated, "Barley is grown into great use of late years . . . and this, in the dear seasons past, the poor found happy benefit, for they were principally relieved and the labourers also fed by the bread made thereof."[10] Barley prices had increased even more significantly than those for wheat (they stood at an index figure of 600 in the 1590's, compared to 499 for wheat[11]), and these were among the reasons why the government passed legislation forbidding the forestalling and hoarding of grain.

There had been a series of very wet seasons in 1594, 1595 and 1596, resulting in scarcity of corn, vaulting prices, and great hardship. In Stratford, on the 4th November 1595, the malsters were bound over not to make malt, and a corn inquiry was carried out the following month. Although a deeply serious matter, there were comic touches about it: the mayor of Stratford, Master Thomas Rogers, bought a cart-load of barley in order to forestall the market, and when reproached for his behaviour, "doth say that he will *justify it*, and he *careth not a turd for them all.*" Other leading townsmen were also very active, in particular Shakespeare's friends and associates Abraham Sturley and Richard Quiney, who were listed as "great corn-buyers and buyers of wood and such like".[12] As a result of continuing scarcity, in August 1597, the Privy Council called upon justices of the peace to make an inquisition upon all "engrossers", described as "a nomber of wycked people in condicions more lyke to wolves or cormerants than to naturell men."[13]

As a result of this Privy Council Order, a survey off all those holding grain and corn in Stratford was taken in February 1598. Shakespeare was listed as holding ten quarters of corn in his house at New Place; as Roland Lewis has written about this survey: "In the entire list of one hundred and twenty persons set down, only about a dozen held more 'corne and malte' than Shakespeare . . . In effect . . . [he] took advantage of a prospective rising market in 'corne and malte' and had 'engrossed and forestalled' along with some of his fellow townsmen."[14] The results of this forestalling activity nearly led to bloodshed. On the 24 January 1598, Abraham Sturley wrote a letter to his friend Richard Quiney about the whole affair. Quiney was in London on corporation business, and Sturley, immediately after asking Quiney to approach Shakespeare about investing money in local property, wrote:

> "U shall understande, brother, that our neighbours are growne with the wantes they feele throughe the dearnes of corne, which heare is beionde all other countries that I can heare of deare and over deare, malecontent. Thei

> have assembled togeather in a great nomber, and travelld to Sir Tho. Luci on Fridai last to complaine of our malsters; on Sundai to Sir Foulke Gre. and Sir Joh. Conwai. I should have said on Wendsdai to Sir Ed. Grevll first. Theare is a meetinge heare expected tomorrowe. The Lord knoweth to what end it will sorte. Tho. West, returninge from the ij knights of the woodland, came home so full that he said to Mr. Baili that night, he hoped within a weeke to leade some of them in a halter, meaninge the malsters; and I hope, saith Jho. Grannams, if God send mi Lord of Essex downe shortli, to se them hanged on gibbettes att their owne dores."[15]

What the objectors did not realise, was that of the four knights that they protested to, three held stocks of malt in the town on their own behalf. Sir Thomas Lucy was the biggest offender: he held over twelve quarters at Sturley's house (Sturley shared Lucy's puritan sympathies, and at one point worked for him), and sixteen quarters at Richard Dixon's. Sir John Conwayes held over seven quarters "att Rafe Lordes", and Sir Edward Greville held ten quarters, also at Richard Dixon's. Although Sir Fulke Greville did not hold any grain on his own account, his cook, John, held twelve quarters at John Wyllmer's house.[16] It is not suprising that such rebellions had scant chance of success, when the local gentry and magistrates that the protestors looked to for support, were more active in hoarding grain than local townsmen. Also, it explains why the law against forestalling and engrossing was so ineffectual.

The conflicting and contradictory position of the townsmen and local gentry, many of whom were of the puritan persuasion, left them exposed to the charge of hypocrisy. When a dispute over the appointment of the puritan minister, Thomas Wilson, broke out in 1621, his supporters were satirized in the following verse:

> "Stratford is a Town that doth make a great show/ But yet is governed but by a few,/ O Jesus Christ of heaven/ I think that they are but seven/ Puritans without doubt/ For you may know them,/ They are so stout,/ They say 'tis no sin, their neighbour's house to take/ But such laws their father the devil did make .../ One of the Chiefest hath read far in Perkin's works,/ The rest are deep dissembling hypocrites."[17]

Shakespeare would have been more than aware of the popular disturbance over the hoarding of grain. Not only had Sturley asked Quiney to contact him in his letter of 24 January where he described the near-riot, but on the 25 October 1598, Quiney wrote to his "Lovinge good ffrend & contreyman Mr Wm. Shackspere."[18] This letter was probably never sent, but Quiney was clearly very friendly with Shakespeare – his son Thomas, married Shakespeare's daughter Judith – and there is no

doubt that all these events were reported to Shakespeare by his family and friends on his annual visits to Stratford. In the same way that there were links between John Shakspeare, Henry Rogers, Edward Grant, and Sir Thomas Lucy, there was an equivalent association between William Shakespeare, Richard Quiney, Abraham Sturley and Sir Thomas Lucy. The relationship between Shakespeare and Lucy was a critical one for Shakespeare's personal and social development. The rebel had become the upholder of the very authority which had punished and forced him to flee his home town. Shakespeare adopted the same conservative economic and social position as his former antagonist, and in the process came to share the same capitalistic ethic of acquisition.

Both John and William Shakespeare operated on the margins of the social order: John had risen to prosperity and had virtually achieved gentleman status – fratenizing with local gentry, such as Edward Grant and Sir Thomas Lucy – but then had fallen into destitution and social disgrace. His son had risen from poverty and social stigma into the position of a prosperous gentleman. Shakespeare knew both sides of the social divide, which gave him the experience and knowledge which he exploited so effectively in his plays. But this social experience left its mark on him: his work is shot through with ambiguity and conflict over class issues. In *Julius Caesar*, he comes to an acknowledgement of the effects of social mobility on social perception: "But 'tis a common proofe,/ That Lowlynesse is young Ambitions Ladder,/ Whereto the Climber vpward turnes his Face:/ But when he once attaines the vpmost Round/ He then vnto the Ladder turnes his Backe,/ Lookes in the Clouds, scorning the base degrees/ By which he did ascend."[19]

In *Midsummer Night's Dream*, the artisans are all comic characters, but with a sinister hint of class warfare in the background. We have already noted that the year the play was probably written – 1597 – was a year of great scarcity and hardship, and it is generally recognized that the play makes indirect reference to this. Quince in his reaction to Bottom's lion's roar (in 1, ii) – "you should doe it too terribly, you would fright the Dutchesse and the Ladies, that they would shrike; and that were enough to hang vs all" – is essentially comic, but there is concealed menace in the speech. Likewise later in the play (3, i), Bottom, Snout and Starveling play with the theme of violence, ending with Bottom asking for a prologue which "seeme to say, we will do no harme with our swords". To translate threat into comedy was a socially acceptable way of dealing with the tension arising out of a potentially explosive situation – but it was

done at the expense of the artisans, who were seen as suitable for comedy but not serious comment.

It is in the character of Coriolanus, that Shakespeare most embodies the disdain of the successful man for the "base degrees". The play begins with a mutiny over the scarcity of grain, with a population who perceive Coriolanus as "chief enemy to the people". However, although this mutinious population is depicted as fickle, unreliable and open to manipulation by the tribunes, Shakespeare puts speeches into the mouths of citizens, which show considerable sympathy for the plight of ordinary people. Speaking of the rich, the 1st and 2nd Citizens complain:

> "they nere car'd for vs yet. Suffer vs to famish, and their Store-houses cramm'd with Graine: Make Edicts for Vsary, to support Vsurers; repeale daily any wholesome Act established against the rich, and prouide more piercing Statutes daily, to chain vp and restraine the poore . . . We are accounted poore Citizens, the Patricians good: what Authority surfets one, would releeue vs. If they would yeelde vs but the superfluitie while it were wholesome, wee might guess they releeued vs humanely: But they thinke we are too deere, the leanesse that afflicts vs, the object of our misery, is an inuentory to particularize their abundance, our suffering is a gaine to them. Let vs revenge this with our Pikes, ere we become Rakes. For the Gods know, I speake this in hunger for Bread, not in thirst for Reuenge."[20]

This speech and other elements in *Coriolanus* have recently been interpreted by Patterson as referring mainly to various riots which occurred in London, as well as the Midland Revolt of 1607.[21] But of the riots that occurred in London, only two were about food (both in 1595), and they were not about a shortage of grain and basic foodstuffs, but about the price of relative luxuries, fish and butter.[22] Likewise, the Midland Revolt was about the iniquity of enclosures in areas well away from Stratford, and although both series of events may have had an influence on Shakespeare, it is more likely that incidents which directly impinged on his own experience and interest had greater impact. The similarity between the speech quoted above from *Coriolanus* and the sentiments of the Stratford poor reported by Abraham Sturley, are too striking to ignore. Shakespeare had to deal with the tension and ambiguity derived from his situation as a man who had known biting economic adversity, and yet had become a comfortable and successful man of property: in particular, a man who hoarded a large stock of grain during a period of scarcity.

He eventually came down on the side of social order: in *2, Henry VI*, Jack Cade the rebel leader, is depicted as unscrupulous, and ultimately

only interested in power and position for himself. There are hints of Orwell's *Animal Farm* in Shakespeare's treatment of this character, and it could be argued that Shakespeare's analysis of rebellion and social order derived ultimately from a realistic understanding of the political-economy of power. But this raises questions beyond the scope of this book, and all we need note here, is the influence of Shakespeare's own position on his political and social perceptions.

In *Timon Of Athens*, Shakespeare reveals the association between personal riot and the breakdown of the social order, a theme which as we have seen, came out of his own experience. Timon in railing against Athens, puts the following curse on the city: "Lust, and Libertie/ Creepe in the Mindes and Marrowes of our youth/ That 'gainst the streame of Vertue they may striue/ And drowne themselues in Riot . . . Piety, and Feare,/ Religion to the Gods, Peace, Justice, Truth,/ Domesticke awe, Night-rest, and Neighbour-hood,/ Instruction, Manners, Mysteries, and Trades,/ Degrees, Obseruances, Customes, and Lawes,/ Decline to your confounding contraries./ And yet Confusion liue."[23] And this prepares the way for Shakespeare's great speech on the breakdown of degree and social order:

> "O, when Degree is shak'd,/ (Which is the Ladder to all high designes)/ The enterprize is sicke. How could Communities,/ Degrees in Schooles, and Brother-hoods in Cities,/ Peacefull Commerce from diuidable shores,/ The primogenitiure, and due of Byrth,/ Prerogatiue of Age, Crownes, Scepters, Lawrels,/ (But by Degree) stand in Authentique place?/ Take but Degree away, vn-tune that string,/ And hearke what Discord followes: each thing meetes/ In meere oppugnancie. The bounded Waters/ Should lift their bosomes higher then the Shores,/ And make a soppe of all this solid Globe:/ Strength should be Lord of imbecility,/ And the rude sonne should strike his Father dead:/ Force should be right, or rather, right and wrong,/ (Between whose endlesse jarre, Justice recides)/ Should loose her names, and so should Justice too./ Then euerything includes it selfe in Power,/ Power into Will, Will into Appetite,/ And Appetite (an vniuersall Wolfe,/ So doubly seconded with Will, and Power)/ Must make perforce an vniuersall prey,/ And last, eate up himselfe."[24]

Here Shakespeare surpasses himself with the brilliance of his imagery and language, surely an unrivalled statement of the conservative case for social and political order. In creating this statement however, he sublimated the tensions and ambiguities of his own personal situation. The speech resonates with aggression and rage, but this in no way detracts from the scale of his achievement. All thinkers who have addressed the problem of human nature and its social containment have had to deal with

the issue of aggression and violence, and Shakespeare's individual experience in no way limits the universal applicability of this speech. However, underneath this conservative apotheosis, the personal and social tensions remained – and this is the subject of our final and concluding chapter.

CHAPTER 14:
THE END OF EXILE

The play in which Shakespeare explored most directly the tensions arising out of violence was *Hamlet*. Ernest Jones and others have analysed the play's psychological dimensions, and it has been examined from a number of other perspectives, all of which have brought out its great richness and complexity. What has yet to be undertaken, is a sociological analysis of the context of the play, something which will enable us to examine certain key strands in Shakespeare's own biography. Although there are a number of themes in the play, the interplay between murder and suicide is one of its central ones. *Hamlet* has mainly been interpreted in terms of the motivations of individual characters, but Shakespeare expressed much more than the personal in the play. It reflected a general cultural shift which was taking place at the time: a historical decline in the overall level of overt violence which occurred in England from about the thirteenth century onwards.[1]

The reasons for this diminishing violence are complex: one factor may have been the growth of literacy, particulary in urban areas, but another factor has been noted by J. B. Given in his study of homicide in thirteenth century England. He found that the murder rate was lower in large towns, such as London, than in rural areas, and has argued that this can partly be explained by the culture of individualism which had developed in the towns. Much homicide was communal in nature – a residue of the blood feud – and paradoxically, the relative social isolation of the townsman, led to a reduction in the level of violence.[2] But a decrease in homicide was associated with a growth in suicide. Durkheim showed in his classic study of suicide, that the individualism associated with town life, frequently engendered what he termed "egoistic" suicide: suicide resulting from excessive self- preoccupation, over-intellectualisation, and an inability to

act[3]: all characteristics associated with Hamlet, and also shown in some of Shakespeare's sonnets, particularly sonnet 62.

The atmosphere and culture of late sixteenth century London was in certain respects unique: all the evidence is that London was a much more literate and less violent place than the country at large. Sharpe in his study of crime in early modern England noted from an analysis of indictments for felony during 1550-1625, that the metropolitan county of Middlesex showed "differences from those derived from rural areas." Of all those indicted for felonies, only five per cent of those in Middlesex were for homicide and infanticide, whereas the figure in more rural counties were mainly in the range of nine to sixteen per cent.[4] Although there were a number of popular disorders in late sixteenth century London – there were at least ninety-six outbreaks in London between 1517 and 1640, of which thirty-five took place between 1581 and 1602[5] – most of these were not of a violent nature. John Graunt in his study of the London bills of mortality, provided a series of figures confirming London's relatively non-violent reputation. During the eight-year period 1629-36, twenty-nine people were murdered, compared fifty-five people committing suicide, giving a homicide rate of 1.51 per 100,000 and a suicide rate of 2.86.[6] The homicide rate was very low compared to that for the rest of the country: in thirteenth century England it was in the range of 9.0 to 47.0 per 100,000, while other research indicates a rate for the country as a whole during the sixteenth and seventeenth century period of 5.0 – 18.0 per 100,000.[7] The low homicide figure for London persisted through the seventeenth and into the eighteenth century, and the rate was very similar to that for England today. That this low murder rate was genuine was confirmed by Graunt, who was born in London, and lived in the city through the period in question. In attempting to explain the very small number of murders in London, Graunt wrote in 1662:

> "My next Observation is; That but few are *Murthered*, viz. not above 86 of the 22950 which have died of other diseases [in the period 1629-58], and casualities; whereas in *Paris* few might scape without their *Tragedie*. The Reasons of this we conceive to be *Two: One is the Government*, and *Guard* of the City by *Citizens* themselves . . . And the other is, The natural, and customary abhorrance of that inhumane *Crime*, and all *Bloodshed* by most *Englishmen:* for all that are executed, few are for *Murther* . . . In brief, when any dead Body is found in *England*, no *Algebraist*, or *Uncipherer* of Letters, can use more subtile, and varitie of conjectures to finde out the Demonstration, or Cipher; then every common unconcerned person doth to finde out the Murtherers, and that for ever, untill it be done."[8]

Graunt's belief in the "customary abhorrence" against "all bloodshed" may have been typical of most Londoners, but it was not characteristic of the English population as a whole. His explanations for this very low rate in London are not very convincing. No doubt having the city policed by its own citizens helped reduce the frequency of murder to some extent, but perhaps more important was the relative anonymity of a large city like London – social distance minimizes emotional contact and the basis of dispute. (London's population doubled in the last two decades of the sixteenth century, and the majority of its population were immigrants – and therefore, in the main, strangers to each other.) Additionally, Londoners were a very well-educated population compared to the country generally; whereas 72 per cent of the latter were unable to sign the Protestation Oath of 1642, "the two London parishes for which usable returns survive have the remarkably low rate of 28 per cent."[9] The inhibiting effect of education and rational thought – Hamlet's "pale cast of thought" – operated in London's more literate culture. (This was reinforced by the puritanism of the city corporation – there being a general association between literacy and the spread of puritanism.) The "abhorrence" against "all bloodshed" was of course a matter of degree: the bear and bull-baitings that took place near the Globe Theatre in Southwark, are reminders that London was not an entirely unviolent city.

In this context, Ernest Jones's reference to Hamlet as the first modern man is very apt: Hamlet, whose "hue of resolution/ Is sicklied o'er with the pale cast of thought", embodies the irreconcilable conflict between the aggressive and the rational. Aggression inhibited by rational restraint is invariably internalised, and turned against the self: it is partly for this reason that most modern societies have relatively low homicide but high suicide rates.[10] Aggression directed inwards leads to self-denigration, resulting in melancholy and depression, culminating in *extremis* in suicide – all feelings and reactions experienced by Hamlet. Shakespeare himself expressed similar feelings of self-depreciation and personal unworthiness in some of the sonnets.

However, we must not exaggerate the decline in overt violence during Shakespeare's period; for example, Gough recorded ten murders in and around the village of Myddle in Shropshire for the seventeenth century. Although not all the murders took place in the village itself, it only had a population of about six hundred people, and therefore was a very violent place by modern standards. We do not have enough information on the sociological history of Stratford, to know whether it was as violent as this

Shropshire village, although on an anecodotal level, this appears to have been the case. For example, Shakespeare had bought New Place in 1597, and the two previous owners, William Bott and William Underhill, were both involved in murders: Bott poisoned his own daughter, and Underhill in turn was poisoned by his eldest son. (It is possible that these incidents influenced Shakespeare's choice of the method of murder for King Hamlet.) Likewise, Shakespeare's friend and associate, Richard Quiney, died in 1602 after having been wounded in a drunken brawl with some of Sir Edward Greville's men.[11] We have already seen how frequent were the fights between townsmen in Stratford, and in order to attempt to control physical violence, the corporation on more than one occasion banned single men from carrying weapons in the town.

Thus Shakespeare's move from Stratford to London was cultural as well as geographical. The movement between the two worlds is echoed in the description of Hal's transformation in *Henry V:* "His Houres fill'd vp with Ryots, Banquets, Sports;/ And neuer noted in him in any studie,/ Any retyrement, and sequestration . . . Consideration like an Angell came/ And whipt th'offending *Adam* out of him . . . Neuer was such a sodaine Scholler made:/ Neuer came Reformation in a Flood ..." All that we know about Shakespeare suggests that he underwent a similar transformation; having spent his youth in "wild riot", he became in his maturity, a man of reserved and private habits. Aubrey in the material he collected for his *Brief Lives* (probably from the actor Beeston), described Shakespeare's social life: "he was not a company keeper [-] lived in Shoreditch, wouldnt be debauched, & if invited to [-] writ; he was in paine."[12]

The evidence from the Belott-Mountjoy suit confirms this picture of a private and modest life-style: Shakespeare lodged with the Mountjoys, who were a Huguenot family, and helped arrange the marriage between Mountjoy's daughter and his apprentice Bellott. Shakespeare's relative isolation is suggested by the tone and substance of a number of the sonnets – "I all alone beweep my outcast state" – and this was an expression of the sense of exile discussed earlier. This however must be qualified by the anecdotes which describe Shakespeare enjoying the company of friends. The sonnets suggest that most of his friendships were intensely private, although with fellow writers such as Jonson and Drayton, he is reported to have spent time in drinking and social companionship, particularly in later life.[13]

It is perhaps more apt to see Shakespeare living in a transitional period, when the "blood and roses" of the medieval world were beginning to give way to the more constrained and literate culture of town life. Nevertheless, Shakespeare's place of origin, Stratford, was in many respects similar to Gough's rural world – a world which had not yet been "civilised", a world of raw physical and social intensities, which modern man would find very frightening. Shakespeare in fleeing from Stratford, left behind this raw, physical world, with its passionate acting out of impulse and feeling, and entered Freud's "civilization and its discontents". This new civilized world was one of culture and great artistic creativity, but it carried with it all the personal tensions and neuroses so painstakingly depicted by Freud.

Shakespeare's attempt to resolve these tensions culminated in *The Tempest*. We have seen that Shakespeare partially achieved this by "splitting" his own biographical presence into the characters of Prospero and Caliban. Prospero's power is used in the service of moral revenge against all those who have usurped his rightful position, and have been responsible for his exile. This allowed Shakespeare to finally sublimate his own aggression into a morally acceptable position – although at the cost of having created Caliban, a metaphor for alienated man. *The Tempest* announces the end of exile: "I'le breake my staffe,/ Bury it certaine fadomes in the earth,/ And deeper than did euer Plummet sound/ Ile drowne my booke."[14] This renunciation of Prospero's power – based on his command of the written word – signals Shakespeare's retirement from the stage and his return to his home town, Stratford.

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Before returning to Stratford, Shakespeare had to bring about a psychological reconciliation with his wife, and this was achieved in *The Winter's Tale*. Leontes, having effectively killed his wife Hermione, is finally forgiven: "Sir, you haue done enough, and haue perform'd/ A Saint-like Sorrow:/ No fault could you make,/ Which you haue not redeem'd; indeed pay'd downe/ More penitence then done trespass: At the last/ Doe as the Heauens have done; forget your euil,/ With them, forgiue your selfe."[15] In the final extraordinary scene in the play, Hermione, who has been pretending to be a statue for twenty years, comes to life, and Shakespeare describes the reconciliation between her and Leontes through Paulina's speech as follows:

> "Musick; awake her: Strike:/ 'Tis time: descend: be Stone no more: approach:/ Strike all that looke vpon with meruaile: Come:/ Ile fill your Graue vp: stirre: nay, come away:/ Bequeath to Death your numnesse: (for from him/ Deare Life redeemes you) you perceiue she stirres:/ Start not: her Actions shall be holy, as/ You heare my Spell is lawfull: doe not shun her,/ Vntil you see her dye againe; for then You kill her double: Nay, present your Hand:/ When she was young, you woo'd her: now, in age,/ Is she become the Suitor?"[16]

There is a note of ambiguity in this reconciliation: although Hermione is brought back to life, there is a hint of reluctance on Leontes's part, as he hesitates to take her hand – perhaps an ambiguity reflected in the famous "second-best bed" that Shakespeare left to his wife? On retirement, Shakespeare went back to live in Stratford, and so returned to live with his wife after a twenty-year-or-so absence. Rowe described his retirement as follows:

> "The latter Part of his Life was spent, as all Men of good Sense will wish theirs to be, in Ease, Retirement, and the Conversation of his Friends. He had the good Fortune to gather an Estate equal to his Occasion, and, in that, to his Wish; and is said to have spent some Years before his Death at his native *Stratford*. His pleasurable Wit, and good Nature, engag'd him in the Acquaintance, and entitled him to the Friendship of the Gentlemen of the Neighbourhood. Amongst them, it is a Story almost still remember'd in that Country, that he had a particular Intimacy with Mr *Combe*, an old Gentleman noted there-abouts for his Wealth and Usury: It happen'd, that in a pleasant Conversation amongst their common Friends, Mr *Combe* told *Shakespear* in a laughing manner, that he fancy'd, hed intended to write his Epitath, if he happen'd to out-live him; and since he could not know what might be said of him when he was dead, he desir'd it might be done immediately: Upon which *Shakespear* gave him these four verses. *Ten in the Hundred lies here ingrav'd,/ 'Tis a Hundred to Ten, his Soul is not sav'd:/ If any Man ask, Who lies in this Tomb?/ Oh! ho! quoth the Devil, 'tis my* John-a-Combe. But the Sharpness of the Satyr is said to have stung the Man so severely, that he never forgave it."[17]

Rowe's account indicates that Shakespeare had achieved in retirement a form of resolution to the various personal and social tensions which had bedevilled his earlier life. His sociability and enjoyment of the company of good friends, hints at a life of contentment, but even in this, there is a note of ambiguity: the satire on Coombe, who had puritan leanings, is a reminder of the ambivalence of Shakespeare's relationship with his father, who like Coombe, had been a money- lender. Also, within the good companionship and enjoyable sociability, there was a hint of an old problem, that seems never to have entirely disappeared. There are a

number of stories within the traditional *corpus*, that mention Shakespeare's drinking bouts in Stratford and surrounding villages. The strongest and most important is that given by John Ward, who was vicar of Stratford during the years 1662-81. Ward kept note-books and some time during 1661-63, made the following note about Shakespeare's death: "Shakespear, Drayton, and Ben Jhonson, had a merry meeting, and it seems drank too hard, for Shakespear died of a feavour there contracted . . ."[18]

How ironic, given his father's history, that Shakespeare is reported to have died from excessive drinking. The play in which Shakespeare's own attitude to drink is explored most directly is *The Tempest*. Caliban is the repository of all that is dark and malignant: he attempts to rape Miranda, is treacherous, and plans to murder Prospero. His malignacy is fed by addiction to drink, to which he is introduced by Stephano and Trinculo. He comes to worship Stephano, a drunken butler, as a result of drinking from "a butt of sack"; he offers to render Stephano all the services that he has been forced to give to Prospero. As Trinculo says of him: "A most rediculous Monster, to make a wonder of a poore drunkard."[19] And at the end of the play, Caliban comes to renounce Stephano, in the way that Hal rejects Falstaff: "what a thrice-double Asse/ Was I to take this drunkard for a god?/ And worship this dull foole?"[20]

As we have seen, Caliban is Prospero's "shadow" – "this thing of darkness I/ Acknowledge mine" – and as Prospero is widely recognized as a character with strong autobiographical associations, Caliban can be seen as a metaphor for Shakespeare's own "dark side". The power that Prospero has over Caliban resides in his books: "Remember/ First to possesse his Bookes; for without them/ Hee's but a Sot, as I am"[21] – a remarkable confirmation of the linkage between Prospero and Caliban, and the hidden world of alcoholism. It is as if there is a war between the world of the "book" and that of the "bottle": both having an almost magical power over men's lives, and Caliban pleads for Stephano to remove Trinculo's bottle, in the same way that he demands the seizure of Prospero's books. We see here a re-surfacing of the issue of wildness and its containment by rationality: a containment which was always very fragile, and which was constantly under threat of breakdown through alcoholic drink. But perhaps this ambiguity and ambivalence is an inevitable condition of "civilized life", and a point has now been reached where we must sum up and draw conclusions for our understanding of Shakespeare's biography.

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John Shakespeare stands at the centre of this book, and he is the key figure in the life and writings of his son William. Not only did he have a profound effect through his larger-than-life personality – his wit, his sophistication and gargantuan appetite for life – but created the conditions responsible for Shakespeare's exile from his home and family. John Shakespeare as Falstaff embodied physicality, spontaneity, and all the qualities that Freud described in the concept of the "id". He also displayed the cunning and intellectual virtuosity of the cosmopolitan trader – Freud's "ego" – and we can see in John Shakespeare the two worlds which were to shape Shakespeare's life: the raw and violent physicality of Stratford, and the intellectual and cultural world of London. Running through these two worlds was a common thread: an addiction to drink, with all that it represented – not only the sociability of the inns of Stratford, London and elsewhere, but the eventual decline into poverty and social humiliation.

For Shakespeare, this licentiousness was mirrored by the wildness of his own youth: the poaching of deer and rabbits, his rebelliousness against authority, and the youthful enjoyment of sexual pleasure, culminating in Anne Hathaway's pregnancy and early marriage. There was a direct parallel between John Shakespeare's addiction to drink, and his son's love of sport and pleasure. Falstaff as a figure comes to embody both these strands, and his rejection represents not only a turning away from John Shakespeare and his way of life, but a repudiation of Shakespeare's own wildness. In the way that John Shakespeare had undergone "gravity's revolt to wantoness", Shakespeare experienced the reverse: from "Ryots, Banquets, Sports" to a "sodaine Scholler made". The linking of opposites is a commonplace, with a psychological affinity between the puritan and the drunkard – expressed so well by Caliban in his description of Prospero: for without his books, "Hee's but a Sot, as I am."

Shakespeare's flight to London was the direct result of his poaching activities, but was associated with his abandonment of his wife in Stratford. In the process, he became a private man: the outcast individual, diligently pursuing his calling, and in many ways his new life was characterized by Weber's protestant ethic: resulting in a methodical pursuit of economic success and wealth. Behind this quest for prosperity lay the shadow of social humiliation deriving from his father's poverty

and debt – felt all the more keenly because of the prosperity and success which had preceeded it. In this respect, Shakespeare's life is yet a further example of Edmund Wilson's "wound and the bow": the wound of personal trauma fuelling the drive for artistic success. For Shakespeare, much of this ambition was strictly economic: the restoration of family fortunes became a key imperative, and as Pope observed, much of the motivation for writing the plays was commercial rather than aesthetic. (Thus the fact that only about half the plays were published in Shakespeare's lifetime – their non-publication representing a commercial advantage in an age when a monopoly of material for stage production was more important than revenue from the sale of books.)

But beyond the realm of the economic, Shakespeare's work is an affirmative expression of "the uses of adversity". Out of the misfortunes of his exiled state, came the striving to overcome his afflicted condition. There is something of a quest for superiority to compensate for feelings of inferiority in this – as described by Adler – but perhaps Nietchze's "will to power" is a more appropriate metaphor, particularly as represented in the figure of Coriolanus. The result of this striving was a steady accumulation of capital and the eventual restoration of family fortunes. From this accumulation, came the portly figure – bourgeois, successful and comfortable – depicted in the bust placed in Stratford church. Shakespeare embraced capitalism just as enthusiastically as his father, although with him it was less adventure capitalism, and more the methodical variety associated with the protestant ethic. (It is perhaps no accident that he had lodged with a Huguenot family, was friendly with the puritan Coombe family at the end of his life, and had a son-in-law and daughter with puritan leanings.)

Beyond this comfortable puritanism, lay the profound consciousness of "modern man": an existential awareness of the ultimate aloneness of the individual, with a resulting sense of exile. But even here, John Shakespeare played a pivotal role: his death ushered in the "dark period", in which Shakespeare wrote all the great tragedies, with a series of tragically afflicted heroes. Yet Shakespeare was able to transcend the exile and alienation – "my outcast state" – through a self-affirmation, which used the adversities and difficulties of his youth to create the successes of his later, mature life. Shakespeare's genius was to translate the culture of his early life – characterized by all its intensities – into the new literacy associated with his London experience, using his "natural wit" to create the great comedies, histories and tragedies. In this respect, we can say

that he followed Freud's dictum: "where id was, ego shall be" – with all the ambiguities and tensions that inevitably result. In the process, he sublimated his own inner torments and conflicts, to achieve artistic and economic success, returning to Stratford and bringing about an end to exile. It is perhaps therefore appropriate, to conclude with a quote from Shakespeare, which epitomises the fundamental self-affirmation which lies at the centre of his life and work:

> "Sweet are the uses of adversity, which like a toad, ugly and venemous, wears yet a precious jewel in his head. And this our life, exempt from public haunt, finds tongues in trees, books in the running brooks, sermons in stones, and good in everything."[22]

FOOTNOTES

CHAPTER 1: INTRODUCTION

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CHAPTER 2: THE RISE OF JOHN SHAKESPEARE

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2 D.L.Thomas and N.E.Evans, "John Shakespeare In The Exchequer", *Shakespeare Quarterly*, Vol. 35 (1984), p.318
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4 *The Victoria County History Of Warwickshire*, Vol. 3 (1945), p.236
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7 *Ibid*
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11 Joan Thirsk (Ed), *The Agrarian History Of England And Wales, Vol. 4: 1500-1640* (1967), pp 857, 858; E.H.Phelps-Brown and Sheila V.Hopkins, "Seven Centuries Of The Prices Of Consumables,

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14 Victor Skipp, *Crisis And Development: An Ecological Case Study Of The Forest Of Arden, 1570-1674* (1978), pp 13, 47

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16 *Ibid*, p.62

17 *Ibid*, p.70

18 *Ibid*, p.70

19 *Ibid*, p.71

20 *Ibid*, p.56

21 *Ibid*, pp 70, 72

22 Robert Finlay, *Population And Metropolis: The Demography Of London* (1981), pp 6, 51

23 J.M.Martin, *Population And Mortality In Tudor And Stuart Stratford* (1972) (Manuscript thesis in the Warwickshire County Record Office), p.45

24 *The Registers Of Stratford-On-Avon*, Ed. Richard Savage (Parish Register Society, 1897-1905)

25 *Ibid;* Edgar Fripp, *Shakespeare: Man And Artist*, Vol.1 (1938), p.40; Vol.2 (1938), pp 491, 492

26 Finlay, *op.cit.*, p.9

27 See Edgar Fripp, *Master Richard Quyney* (1924), p.103; B.Rowland Lewis, *The Shakespeare Documents*, Vol.1 (1940), pp 281, 285

28 Fripp, *Quyney op.cit.*, p.104

29 I have used the Phelps-Brown & Hopkins price index for the period 1570-79 as the base figure, and inflated it using their series up to 1954. See Phelps-Brown & Hopkins, pp 193-195. For the post-1954 period I have used *Department of Employment, Retail Price Indices 1914-1986* (HMSO, 1987), p.1, and the *Central Statistics Office, Annual Abstract of Statistics* (1990), p.322, and *Employment Gazette, February 1990*, p.55. The inflation ratio of 140 is a conservative one, in particular with respect to house prices, which are not included in the ratio. House prices rose much more significantly than other prices: for example, Shakespeare appears to have paid about £60 for New Place in 1597; an equivalent house today would be worth at least £300,000, giving an inflation ratio of 500.

30 Skipp, *op.cit.*, p.9
31 Lewis, *op.cit.*, pp 149-152
32 *Ibid*, p.130
33 *Ibid*, p.53
34 *Ibid*, p.64
35 Thirsk, *op.cit.*, p.857
36 Richard Savage and Edgar Fripp (Eds), *Minutes And Accounts Of The Corporation Of Stratford-Upon-Avon And Other Records, Vol.1: 1553-1566* (Dugdale Society, 1921), pp 47,48
37 *Ibid*
38 Lewis, *op.cit.*, p.63
39 Thomas & Evans, *op.cit.*. The following discussion is based on this article.
40 Lewis, *op.cit.*,p.68
41 *Ibid*, p.64
42 *Ibid*

CHAPTER 3: JOHN SHAKESPEARE'S CULTURAL WORLD

1 Lewis, *op.cit.*, p.139
2 Alan Everitt, "The Marketing of Agricultural Produce", in Joan Thirsk, *op.cit.*, pp 543-563. The discussion of free individual trading relies heavily on Everitt's work.
3 *Ibid*, p.543
4 Peter J.Bowden, *The Wool Trade In Tudor And Stuart England* (1962), p.82
5 *Ibid*
6 *Ibid*, p.91
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9 Everitt, *op.cit.*, p.557
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13 *Ibid*
14 S.Schoenbaum, *William Shakespeare: A Compact Documentary Life* (1987), p.37

15 Lewis, *op.cit.*, p.230
16 Halliday, *op.cit.*, pp 399, 400
17 Lewis, *op.cit.*, p.54
18 See Savage & Fripp, *op.cit.*, Vol.1 (1921) & Vol.2 (1924)
19 *Ibid*, Vol.2 (1924)
20 *Ibid*, p.112
21 *Ibid*, Vol.3 (1926), p.10
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24 Savage & Fripp, *op.cit.*, Vol.3, p.170
25 M.Eccles, *Shakespeare In Warwickshire* (1961), p.27
26 Chambers, *William Shakespeare*, Vol.2, p.20
27 Savage & Fripp, *op.cit.*, Vol.2, p.xlv
28 See for example *Ibid*, Vol.3, p.10
29 *Ibid*, Vol.1, pp 128, 138, 139
30 H.Mutschmann and K.Wentersdorf, *Shakespeare And Catholicism* (1952), p.39
31 *Ibid*, p.42
32 Savage & Fripp, *op.cit.*, Vol.1, p.xxxix
33 *Victoria County History Of Warwickshire*, Vol.3 (1945), p.281
34 Savage & Fripp, *op.cit.*, Vol.2, p.92
35 *Ibid*, Vol.4 (1929), p.13
36 See *Ibid*, Vols. 1-4
37 Fripp, *Master Richard Quyney*, pp 47, 48
38 See Savage & Fripp, *op.cit.*, Vols. 1-4 for many references
39 Fripp, *Shakespeare Man And Artist*, Vol.1, p.156
40 *Ibid*, p.151
41 *Ibid*, p.78
42 Mutschmann & Wentersdorf, *op.cit.*, p.44
43 *Ibid*, p.46
44 For a discussion of this, see Schoenbaum, *op.cit.*, pp 45-53
45 Eccles, *op.cit.*, pp 33, 34
46 *Ibid,*, p.33
47 Savage & Fripp, *op.cit.*, Vol.4, pp 57-60
48 Eccles, *op.cit.*, p.33
49 *Ibid,*, p.33

CHAPTER 4: THE SHAKESPEARE/LAMBERT DISPUTE

1 Lewis, *op.cit.*, p.135
2 *Ibid*, p.113
3 *Ibid*, pp 132, 133
4 *Ibid*, p.133
5 *Ibid*, pp 138, 139
6 "Shakespeare In The Public record Office", *Public Record Office Handbook*, No.5 (1964), p.6
7 Lewis, *op.cit.*, pp 140, 141
8 *Ibid*, pp 142, 143
9 *Ibid*, p.144
10 Eccles, *op.cit.*, p.29
11 Schoenbaum, *op.cit.*, p.39
12 Halliday, *op.cit.*, p.443
13 Lewis, *op.cit.*, p.145
14 *Ibid*, pp 141, 143
15 Schoenbaum, *op.cit.*, pp 40, 41
16 Eccles, *op.cit.*, p.31
17 Schoenbaum, *op.cit.*, pp 15, 16
18 See Savage & Fripp, *op.cit.*, Vols 1-4
19 See P.E.Razzell, "Introduction" in Richard Gough, *History Of Myddle* (Caliban Books, 1979)

CHAPTER 5: JOHN SHAKESPEARE AS FALSTAFF

1 Savage & Fripp, *op.cit.*, Vol.4, p.161
2 Schoenbaum, *op.cit.*, p.332
3 J.O.Halliwell-Phillips, *The Life Of William Shakespeare* (1848), p.127
4 Eccles, *op.cit.*, p.34
5 *Ibid*
6 Chambers, *op.cit.*, p.247. The only known Sir John Mennis alive in this period was far too young (he was born in 1599) to have met Shakespeare's father, who died in 1601. However, as Eccles points out,"Mennis, who collected poems and anecdotes, may have borrowed the story from an earlier visitor to Stratford." Eccles, *op.cit.*, p.71. As Mennis was a friend of Sir William D'Avenant, reputed

illegitimate son of Shakespeare, it is possible that D'Avenant was the source. The authenticity of the story is indicated by Plume/Mennis's knowledge of John Shakespeare's occupation as a glover – something no other contemporary writer was aware of.

7 E.K.Chambers, *Sources For A Biography of Shakespeare* (1946), p.10; Lewis, *op.cit.*, p.8

8 W.H.Auden, "The Prince's Dog" in G.K.Hunter (Ed),*Shakespeare Henry IV Parts 1 and 2* (1970), pp 187, 188

9 Chambers, *William Shakespeare*, Vol.2, p.266. The Reference to £1,000 also occurs in *Much Ado About Nothing*, 1, i and 3, iv

10 R.B.Wheeler, *History And Antiquities of Stratford-Upon-Avon* (1806), p.73

11 Chambers, *op.cit.*, p.20. £500 was the sum that Shallow asked back from Falstaff when he discovered that Falstaff was to be banished.

12 *2, Henry IV*, 733-735,in *The First Folio Of Shakespeare* (Norton Facsimile Edition, edited by Charlton Hinman, 1968). All references to Shakespeare's plays will be to this Folio edition, using the line numbering system, unless otherwise stated. All other references to Shakespeare's sonnets and poems will be from Peter Alexander, *William Shakespeare: The Complete Works* (1951).

13 Chambers, *op.cit.*, pp 19, 21

14 *Ibid*, pp 26, 28-32; Eccles, *op.cit.*, p.17

15 Schoenbaum, *op.cit.*, p.229

16 The reference to Jonson's visit to Stratford was for the year of Shakespeare's death, but it is possible that he had made earlier visits. According to John Ward, Vicar of Stratford in 1662-81, "Shakspear, Drayton, and Ben Jhonson, had a merry meeting, and itt seems drank too hard, for Shakespear died of a feavour there contracted." See Halliday, *op.cit.*, p.520

17 Ronald McKerrow (Ed), *The Works Of Thomas Nashe*, Vol.1 (1966), p.171

18 *1,Henry IV*, 1183

19 *2,Henry,IV*, 2353-2361

20 *Henry V*, 2267-2270

21 *Ibid*, 2627-2633

22 *Ibid*, 2934-2947

23 *2,Henry,IV*, 3231-3234 & 3247-3266

24 *Henry V*, 587

25 Eccles, *op.cit.*, p.35

CHAPTER 6: THE FALL OF JOHN SHAKESPEARE

1 Fripp, *Shakespeare Man & Artist*, p.151
2 *Hamlet* (Quarto Edition, 1604), 1, iv
3 Harold Jenkins (Ed.), *Hamlet* (The Arden Edition, 1982), p. 448
4 McKerrow, *op.cit.*, p.205
5 *Othello*, 1188-1197
6 *The Rape Of Lucrece*
7 *Timon Of Athens*, 832-837
8 *Ibid*, 657-659
9 Everitt, *op.cit.*, pp 567, 568
10 Lewis, *op.cit.*, p.241
11 Everitt, *op.cit.*, p.568
12 Quoted in Max Weber, *The Protestant Ethic And The Spirit Of Capitalism* (1930), p.175
13 Everitt, *op.cit.*, p.572
14 Savage and Fripp, *op.cit.*, Vol.1, pp xxi, xxii
15 *Ibid*, Vol.4, p.165
16 Fripp, *Master Richard Quyney*, p.133
17 Halliwell-Phillips, *Outlines*, Vol.1, p.142

CHAPTER 7: THE DEATH OF JOHN SHAKESPEARE AND THE WRITING OF HAMLET

1 Halliday, *op.cit.*, p.204
2 Jenkins, *op.cit.*, pp 573, 574
3 *Ibid*, p.4
4 L.Kirschbaum, "The Date Of Shakespeare's *Hamlet*", *Studies In Philology*, Vol.34 (1937)
5 *Hamlet*, 2, ii
6 See Fredson Bowers, *Introductions, Notes And Commentaries To Texts In 'The Dramatic Works Of Thomas Dekker'*, Vol.1 (1980), pp 179-196 for this and other information on the dispute.
7 C.H.Heford and Percy Simpson, *Ben Jonson*, Vol.4 (1980), p.203
8 *Ibid*, p.251
9 *Ibid*, p.255
10 *Ibid*, pp 255, 256

11 *Ibid*, pp 320, 321
12 Bowers, *op.cit.*, p.180
13 *Ibid*, p.181, fn 5
14 Quoted in *Ibid*, p.196
15 Halliday, *op.cit.*, p.157
16 *Hamlet* (First Quarto, 1603), Scene vii
17 *Hamlet*, 780-789
18 *Hamlet*, 250-288
19 *Hamlet*, 1342-1346
20 *Hamlet*, 2917-2919 & 2941-2948
21 *Hamlet* (Second Quarto, 1604), 5, i. There are echoes of this speech in *Titus Andronicus:* "*Lucius:* Come hither, boy; come, come, and learn of us/ To melt in showers. Thy grandsire lov'd thee well;/ Many a time he danc'd thee on his knee,/ Sung thee asleep, his loving breast thy pillow;/ Many a story hath he told to thee,/ And bid thee bear his pretty tales when he was dead and gone. *Marcus* How many thousand times hath these poor lips,/ When they were living, warm'd themselves on thine!" (5, iii). This is one of the few passages which can read as referring to Shakespeare's own son; another such passage occurs in *The Winter's Tale* (1, ii), where Leontes and Polixenes compare their feelings for their sons: "*Leontes* . . . Are you so fond of your young Prince, as we/ Doe seeme to be of ours? *Polixenes* If at home (Sir)/ He's all my Exercise, my Mirth, my Matter;/ Now my sworne Friend, and then mine Enemy;/ My Parasite, my Souldier: States-man; all:/ He makes a Julyes day, short as December,/ And with his varying child-nesse cures in me/ Thoughts, that would thick my blood."

CHAPTER 8: THE DEER POACHING TRADITION

1 See *The Merry Wives Of Windsor*, lines 1835-1836; Chambers, *op.cit.*, p.264.
2 Chambers, *op.cit.*, p.257
3 *Ibid*, p.265
4 *Ibid*, pp 289, 290
5 *Ibid*, p.293
6 *Ibid*, p.296

7 *Ibid*, p.297
8 *Ibid*, p.301
9 Alice Fairfax-Lucy, *Charlecote And The Lucys* (1958), p.279
10 C. Holte Bracebridge, *Shakespeare No Deerstealer* (1862)
11 J.O.Haliwell-Phillips, *Observations on The Charlecote Traditions* (1887), p.23
12 Chambers, *op.cit.*, p.299
13 Halliday, *op.cit.*, p.291

CHAPTER 9: SIR THOMAS LUCY'S DEER PARKS

1 Jane Croom, *The Medieval Deer Parks Of Warwickshire* (BA Dissertation, Birmingham University 1983)
2 *Ibid;* M.W. Beresford, "The Deserted Medieval Villages In Warwickshire", *Transactions Of The Birmingham Archaelogical Society*, Vol.66 (1950), pp 54, 100
3 Edgar Fripp, *Shakespeare's Haunts Near Stratford* (1929), p.123; *Inq.P.M. II, Vol.43, 50, Trin. Term. 17 Hen VIII, C42/43/50*, in the Public Record Office (afterwards, cited as the P.R.O.)
4 William Dugdale, *The Antiquities Of Warwickshire*, ii, p.670; *Letters & Papers (Foreign & Domestic), Vol.1, Part 1 (1509), 604/22; Del.Weston, 14 Oct., 2 Hen.VIII (1510); Esch. Ing. 17 Hen.8., E150/1130/1* P.R.O. Quoted in Halliwell-Phillips, *Observations On The Charlecote Traditions*, pp, 24, 47, 48
5 Quoted in Fripp, *Shakespeare's Haunts*, pp 123. 124
6 *V.C.H. Of Warwickshire*, Vol.3, p.103
7 *Lucy Fine, Hil. 36 Eliz.*, P.R.O., quoted in Halliwell-Phillips, *Observations On The Charlecote Traditions*, p.25. See also *Calendar Of Patent Rolls, 1548-1549*, p.255
8 Quoted in Fripp, *Shakespeare's Haunts*, p.125; *Patent Roll, 16 James I, ii, 13*, P.R.O.
9 The original is in Charlecote House, and there is a transcript in the Stratford Birthplace Records Office.
10 *E150/1151 m4, Warwick Inq. taken 23 Sept. 5. Edw.VI, 1551*, P.R.O.; *V.C.H. of Warwickshire*, Vol. 3, p.119
11 *V.C.H. Of Warwickshire*, Vol.3, p.103; *C66/914 m14, Patent Roll 3 & 4 Phillip & Mary, Part 9, 1557*, P.R.O.

12 *V.C.H. Of Warwickshire*, Vol.3, p.102
13 *Ibid, p.103*
14 *C 66/914 m14*, P.R.O.
15 *Wards 7/26/95 and C 142/263,7 (i)*, P.R.O.
16 *Calendar Of Patent Rolls, Edw.VI, Vol.2, 1548-1549*, p.255.
17 *V.C.H. Of Warwickshire*, Vol.3, p.102
18 "An Acte concerninge certyne assurances of Sir Thomas Lucie and others", *27. Eliz.I, No.39*
19 See James Fish, "The Survey Book Of The Manours Of Charlecot & Hunscot With Part Of The Manours Of Hampton-Lucy, Hatton, & Fullbrook In The County Of Warwick All Being The Lands Of The Worshipfull Tho. Lucy Esq" (1736), in the Warwickshire C.R.O., *LS/1036*
20 *V.C.H. Of Warwickshire*, Vol. 2, pp 291, 292
21 *Ibid*, Vol.3, p.91
22 *Letters And Papers (Foreign & Domestic), Hen. VIII, Vol.1, Part 1 (1509), 604 (Grants),22*, P.R.O.
23 *Patent Roll 1 Mary, Part 9 (1553-1554), E 66/872 m25*, P.R.O.
24 *Letters And Papers Foreign And Domestic, Hen.VIII, Vol.7*, p.292., P.R.O.
25 Lucy Toulmin,(Ed) *The Itinerary Of John Leland*, Vol.2 (1964), p.48
26 *V.C.H. Of Warwickshire*, Vol.3, p.93
27 *Harleian MS 607, f.128*, British Library
28 *V.C.H. Of Warwickshire*, Vol.3, p.93
29 *Calendar Of Patent Rolls, 1557-1558*, p.174
30 *Dictionary Of National Biography*, Vol.6, p.791
31 *Calendar Of Patent Rolls, 1569-1572*, p.194
32 *LR 1/135 (Vol.7),f.100 d (1573)*, P.R.O.
33 *SC6 Eliz./2308/2309/2310 (Warwickshire)*, P.R.O.
34 *Calendar Of State Papers, Dom.Ser.,Addenda 1566-1579*, p.509
35 *C66/1107*, P.R.O. See also *Calendar Of Patent Rolls, 1569-1572*, p.46
36 *John Kempson's Survey Of Northbrook Farm, 1813*, in the Warwickshire County Record Office; see also the Ordnance Survey map of Fulbrook parish.
37 See *SC6.Eliz.I.2308*, P.R.O. for a reference to this inquiry.
38 *V.C.H. Of Warwickshire*, Vol.3, p.92
39 *Calendar Of State Papers, Domestic, 1581-1590*, p.632
40 Edgar Fripp, *Shakespeare's Haunts Near Stratford* (1929), pp 116, 117
41 *Calendar Of Patent Rolls, 1575-1578*, p.449

42 *V.C.H. Of Warwickshire,*, Vol.3, p.250
43 In 1595, Adrian Quiney brought an action against Philip Green, Henry Rogers and John Shakespeare for a debt of £5. See Lewis, *op.cit.*, p.68
44 Fripp, *op.cit.*, pp 116, 117
45 Schoenbaum *op.cit.*, p.107
46 *35. Eliz.c.5*
47 *SP 38/8, Warrants 26 September 1607*, P.R.O.
48 *Calendar Of State Papers, Domestic 1603-10*, p.371
49 *Harleian MS 607, f.128*, British Libary
50 *SC6. Eliz.I.2308* and *LR 1/135 (Vol.7), f100d (1573)*, P.R.O.

CHAPTER 10: THE DEER PARK AND CONY WARREN AT CHARLECOTE

1 See *The Sheldon Tapestry Maps* (Warwick Museum Publications); E.A.B.Barnard & A.J.B.Wace, *The Sheldon Tapestry Weavers And Their Work* (1928)
2 John Humpherys, *Elizabethan Sheldon Tapestries* (1929), p.16
3 *L6/254*, Warwickshire C.R.O.
4 C.Holte Bracbridge, *Shakespeare No Deerstealer* (1862), pp 6, 7
5 Fripp, *Shakespeare Haunts op.cit.*, p.124
6 Croom, *op.cit.*, p.12
7 *Ibid*, p.20
8 *Ibid*, p.34
9 H.V.Thompson and A.N.Worden, *The Rabbit* (1956), p.187
10 *Calendar Of State Papers, Addenda 1566-1579*, p.289
11 *L6/191*, Warwickshire C.R.O.
12 *L6/193*, Warwickshire C.R.O.
13 *5.Eliz.I, c.21*
14 *32.Henry VIII, c.11*
15 Quoted in J.O.Halliwell-Phillips, *Observations On The Charlecote Tradition* (1887), pp 15, 16
16 *Ibid*, p.16
17 *Ibid*, p.12

CHAPTER 11: THE WILD YOUTH

1 *The Winter's Tale*, 1501-1507
2 *Cymbeline*, 2834-2842
3 *1, Henry VI*, 1996; *3, Henry VI*, 2363
4 *3, Henry VI*, 1398-1405
5 *Love's Labour's Lost*, 983-984
6 *As You Like It*, 638-650
7 *Much Ado About Nothing*, 619
8 *Coriolanus*, 2870-2871
9 *3, Henry VI*, 523-524
10 *The Merry Wives Of Windsor*, 422-425
11 *Love's Labour's Lost*, 1214-1226
12 *Julius Caesar*, 1426-1432
13 *As You Like It*, 663-665
14 *Titus Andronicus*, 1228-1230
15 *Ibid*, 642-655
16 *Venus And Adonis*
17 *Twelth Night*, 24-28
18 *Venus And Adonis*
19 *Hamlet*, 913-915
20 *Love's Labour's Lost*, 1565
21 *Ibid*, 1963-1964
22 *All's Well That Ends Well*, 2703-2705
23 *Measure For Measure*, 294-295 & 309-310
24 *Timon Of Athens*, 1883-1886
25 Halliday, *op.cit.*, p.443
26 Ralph Houlbroke, "The Making Of Marriage In Mid-Tudor England: Evidence From The Records Of Matrimonial Contract Legislation", *Journal Of Family History*, Vol.10 (1985), pp 339-351
27 J.M.Martin, *Marriage And Population Change In Tudor And Stuart Warwickshire* (1973. Manuscript thesis in the Warwickshire C.R.O.), p.36
28 *Ibid*, p.25
29 Skipp, *op.cit.*, p.14
30 *The Tempest*, 1666-1675
31 *Ibid*, 2269-2270
32 *Ibid*, 494-498

33 *Hamlet*, 3252-3255 & 3263-3266
34 *Love's Labour's Lost*, 2277-2278
35 *Twelth Night*, 2318-2322
36 *Much Ado About Nothing*, 1704-1708
37 *As You Like It*, 1504-1506
38 Chambers, *op.cit.*, p.253
39 *A Midsummer's Night's Dream*, 2186-2192
40 *Henry V*, 3168-3169
41 *The Taming Of The Shrew*, 2663-2665
42 *As You Like It*, 2055-2056
43 *All's Well That Ends Well*, 2041-2043
44 *The Comedy Of Errors*, 1516-1549
45 *Ibid*, 620 & 770-775
46 *Ibid*, 789-809
47 *Othello*, 3066-3076
48 *Love's Labour's Lost*, 1596 & 1603-1604
49 *Romeo And Juliet*, 1109-1110
50 *As You Like It*, 1839-1840
51 *A Midsummer's Night's Dream*, 584-590
52 *King Lear*, 2467-2471
53 See for example *Cymberline*, 5, v and *Twelth Night*, 5, i
54 *The Comedy Of Errors*, 364-381
55 *All's Well That Ends Well*, 1199-1200
56 *Ibid*, 944-45

CHAPTER 12: BANISHMENT AND EXILE

1 *King Lear*, 1913-1914
2 *Ibid*, 696-698
3 *Ibid*, 2, ii (Alexander Edition)
4 *All's Well That Ends Well*, 874-877
5 *Ibid*, 1164-1165
6 *The Winter's Tale*, 1757-1758
7 *Anthony And Cleopatra*, 2265, 2273-2278, 2309-2313
8 *Romeo And Juliet*, 304-305
9 *All's Well That Ends Well*, 2290-2292
10 *Love's Labour's Lost*, 2636-2637

11 *A Midsummer's Night's Dream*, 172-173
12 *Hamlet*, 1570-1571
13 *King Lear*, 2595-2606
14 *Romeo And Juliet*, 1815-1826
15 *King Lear*, 993-995 & 1018-1921
16 *Ibid*, 1809-1810 & 1698-1706
17 *Coriolanus*, 3317, 3373-3374, 3383-3385, 3394
18 *Ibid*, 2659-2692
19 *Ibid*, pp 285, 286
20 *Romeo And Juliet*, 2798-2802
21 *As You Like It*, 10-11
22 *The Taming Of The Shrew*, 616-618

CHAPTER 13: THE RISE OF WILLIAM SHAKESPEARE

1 Chambers, *William Shakespeare*, Vol.2, pp 252, 253
2 Quoted in S.Schoenbaum, *Shakespeare's Lives* (1970), pp 122, 123
3 Chambers, *op.cit.*, p.259
4 *Ibid*, p.254
5 *Ibid*, pp 264, 265
6 *The Merchant Of Venice*, 787-790
7 *Cymbeline*, 3176-3182
8 Fripp, *Master Richard Quyney*, p.177
9 Phelps-Brown & Hopkins, *op.cit.*, p.189
10 Quoted in Skipp, *op.cit.*, p.48
11 Thirsk, *op.cit.*, p.857
12 Fripp, *op.cit.*, p.103
13 Lewis, *op.cit.*, p.284
14 *Ibid*, p.285
15 *Ibid*, p.227
16 *Ibid*, pp 281-283
17 Quoted in Ann Hughes, *Politics, Society And Civil War In Warwickshire 1620-1660* (1987), p.83
18 Halliday, *op.cit.*, p.400
19 *Julius Caesar*, 638-643
20 *Coriolanus*, 80-85 & 17-26
21 See Annabel Patterson, *Shakespeare And The Popular Voice* (1989)

22 Brian Manning, *Village Revolts, Social Protest And Popular Disturbances In England, 1509-1640* (1988), p.204
23 *Timon Of Athens*, 1518-1531
24 *Troilus And Cressida*, 560-583

CHAPTER 14: THE END OF EXILE

1 See Lawrence Stone, "Interpersonal Violence In English Society 1300-1980", *Past And Present*, Vol.101 (1983), pp 22-33; Martin Daly and Margo Wilson, *Homicide* (1988), p.276; Razzell, *op.cit.*, pp vii-xxi
2 J.B.Given, *Society And Homicide In Thirteenth Century England* (1977). Alan Macfarlane has argued that individualism was not confined to the towns, but was a general characteristic of English society at this time; his thesis is however controversial and would have to be qualified by the generally accepted fact that the open field system involved a major degree of communalism. See Alan Macfarlane, *The Origins Of English Individualism* (1978)
3 Emile Durkheim, *Suicide: A Study In Sociology* (1952), pp 278-282
4 J.A.Sharpe, *Crime In Early Modern England: 1550-1750* (1984), pp 54-56
5 Manning, *op.cit.*, p.187
6 John Graunt, *Natural And Political Observations Made Upon The Bills Of Mortaility* (1662), p.74 In order to calculate these rates, I have used the population figures for London given in Finlay, *op.cit.* Finlay has concluded that the London bills of mortality were more reliable than hitherto thought. However, it should be noted that London's suicide rate was surprisingly low at this time; for example, the rate for Kent during the period 1561-1600 was 10.0 per 100,000, although this may have been higher than elsewhere because of its proximity to London. See Michael Zell, "Suicide In Pre-Industrial England", *Social History*, Vol.11 (1986), p.309.
7 See Razzell, *op.cit.*, p.xxxii; Zell, *op.cit.*
8 Graunt, *op.cit.*, p.22
9 Keith Wrightson, *English Society: 1580-1680* (1982), pp 190, 194
10 Razzell in Gough, *op.cit.*, pp xiv, xv
11 See Schoenbaum, *op.cit.*, pp 233, 234, 238

12 Chambers, *William Shakespeare*, Vol.2, p.252
13 *Ibid*, pp 250, 268, 286, 291
14 *The Tempest*, 2005-2008
15 *The Winter's Tale*, 2727-2732
16 *Ibid*, 3306-3317
17 Chambers, *op.cit.*, pp 268, 269
18 *Ibid*, p.250
19 *The Tempest*, 1209-1210
20 *Ibid*, 2292-2294
21 *Ibid*, 1445-1447
22 *As You Like It*, 618-623. I have modernized this passage appropriate to the context.

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THE PROBLEM OF DETERMINISM: A SOCIOLOGICAL SOLUTION

Contemplating the possibility of determinism, the social philosopher, Isaiah Berlin, wrote:

> . . . the changes in the whole of our language, our moral terminology, our attitudes toward one another, our views of history, of society, and of everything else will be too profound to be even adumbrated. The concepts of praise and blame, innocence and guilt and individual responsibility . . . are but a small element in the structure, which would collapse or disappear. If social and psychological determinism were established as an accepted truth, our world would be transformed more radically than was the teleological world of the classical and middle ages by the triumphs of mechanistic principles or those of natural selection. Our words – our modes of speech and thought – would be transformed in literally unimaginable ways; the notions of choice, of responsibility, of freedom, are so deeply embedded in our outlook that our new life, as creatures in a world genuinely lacking in these concepts, can, I should maintain, be conceived by us only with the greatest difficulty.[1]

Although written perhaps with a touch of hyperbole, this quote indicates the seriousness with which some philosophers have viewed the problem of determinism, a concern which has not abated in the last twenty years since the above passage was written. The number of publications on the issue has if anything increased, partly due to the growing success of the natural sciences, particularly in the fields of genetics and human biology. However, in spite of the proliferation of writing on the subject, one leading authority – J.O. Urmson – has concluded, that "no solution to these problems has been found which commands anything approaching general consent."[2]

The nub of the problem has been very succinctly summarized by J.R. Lucas in his book, *The Freedom of the Will.*

> We have a profound conviction of freedom. We know we are free. Yet when we think of ourselves from a scientific point of view, we do not see how we can be free. It would be a denial of science, we feel, to make man an exception to the universal laws of nature, and say that although everything else could be explained in terms of cause and effect, men were different, and were mysteriously exempt from the sway of natural laws.[3]

From the vast literature on the subject, and from everyday experience, it does seem that the majority of people do have a sense that both determinism and free-will are true, in spite of what appears to be a fundamental contradiction between them. The aim of this paper is to put forward a sociological resolution to this apparent contradiction. This will necessarily only touch on topics of great complexity, and will cover material from a number of disciplines, without being able to do full justice to any of them. The problem has of course had profound impact on the development of the social sciences, starting with the application of Kant's distinction between the "laws of freedom" and "laws of nature" in the nineteenth century. This led to the creation of the two separate disciplines

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[1] Isiah Berlin, *Four Essays on Liberty,* 1969, p. 113.

[2] J.O. Urmson and Jonathon Rie (eds.), *The Concise Encyclopaedia of Western Philosophy*, 1989, p. 113.

[3] J.R. Lucas, *The Freedom of the Will,* 1970, p. 1.

Geisteswissenschaften and *Naturwissenschaften,* phenemonological and positivistic sociology respectively. Additionally there have been a number of sociologists who have attempted to integrate these two perspectives, including Max Weber and Talcott Parsons. This proliferation of approaches has generated much controversy.

Determinism first became an issue in its modern form in the seventeenth century, although even then, Hobbes could write that the problem had already given rise to "vast and insoluble volumes".[4] Although it had been discussed in fragmentary form by some of the early Greek philosophers – particularly Epicurus – its first major presentation was in a religious context. A number of early Christian thinkers tried to reconcile the paradox of an omnipotent and omniscient God, who both predetermined the fate of the universe – including that of man – and created at the same time the capacity for free-will.[5] This led to numerous controversies in Christian theology, culminating in a polarisation of doctrine between the Calvinist belief in predestination, and the free-will Arminianism of the Quakers and Universal Baptists.

The success of the natural sciences in astronomy and other areas, led Descartes to adopt a mechanistic view of the material universe, which inevitably raised the question of the application of this mechanical principle to man himself. Descartes' solution to this problem was his well-known dualism, between mind and matter. Mind – or consciousness – was the basis of an "I" that was capable of acting freely, independently of the laws of nature. The body was seen by Descartes as a part of the material world, raising the issue of the relationship between mind and body – a problem he never successfully resolved. This dualism was rooted in Greek and Christian thinking, and Descartes' "mind" was the notion of the soul written in new language. The major difficulty faced by Descartes was how could the non-material substance of mind interact with and influence the material body? Descartes argued that the mind was equivalent to an internal pilot guiding the machinery of the body, operating in the pineal gland, the seat of the mind-body interaction. The unsatisfactory nature of this solution was clear even to Descartes himself, but he defined the problem in terms familiar to us today, largely because of his understanding of the principle of causality as applied to the natural sciences.[6]

As a part of this dualism, Descartes postulated a thinking "I", a self which was the origin and basis of all free action. He was influenced by Aristotle's notion of an "originating principle of action", capable of generating its own actions. This idea of an "originator" has been key in all the discussions on free-will and determinism; most defenders of free-will have argued for a human capacity for originating totally free action, and rooted this capacity in a "self", "mind", "person" or other form of individual identity. All these concepts arose historically out of the notion of an individual soul, which was central to both Greek philosophy and Christian theology. The soul was an essential and substantial spiritual self, created by God – and thus lying outside of the realm of nature, with its deterministic laws. In practice, there was a great deal of controversy about the nature of the soul, both in Greek and Christian thinking, a subject which we will return to later.

With the rise of science, it became necessary to substitute secular for religious language. The concepts of the mind and the self replaced that of the

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[4] Quoted in Ted Honderich, *The Consequences of Determinism,* 1990, p. 84.

[5] B.A.O. Williams, 'Freedom and the Will', in D.F. Pears (ed.), *Freedom of the Will,* 1963, pp. 5, 6.

[6] John Cottingham (ed.), *The Cambridge Companion to Descartes,* 1992.

soul, although they involved the use of the same basic assumptions: that the self/mind was a simple, unitary essential "I", capable of initiating free action. This change in language did not resolve the basic contradiction – the mind/body problem – and in fact raised new difficulties by postulating the self as an empirical reality subject to scientific scrutiny. It was Hume who first rigorously examined the concepts of the self and mind from an empirical point of view. From an analysis of mind, he concluded that "what we call a *mind*, is nothing but a heap or collection of different perceptions, united together by certain relations, and supposed though falsely, to be endowed with a perfect simplicity and identity."[7] Similarly, with the concept of self, he argued that "when I turn my reflection on *myself,* I never can perceive the *self* without some one or more perceptions, nor can I ever perceive anything but perceptions."[8] He criticized Descartes for his assumption that the mind was a substance of unitary identity, pointing out that "everything that exists, is particular: and therefore it must be our several particular perceptions that compose the mind."[9] A similar conclusion has been reached in our own day by Ryle who has argued that the conventional notion of the mind/self is nothing but the "ghost in the machine".[10]

Hume and subsequent thinkers saw that when the mind and self were analysed empirically they dissolved as unitary entities, and became sets of highly complex particular perceptions lacking any observable unity. Hume based his conclusions on subjective introspection, but an objective neurological and biological analysis involves equal difficulties for the concepts of a unitary mind and self. The same conclusion applies to existing sociological and social-psychological analyses of the mind and self; for example, in Mead's work, both mind and self arise out of a process of social interaction, and originate through a pattern of role taking and linguistic communication. The self is seen as being constituted as an "I", defined as the spontaneous, unique individual, and the "Me" which is a reflection of the "Generalized Other", the composite of all social expectations. When Mead's work is examined in detail, it turns out that the "Me" and "Generalized Other" are not unitary phenomena, but are concepts reflecting specific roles that individuals enter in to, giving multiple sets of self-definitions.[11] It is for this and other reasons that contemporary philosophers – even those sympathetic to arguments of indeterminism – have referred to the idea of a self, ego or mind as "dreadful and bizarre" and "extravagant". This scepticism about the self has reached a point where a current *Dictionary of Philosophy* has referred to it as "an obsolescent technical term."[12]

Hume was aware of the practical difficulties that ensued from this dissolution of the unitary self and mind. He had argued that causality could not be validated through inductive analysis: a perceived regularity could not guarantee the existence of a causal pattern outside acts of perception. His way of dealing with all these problems was his well-known resort to everyday life: "It is not . . . reason, which is the guide of life, but custom."[13] Elsewhere he appealed to nature as a practical guide: "Nature has . . . doubtless esteemed it an affair of too great importance, to be trusted to our uncertain reasonings and

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[7] David Hume, *A Treatise of Human Nature* [Book 1], 1962, p. 258.

[8] Ibid., p. 329.

[9] Ibid., p. 349.

[10] Gilbert Ryle, *The Concept of Mind*, 1949, pp. 15, 16.

[11] See George Herbert Mead, *Mind, Self and Society,* 1934.

[12] Anthony Flew (ed.), *A Dictionary of Philosophy,* 1979, p. 299.

[13] Hume, *op.cit.*, p. 343.

speculations."[14] Hume himself thus was able to accept the disturbing consequences of his own analysis with some equanimity, but his contemporaries were less happy with his conclusions. In particular, Kant concluded that Hume's work had undermined the philosophical basis of all knowledge, including the foundations of morality and individual freedom.

Kant's reaction to the problems raised by Hume was to resort to the two realms defined by Descartes, but to refashion this duality in a much more subtle and complex way. He postulated a phenomenal world of experience, not unlike Hume's, which was subject to the empirical laws of science and the principles of causality. All that could be observed and experienced was a part of this realm of nature, but in order for knowledge of this realm to be valid, Kant argued that it was necessary to postulate certain *a priori* categories of knowledge which could only be understood through the faculty of reason. Reason is the ultimate grounding and source of all continuity in human existence: "Reason is present in all the actions of men at all times and under all circumstances, and is always the same."[15] It was through reason that man could find a point of fixture, a principle invoked as a bastion against the flux of experience that Hume had discovered in his philosophy. Almost as important for Kant was the *a priori* category of freedom, that lay at the core of his moral ideas. All these categories were of a transcendental nature, and could not be derived from experience or empirical evidence. It was impossible according to Kant to know anything about the metaphysical content of these transcendental categories, as they could only be apprehended by rational understanding and not through empirical experience. The ultimate basis for all the categories was *practical necessity*: without them, it was impossible to establish a philosophical basis for either knowledge or moral freedom.[16]

Kant had succeeded in removing some of the more obvious difficulties in Descartes' dualism, but at the cost of transferring the ultimate realities – noumenal self, reason and freedom ("things-in-themselves") – to the empty realm of the transcendental. Although Kant's solution was radically different to Hume's, they both shared an appeal to practical necessity as a final resting point, although for Kant it was a formal part of his philosophy, whereas for Hume it was a form of almost perplexed resignation. Kant's postulate of the two realms of "nature" and "freedom" was associated with appropriate forms of causation – natural necessity and the causality of freedom. All empirical human acts were subject to the laws of nature, and according to Kant there were no exceptions to this rule. All acts could however be viewed from both standpoints, so that an act was both naturally caused, while at the same time originating from a free choice of the noumenal self.[17] The former was empirically observable, but the latter could only be abstractly postulated through transcendental reason.

Kant's solution to the problem of determinism – the creation of two realms – was unsatisfactory on a number of accounts. Firstly, it was a transcendental solution, and therefore had an obscure, remote quality. Secondly, and most importantly, the noumenal self which was the originating source of freedom, was a non-empirical postulate, and therefore subject to the same objection as Descartes original formulation. Kant had initially seen the self as "a

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[14] Ibid, p. 238.

[15] Immanuel Kant, *Critique of Pure Reason*, 1933, p. 478.

[16] Ibid, p. 343; Immanuel Kant, *Critique of Practical Reason*, 1956, pp. 5, 6.

[17] Kant, *Critique of Practical Reason*, pp. 464, 467.

spiritual, enduring, incorruptible being"[18] – the soul – but later in his philosophy was content to postulate it merely as a transcendental category. Kant defined the soul as having the following qualities: "l. The soul is *substance.* 2. As regards its quality it is *simple.* 3. As regards the different times in which it exists, it is numerically identical, that is, *unity* (not plurality). 4. It is relation to *possible* objects in space."[19] The fourth point was necessary to deal with the problem of the soul interacting with the empirical world of nature, but it was in effect self-contradictory: Kant had defined the noumenal soul as being outside space and time, so how was it possible for it to influence the material world of nature? Kant's retreat into the transcendental postulate does not in any way solve this problem, and the formulation has failed to satisfy most philosophers. However, I will be arguing later, using sociological arguments, that it is possible to restate Kant's thesis in a much more acceptable and valid form.

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Most philosophers writing on determinism have recognized that it is not a theory which can be proved true or false, but rather is a set of heuristic assumptions making possible the practice of science, at least in its classical form. It is impossible to falsify its premises, as any falsification of a particular hypothesis or theory, leads to further attempts to give causal explanations of the phenomenon in question. It is the source of the fruitfulness of science, that it never abandons its quest for explanation on the grounds of a particular failure. It is the basis of its aggressiveness, laying claim to all areas of experience, and given the hypothetical nature of scientific truth, it is unlikely to ever lose this dynamic quality, at least in the foreseeable future.

The reason why determinism has been taken so seriously is not because its major thesis has been proved to be true, but rather because of its successes in the natural sciences. In particular, the spectacular results in research in genetics and human biology in the last thirty or forty years, has given rise to the unease expressed by Berlin and quoted at the beginning of the paper. The explanations given by biology and genetics are in classical causal form, e.g. some of the recent work on genetic diseases such as muscular dystrophy, specifically defining muscular degeneration as an effect of a particular defective gene. In sociological terms, deterministic assumptions can be said to be a "functional pre-requisite" for the practice of classical science, a pre-requisite which is in the form of fundamental premises rather than testable hypotheses.

The major difficulty with this line of argument is the emergence of quantum mechanics in twentieth century physics. This is subject of much controversy and obscurity, so that Feynman, one of the leading contributors to the development of relativistic quantum field theory, could write, "nobody really understands quantum field theory."[20] Physicists have been unable to agree amongst themselves whether or not quantum mechanics is fundamentally indeterminist, as Bohr and Heisenberg, two of the authors of the Copenhagen Statement, argued, or whether as Einstein believed "God does not play dice with the universe". The dispute continues unabated, and a number of physicists have continued to search for "hidden variables" in order to give a complete

[18] Keith Ward, *The Development of Kant's View of Ethics,* 1972, p. 72.

[19] Kant, *Critique of Pure Reason*, 1933, pp. 330. 331.

[20] Quoted in Euan Squires, *The Mystery of the Quantum World,* 1986, p. 122.

deterministic account of quantum mechanics. It is clearly beyond the competence of an outsider to comment on what is such highly specialized and difficult work.

However, a number of scholars have pointed out that the problems of interpreting the behaviour of sub-atomic phenomena do not appear to apply to the macroscopic level of reality.[21] And this is ironically confirmed by Heisenberg: in describing the death of a physicist colleague, he stated that "I cannot doubt but that the beginning of his illness coincided with those unhappy days in which he lost hope in the speedy completion of our theory of elementary particles. I do not, of course, presume to judge which was the cause and which the effect."[22] So in practice, Heisenberg was forced to resort to deterministic language when talking about his own experience. As indicated by Hume, we assume the principle of determinism applies to our everyday lives, particularly in its physical aspect. And it is for this reason that the problem of determinism will not go away, in spite of the emergence of quantum mechanics in contemporary physics.

The success of biology and neurology as disciplines in recent decades has led to a great deal of discussion of the mind/ body problem, focussing on the brain and its relationship to consciousness. This has become a matter of some controversy, but it is universally agreed that there is a very close relationship between brain and mental activity. The most coherent and consistent explanation of this relationship is that known as identity theory. There are a number of variants, but I will confine myself to a discussion of the form which I believe can lay the foundations for a solution to the mind/ body problem. The starting point is Frege's doctrine that certain terms of language have both reference and sense. The most familiar example is the relationship between the Morning Star and the Evening Star; they are in fact the same star (having the same reference) but because they are perceived at different times (morning and evening), they have a different sense. In other words, the same phenomenon is described in different language because it was viewed from different perspectives, the identity of the two stars not being realized when the two separate names were coined.

Similarly, it is argued by identity theorists that brain processes and consciousness are identical, the one being viewed from the outside, the other from inside. Consciousness is the process of the brain – it is merely that which is experienced from the inside. The term coined by the analytical behaviourists – privileged access – is germane to this formulation; the person in question has a privileged access to the private experience of consciousness because it can only be experienced from the inside. From the outside, this experience will be described in neurological and biological terms, and so we have the language of the subject (inner consciousness) and that of the objective observer (neurology and biology) – both referring to the same, identical phenomena.[23]

This deceptively simple formula raises a host of problems, but I believe all these can be solved through careful analysis. Firstly, the most simple types of identity – for example pain – can clearly be seen to refer to the same phenomena. A toothache arising from caries caused through bacteriological infection and transmitting information to the brain (biology and neurology) is subjectively experienced as pain (consciousness). The first is an objective explanation in causal language, made by the outside observer; the second is a subjective account of consciousness made by the person undergoing the biological experience from

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[21] See for example Ted Honderich, *Mind and Brain,* 1990, p. 105.

[22] Werner Heisenberg, *Physics and Beyond,* 1971, p. 236.

[23] See Edgar Wilson, *The Mental as* Physical, 1979 and D.M. Armstrong, *A Materialist Theory of Mind*, 1968.

the inside – and of course, they refer to the identical phenomena. Similarly with hunger and sexual desire (subjective experiences) – they are identical to certain physiological and neurological states which can be defined objectively and scientifically. Acts of cognition likewise can be readily analysed in this way; for example, a person opening his eyes from sleep and seeing an object (a picture) – this can be described either as: 1. an act of consciousness or 2., a physiological movement of the eyes and the activation of certain brain processes. (Patterns of sleep, dreaming etc have been analysed through encephalograph measurements.) Both these descriptions refer to an identical event, merely using different language, depending on perspective.

These examples do not pose major problems for identity theory, but there is more difficulty with subjective phenomena such as intentions, purposes and facts of choice. Identity theory works well with obvious physical events, but becomes more difficult to accept with subtle and complex phenomena of a less obviously physical nature. There are two reasons for this: 1. The difficulty of locating the phenomena in question or, 2. The problem of giving any kind of coherent explanation of them. Although it is not possible to precisely locate a subjectively described phenomenon such as (say) an intention, it is clear that it must be located in principle in the brain, even it is not possible (at least not on current knowledge) to identify it with a specific neurological process. Empirically, we can address this point by asking, if not in the brain, where else would it be located? And we may add from a scientific point of view, if it is located in the brain, it must necessarily be a physical phenomenon.

The second point is more serious. One of the major criticisms of identity theory is that it does not do justice to "the indispensability of the mental".[24] It is unclear exactly what this phrase refers to – possibly the sheer subjective conviction of consciousness and mental experience. This itself is no objection to identity theory, but it does contain an implication which is valid. "The indispensability of the mental" implies a Cartesian insistence on consciousness as the basis of knowledge and individual identity, with the tacit assumption that it is the foundation of a self capable of moral choice. Most accounts of identity theory, are unable to give a coherent explanation of what we might call the moral dimension of experience, so that for example, one of the most persuasive recent expositions of the theory, virtually eliminates moral choices and intentions from its analysis.[25] We are thus returned to the central dilemma of this paper: how can a deterministic account of human behaviour – such as the identity theory – be reconciled with notions of free-will?

The answer is contained within identity theory itself. There are two ways of describing events: one in the language of the subject, the other in the language of the objective observer. This has most eloquently been summarized by J.R.Lucas:

> Free-will belongs to the agent's language, determinism's to the spectator's. I, as an agent, perform some actions freely: he, as a spectator, may predict events correctly. But I am not he; to be an active participator is not the same as to be an observer from the sidelines, and actions and events are logically very different; and therefore . . . no conflict can arise between my belief as an agent that I am acting freely and his certainty, as a spectator, that events will follow their pre-established course; since the

[24] Honderich, *Mind and Brain*, p. 105.
[25] Wilson, *op.cit.*

> key concepts of the opposition must be formulated in different languages, no contradiction between them can arise.[26]

Lucas was writing from the perspective of analytical philosophy, with its emphasis on "linguistic games", and the function of language regarding the activities of separate linguistic communities. Kant's distinction between the phenomenal and noumenal self is very similar, referring to the separate realms of natural necessity and freedom. None of these accounts give a satisfactory explanation of the existence of these separate modes of experience, but they all agree that they are based on *practical necessity.* For Hume it was the inevitability of nature and communal living; for Kant it was the necessity of practical reason; and for Wittgenstein and his followers, it was the functions of language for social life. Kant had summarized his philosophy when he wrote: "Two things fill the mind with ever new and increasing admiration and awe ... the starry heavens above me and the moral law within me."[27] This way of viewing the problem points us in the direction of a correct solution to the problem of determinism: the existence of two separate *social roles* – that of the *objective observer* and that of the *moral self.*

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There are innumerable and conflicting definitions of social role in the literature, but it can be defined as a set of normative expectations (obligations and rights) structured around a particular social position. In modern society, it is virtually impossible to escape the tensions which arise out of the above two role perspectives. This is not only because of the ubiquity of activities influenced by the natural sciences, but also because of the growth of bureaucratic and legal procedures which give rise to a rationalising perspective linked with the objective attitude. In law it is now common to appeal to deterministic criteria in mitigating the consequences of criminal behaviour; the law is of course the main area in which the notion of personal responsibility is activated, but appeals to mitigating medical and psychological handicaps have become increasingly common in the last few decades. The debate about capital punishment illustrates this theme: those who view it as a deterrent see it in term of objective consequences, whereas those demanding revenge and punishment are adopting the moral and subjective perspective. In legal situations, whether to define behaviour morally or medically is largely a question of choosing the language and assumptions of the two role attitudes. There is no intrinsic or technical criteria for making this choice, it must by the very different nature of the two perspectives, be a matter determined by other criteria: sympathy, social position, power and the ability to manipulate others to give favourable definitions.

The attitudes and behaviour in the two role situations will be fundamentally different: in one sense, we can say that the person fulfilling these two roles will feel him or herself to be a different person in the two situations. The two roles will elicit distinctive perceptions, emotions and physical responses, and if required to describe role behaviour, will generate different languages.

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[26] Lucas, *op.cit.*, p. 17.

[27] Emmanuael Kant, *Critique of Practical Reason,* 1783, Conclusion, [Translated by Lewis White Beck]

Of course, there are many considerations other than role behaviour in these situations, and in any one instance there will inevitably be a mixture of role responses. Social roles are clusters of ideal, normative expectations, which in practice are hardly ever enacted in pure form. There are innumerable other variables which determine any one type of behaviour, but for our purposes, it is sufficient to note that the distinction between objective observer and moral self is both logically valid and empirically fruitful. The role of the moral self is however more significant than that of objective observer, and is the most fundamental role in human society, with universal applicability. We are here dealing with matters of great complexity, and it will only be possible to touch on the most significant features of the moral self.

One complication in the analysis of the objective observer and the moral self roles is the prevalence of magical thinking in the earliest stages of human cultural evolution, which inhibited objective realism as well as complicated the analysis of the moral self. For example, James Morrill, who spent thirteen years living with the aborigines of Queensland in the middle part of the nineteenth century, described some of their beliefs as follows

> The moon *(werboonburra*), they say is a human being, like themselves, and comes down on the earth, and they sometimes meet it in some of their fishing excursions. They say one tribe throws it up and it gradually rises and then comes down again, when another tribe catches it to save it from hurting itself . . . They think the falling stars indicate the direction of danger, and that comets are the ghosts or spirits of some of their tribe, who have been killed at a distance from them, working their way back again . . . They think all the heavenly bodies are under their control; and that when there is an eclipse, some of their tribe hide it [the sun] with a sheet of bark to frighten the rest . . . But they are very uneasy during its continuance. They pick up a piece of grass and bite it, making a mumbling noise, keeping their eyes steadily fixed on it till it passes over, when they become easy again and can go to sleep comfortably. They think they have power over the rain *(durgun*) to make it come and go as they like.[28]

There is no doubt that magic was ubiquitous in tribal societies, although a number of anthropologists have pointed out that a belief in magic was limited by the existence of economic technology, which ensured a degree of objectivity. However, the existence of magic affected both the practice of objective realism and the attribution of personal responsibility. We are told of the Australian aborigines that "they do not suppose that any one dies from natural causes, but [always] from human agencies", with a number of examples given of individuals punished and killed on account of the alleged use of magic.[29] Additionally, magic was frequently used as a mode of punishment or retaliation. If as Levy-Bruhl and others have argued, the ubiquity of magic eclipsed the distinction between individual self and a universal, spiritual and mystical reality, personal responsibility would be impossible. In practice, all tribal peoples do make such distinctions, so that for example, as Evans-Pritchard tells us of the Azande, "if you tell a lie, or commit adultery or steal ... you cannot elude punishment by saying that you were bewitched."[30] Tribal peoples do universally ascribe spiritual qualities to the self, but it is the necessity of individual responsibility which limits

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[28] James Morrill, *Sketch of a Residence Among the Aboriginals of Northern Australia,* 1864, pp. 19, 20.

[29] B. Malinowski, *Magic, Science and Religion,* 1948.

[30] E.E. Evans-Pritchard, *Witchcraft, Oracles and Magic among the Azande*, 1937, p. 74.

the extent of magical belief, and, along with technology, is responsible for the beginnings of objective realism.

However, some anthropologists – in particular Levy-Bruhl – have argued that no distinction was made in tribal societies between the individual self and other subjectively defined realities, and an authority of the stature of Marcel Mauss, has concluded that a full sense of the individual self only arose in the modern period. This is a matter of some controversy, and Mauss, who was very familiar with the anthropological evidence, qualified this conclusion by writing that

> In no way do I maintain that there has ever been a tribe, a language, in which the term 'I', 'me' *(je, moi)* . . . has never existed, or that it has not expressed *something* clearly represented . . . it is plain, particularly to us, that there has never existed a human being who has not been aware, not only of his body, but also at the same time of his individuality, both spiritual and physical.[31]

Steven Lukes has pointed out, if we leave aside more arcane theoretical considerations, there is a parallel in "everyday conceptions of the person", in our own culture and those ranging from classical China through to tribal Africa.[32] The notion of an individual self is universal, and is as important and significant in tribal societies, as it is elsewhere. Reactions to death of a particular individual indicate that people in tribal societies display as much, if not more, grief than do modern Europeans. However, many tribal societies appear to confer less status on very young children and to some extent the very elderly, and therefore less importance is attached to loss of life in these categories than with other persons.

The pervasiveness and ubiquity of the concept of self requires special explanation. Our starting point must be the analysis of practical necessity, or to use a sociological term, functionality. Functionalism has been criticized because of the teleological nature of much of its argument, as well as its conservative ideological bias. It is however possible to restate to the tenets of classical functionalism so as to overcome these objections. The seeds of this restatement are to be found in a passage by one of the founders of modern functionalism, Wilbert E. Moore:

> The explicit introduction of system survival as a test of necessary consequences of human action and the structural mechanisms for producing those results perforce appealed to an evolutionary perspective. The argument must essentially be that various behaviours appear in human aggregates, some of which support or improve the viability of those aggregates and others that do not. Through natural selection those that contribute to system operation survive, and others are rejected. The same argument can be made for whole societies, whether in competition with other societies or simply coping with the challenges of the nonhuman environment. In the early explicit formulations of what came to be called "functional requisite analysis" this evolutionary assumption was not articulated.[33]

This formulation of functionalism places it squarely in the Darwinian tradition, removing its teleological aspect, and allowing for objective causal analysis. Socially structured behaviour is seen as analogous to a biological structure; its existence is explained through natural selection, so that only those behaviours which enable social

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[31] Marcel Mauss, 'A Category of the Human Mind: the Notion of the Person, the Notion of the Self", in Michael Carrithers et.al. (eds.), *The Category of the Person,* 1985, p. 3.

[32] Steven Lukes 'Conclusion', in Carrithers, *op.cit.*, p. 297.

[33] William E. Moore, 'Functionalism', in Tom Bottomore and Robert Nisbet (eds.), *A History of Sociological* Analysis, 1978, p. 342.

systems – and their individual members – to survive, will be selected. This process of selection is independent of human intention or meaning, although obviously human beings can rationally assess the probability of a particular mode of action ensuring their survival. The latter is associated with the role of the objective observer, which also ensures the survival of both individuals and societies. But much human social behaviour will not fall within this rational category, and this will include aspects of the role of the moral self. Given the non-rationality of much of the behaviour associated with this role, its universality must be explained in terms of its capacity to meet certain fundamental functional pre-requisites.

This approach can be linked with the revival of interest in cultural evolution, as well as the more recent development by Popper and others of evolutionary epistemology. Popper and Eccles have touched on the evolution of consciousness and the self as follows:

> What is usually described as the unity of the self, or the unity of conscious experience, is most likely a partial consequence of biological individuation – of the evolution of organisms with inbuilt instincts for the survival of the individual organism. It seems that consciousness, and even reason, have evolved very largely owing to their survival value for the individual organism . . . The activity of the self, or the consciousness of self leads us to the question of what it does; of what function it performs, and so to a biological approach to the self.[34]

Popper and Eccles are undoubtedly correct in emphasizing the biological basis of the self, and it is the physical separateness of individuals which forms the primary condition for an individual self. It is this biological fact which makes individuals crucial for all social structures and their functioning; the individual necessarily is the focus of all social action, and it is this fact which lays the foundation for the universality of the individual self. Popper has quite correctly pointed out the need to look at the functions of the self to fully understand the phenomenon, but his biological emphasis only provides an initial statement of the problem, and what is required to complete the analysis is a sociological perspective.

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The reference to the unity of the self must be our starting point. All the concepts that have been discussed in this regard – self, soul, ego, personal identity – are essentially the same phenomenon. It is only with such a category and social role, that continuity and consistency in thinking is possible, and this forms the basis of "a thinking, willing I . . . an essence that 'posits' its own acts, 'generates' and possesses psychic realities as its very own and is responsible for them . . . the abiding and supporting principle of all . . . conscious life."[35] The fundamental function of such a unified self is that it enables individuals to be held responsible for their actions, and thus forms the basis of all moral and social action. A self which can be held responsible for its actions constitutes the indispensible functional pre-requisite for all normative and social behaviour, and without meeting this pre-requisite, it would be impossible for any group or social system

[34] Karl Popper and John Eccles, *The Self and Its* Brain, 1977, pp. 108, 114.
[35] Walter Brugger and Kenneth Baker, *Philosophical Dictionary,* 1976, p. 381.

to survive. It is thus for this reason that the concept of a private self or soul is found in all societies, for without this concept and primary social role, no society could continue to exist. The moral self is a social role which creates the coherent and organized set of attitudes which constitutes individual identity, the ego and the self. The major obligation attached to the role is the personal responsibility which underpins all normatively regulated social life; the major right, is the capacity for personal freedom. In order to be held personally responsible, it is necessary to have the freedom to enact that responsibility.

The anthropologist, Paul Radin, has perhaps most clearly recognized the importance of personal responsibility and freedom in tribal societies:

> Now the concept of *person* in aboriginal society involves a number of definite things. This is not due to any mystical or philosophical interest on the natives' part, but flows from the purely practical consideration that they wish to know with whom they are dealing and the nature of the person's responsibility. In civilizations where a belief in reincarnation, ancestor-identification, transformation, multiple souls, etc., is involved in the concept of personality, the nature of an individual's responsibility for a given act is of paramount importance.[36]

This tacitly concludes that language used is secondary to the social reality; the assumption of individual responsibility exists even where it is not articulated explicitly.

According to Radin, although it is social groups who have formal legal responsibility in tribal society, it is individuals who in practice are held responsible, particularly for those most highly personal of activities, murder and marriage.[37] These are the most dramatic examples, but in fact, the concept of personal responsibility is ubiquitous, as without it, even minor forms of social life would be impossible. This can be illustrated through Colin Turnball's study of the Mbuti pygmies. Turnball describes an incident in camp late one evening:

Moke, very quietly, and talking as if only to the hunters but never lowering his arm or taking his eyes off Asuk, said, "That is a completely bad man. I have been watching and I have seen with my eyes, and my spirit (*roho*) makes me speak. He makes noise all the time, and he is the cause of all the noise in the camp. I would like to throw him out forever." [38]

Although responsibility is individual, the quality and context of it is different in tribal societies to what it is in modern European societies. Radin tells us

> That there is a "spiritual" side to a wrongdoer's state of mind is obvious but no feeling of sin, in the Hebrew-Christian meaning of the term, is present. All that is demanded is the realization that an individual has offended against the harmony of communal life. His punishment means the harmony has been re-established . . . Human beings can disport themselves as they will. If they are ridiculous, they will be laughed at; if they commit crimes, they will be punished and then, if they wish, they may commit some more.[39]

This should not be read to imply that there is a lack of internalisation of moral codes amongst tribal peoples; Radin specifically tells us while discussing a myth,

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[36] Paul Radin, *The World of Primitive Man*, 1953, p. 114.
[37] Ibid, p. 290.
[38] Colin Turnball, *The Forest People,* 1961.
[39] Radin, *op.cit.*, pp. 249, 257.

in which a man kills his wife and child during a period of famine, that "he judges and punishes himself. It must be so if society is to persist."[40] Individual responsibility is found in all societies, it is its quality and context which differs: tribal societies emphasize social harmony to a much greater degree than do contemporary European ones. Radin probably over-estimates the degree of individual responsibility in such societies; even in marriage and murder where he believes it to have been particularly strong, it was often the family or wider social unit which took responsibility, and certain categories of individual – for example women – lacked the power and personal independence necessary for the exercise of full responsibility. However, Radin is probably correct in his conclusion that all individuals, with full adult status, were held responsible for their actions in the last resort.

This transition from the status of childhood to that of adulthood is universal, and is linked to becoming a responsible subject:

> Full status was conferred on an individual at puberty and we all know the elaborateness of these rites and their ubiquity. A person was then truly functioning sociologically. He was responsible for his actions; he had to face life independently, and he could marry and raise children.[41]

To hold someone responsible for their actions implies that the person in question is capable of independent action. It has been generally recognized that this form of voluntary action must entail an absence of physical constraint, and also an assumption of personal causality. The term causality is not used here in the classical mechanical sense, but rather with the primary meaning given to it by Aristotle: an attribution of motivation to independent agents. Nevertheless, we can say historically, the assumption of personal causality laid the foundation for the eventual development of objective realism, with its complete separation of subject and object.

This separation was only fully achieved with the development of modern science, which was a part of that process of rationalization which eclipsed magical thinking, at least in the mainstream of European culture. This has led to a crystallisation of the modern self, with the virtual elimination of the projected subjectivity which was involved in animism and magic. But this in no way diminishes the underlying continuity of the moral self found throughout human history, based on the necessity of individual responsibility. Perhaps the greatest difference between the tribal and modern self is the extension of the category of personhood to very young children. In some tribal societies, young children are not considered full persons, and are sometimes killed during periods of great scarcity, through infanticide and other practices. This is consistent with our definition of a person in terms of responsibility, which in turn is linked to a capacity for practical action in economic and other spheres. The extension of personhood to young children is itself a sociological phenomenon, but that takes us away from our main concern, which is the analysis of the role of the moral self and its relationship to determinism.

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[40] Ibid, p. 330.
[41] Ibid, p. 80.

In 1962, Peter Strawson wrote, "Freedom and Resentment", a paper which initiated the modern debate about the problem of determinism. It is impossible to do justice to the complexity and subtlety of Strawson's argument with a brief summary, but an indication of its central theme is given in the following quotation:

> What I want to contrast is the attitude (or range of attitudes) of involvement or participation in a human relationship, on the one hand, and what might be called the objective attitude (or range of attitudes) to another human being, on the other. Even in the same situation, I must add, they are not altogether *exclusive* of each other; but they are profoundly opposed to *each other.* To adopt the objective attitude to another human being is to see him, perhaps, as an object of social policy; as a subject for what, in a wide range of sense, might be called treatment; as something certainly to be taken account, perhaps precautionary account, of; to be managed or handled or cured or trained . . . The objective attitude . . . may include repulsion or fear; it may include pity or even love. But it cannot include the range of reactive feelings and attitudes which belong to involvement or participation with others in inter-personal human relationships; it cannot include resentment, gratitude, forgiveness, anger, or the sort of love which two adults can sometimes be said to feel reciprocally, for each other.[42]

Strawson's contrast between the objective and participating attitudes is very similar to the distinction between the roles of objective observer and the moral self, except that Strawson emphasizes intentionality rather than personal responsibility, and he is not interested in a formal analysis of the two sets of attitudes. For Strawson, individuals can engage in emotionally reactive relationships because of their capacity to express intended and meaningful behaviour as free agents. To adopt the objective attitude towards a person is to remove their capacity to be fully human, to depersonalize them, and to reduce them to the status of objects. Strawson recognizes that adoption of this objective attitude can allow the suspension of normal moral responses which might have humane consequences depending on the situation, but his main interest is the indispensability of the reactive attitude for the continuation of human relationships.

This analysis of the objective attitude has led to what Honderich has termed dismay at the consequences of determinism.[43] Honderich has extended Strawson's analysis to include the "life hopes, personal feelings, knowledge, moral responsibility, actions and principles, and the general moral standing of agents."[44] It is beyond the scope of this paper to discuss these themes, but it sufficient to note that all these problems, like those outlined by Berlin earlier, stem from a belief that determinism undermines the possibility of free, independent action. Only the existence of a self acting as an ultimate "originator", without the interference of the mechanical effects of determinism, can guarantee the individual freedom which will not result in dismay. Anything else will reduce man to the status of a depersonalized object, incapable of genuine humanity. Honderich has attempted to solve this problem by postulating the possibility of self-affirmation, but this very solution requires the assumption of a self which is at the very centre of the problem itself.

The solution to the problem is contained in the recognition that the moral self is a social role that is totally distinct from that of the objective observer. Although both these social roles are subject to deterministic analysis – as are all forms of

[42] P.F. Strawson, 'Freedom and Resentment' in Gary Watson (ed.), *Free* Will, 1982, p. 66.
[43] Honderich, *op.cit.*
[44] Ibid, p.3.

empirical reality when viewed from the perspective of the objective observer – the roles themselves generate entirely different modes of experience.

It might be argued that from the point of view of the objective observer the postulate of a moral self is an illusion, because it assumes a freedom of action which conflicts with the assumptions of determinism. And it is the scrutiny of the role of the moral self from the viewpoint of the objective observer that has given rise to the problem of dismay, outlined by Honderich and others. But the problem only arises through role confusion: from the viewpoint of the moral self, freedom is not an illusion – it is an indispensible necessity of personal and social life. In our roles as moral selves, determinism is irrelevant, and as reality is shaped largely by our role experiences, it is with the acceptance of this reality that the problem of dismay disappears. This has some similarity with Hume's acceptance of the reality of everyday life, except the dimension of role analysis allows us to understand much more clearly and profoundly the nature of this solution, and in certain respects it is closer to Kant's postulate or two realms than Hume's voluntaristic position.

In practice, role confusion is not just a personal matter, but is also sociologically determined. The role of objective observer has become much more prominent in our society through the growth of science, technology and medicine, and this almost inevitably has led to role conflict. In contemporary psychiatry, the mainstream theoretical perspective is deterministic, both in the biological/ behavioural schools, and psychoanalytical/ psychodynamic ones. The language used is that of the objective observer, but inevitably the terminology of the moral self is introduced because of the nature of the disciplines. Strawson observed this when noting

> . . . the strain in the attitude of a psychoanalyst to his patient. His objectivity of attitude, his suspension of ordinary moral reactive attitudes, is profoundly modified by the fact that the aim of the enterprise is to make such suspension unnecessary or less necessary. Here we may and do naturally speak of restoring the agent's freedom.[45]

The aim of the psychoanalyst is to restore the capacity of the patient to become an independent person, to cease being a clinical object, but to become a full subject, capable of free and responsible action. This illustrates that most psychiatric disciplines use the concepts and assumptions of both role models in their work, but this is not inevitable. Behavioural therapy tends to deny the subjectivity of the patient, and sees its work in purely objective, physiological terms,[46] whereas existentialist therapy almost exclusively emphasizes the freedom of the subject. In this sense, existentialist therapy is a contradiction in terms, as in its pure form, it refuses to acknowledge terms such as mental illness, patient cure and the concept of therapy itself.[47] Definitions will of course vary depending on which role model is adopted, so that for example during the First World War, soldiers who refused to stay and fight in the trenches were either classified as malingerers and therefore punished, or defined as suffering from shell-shock and given medical treatment. The first treated the individual as a moral self, the second viewed him as a clinical object.

From the army's point of view – leaving aside ethical considerations – there is the practical question as to which role definition was most effective in getting soldiers to return back to the trenches. Likewise we can ask whether psychoanalysis

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[45] Strawson, *op.cit.,* p. 75.
[46] B.F. Skinner, *Beyond Freedom and Dignity,* 1971.
[47] Thomas Szasz, *The Myth of Mental* Illness, 1962.

or the existentialist attitude – or a combination of both – is more effective in bringing about personal independence. The psychoanalyst will classically take the former role and concentrate on the causally determined sequence of events which take place in childhood; the existentialist will adopt the position of the moral self, and emphasize freedom and personal responsibility. In practice, the effectiveness of the different role definitions will depend on a number of factors, including the expectations of patients and persons concerned.

It has become a commonplace to see bureaucracy as a source of the type of alienation that can be associated with the objective attitude. The dominance of bureaucracy and the devaluation ·of individual responsibility, may have been one of the factors in the collapse of Soviet Communism – all systems need to attribute personal responsibility to function effectively.

Kafka's description of the bureaucratic nightmare is reminiscent of Heidigger's notion of "unauthenticity" – a depersonalized and objectivised mode of being – a concept not all that different from Marx's alienation and Weber's "disenchantment of the world". The existentialists have given some of the most persuasive descriptions ·of personal alienation, and to quote Galen Strawson on Camus, "When *l'etranger* alludes to one of his desires, it is half as if he were recounting a fact about a feature of the world which is extraneous to him – a spectator to his own actions.[48] For existentialists the immediate resolution of this type of alienation is the restoration of the potency associated with a full acceptance of personal responsibility and the freedom of the moral self.

Sociological factors are of course crucial in both determining patterns of alienation and the conditions necessary for their resolution. A capacity for freedom is inextricably linked with the structure of power in any society which in turn is shaped by its economic and social conditions. For example, in order for women to be full and free subjects, they not only have to achieve equal status with men, but also have to acquire the freedom which comes with the abolition of economic scarcity and political oppression. The same would apply to slaves, lower castes and all oppressed groups.

Power is a critical dimension in the overcoming of this form of alienation, as power is intrinsically linked with the capacity for self-determination and the independence necessary for full personal responsibility and individual freedom. Ultimately the freedom of any one individual is linked with the freedom of all, but this is to raise a theme beyond the scope of the present paper. However it is appropriate to end with a positive conclusion: the distinction between the objective observer and the moral self resolves the problem of determinism, and in doing so, provides a clear intellectual foundation for the existence and practice of individual responsibility and freedom, along with the personal self-affirmation which flows from it.

[48] Galen Strawson, *Freedom and Belief*, 1986, p. 234.

The Growth of Population in Eighteenth-Century England: A Critical Reappraisal

PETER RAZZELL

Population growth in eighteenth-century England was due mainly to a fall in mortality, which was particularly marked during the first half of the century. The fall affected all socioeconomic groups and does not appear to have occurred for primarily economic reasons. In addition to an explanation involving the introduction of smallpox inoculation, the major hypothesis considered in this article is that the significant improvement in domestic hygiene associated with the rebuilding of housing in brick and tile brought about a major reduction in mortality.

The growth of the English population in the eighteenth century has long interested economic historians and, since the time of Thomas Malthus, has provoked much debate about the relationship between population change and economic growth. In our own time, scholars have focused on the nature and chronology of change: whether economic development preceded and prompted population growth or vice versa. The structure of demographic change has, however, yet to be resolved. Prior to the nineteenth century, English demographic data are incomplete: there were no national censuses before 1801, and civil registration of births, marriages, and deaths did not begin until 1837. Demographic research on the pre-nineteenth-century period has relied mainly on parish registers, which list baptisms, marriages, and burials. The accuracy and coverage of these materials is uncertain, and their survival is uneven.

Despite these difficulties, all demographers have discerned a rise in the rate of English population increase in the second half of the eighteenth century, and many have emphasized fertility as the key mechanism of population growth. These ideas have received added

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The author is Research Fellow at the Wellcome Institute for the History of Medicine at Oxford University.

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weight from the ambitious program of research undertaken since the 1960s by The Cambridge Group for the History of Population and Social Structure. The Cambridge Group's demographic findings were presented in *The Population History of England*, written by two of the group's leading members, Tony Wrigley and Roger Schofield.[1] The authors argued that English population grew in the latter half of the eighteenth century mainly because of a rise in fertility. This rise, they hypothesized, was due to a reduction in the age at marriage, itself a consequence of rising real incomes caused by economic development. This article questions the validity of their conclusion and develops an alternative chronology and explanation of the demographic transition in England.

NUPTIALITY AND MARITAL FERTILITY

The Cambridge Group has used two methodologies in its demographic work: "back projection" and "family reconstitution." I will evaluate each in turn and offer evidence suggesting that the reliability of both methods as applied to the English data is open to question.

Back Projection

Back projection was a technique used by Wrigley and Schofield to estimate earlier population levels by retrospectively adding the number of deaths and net emigrants to the various age groups enumerated in the nineteenth-century censuses, extending this process back into the sixteenth century. They used records of baptisms, marriages, and burials from a sample of 404 parish registers, which in theory allowed them to reconstruct the numbers of people living at all periods, as well as to compute marriage, birth, and death rates. The method entails a number of assumptions of unknown reliability, with scope for the compounding of errors and assumptions over long periods of time. Although the technique was developed using a very sophisticated computer program, the unknown reliability of the raw data and the uncertain assumptions used in the program led Schofield himself to compare it with looking "through a glass darkly."[2]

Ronald Lee, an active associate of The Cambridge Group, expressed his own reservations about the method in the following terms: "Back projection attempts an impossible task, and can only arbitrarily select one demographic past from among an infinite set of equally plausible and acceptable ones, which are consistent with the input data."[3] Recognition of the method's problems led other scholars to propose adjustments to the technique. Lee advocated its replacement with what

[1] Wrigley and Schofield, *Population History*.
[2] Schofield, "Through a Glass Darkly."
[3] Lee, "Inverse Projection and Back Projection," p. 190.

TABLE 1
BIRTH RATES PER 1,000 POPULATION IN ENGLAND AND WALES

	1749–1753	1814–1818
Birth rate before inflation	29.70	27.99
Penultimate estimates (after inflating for nonconformity and delayed baptism)	32.14	32.69
Final estimates after "residual" inflations	33.76	41.92

Source: Lindert, "English Living Standards," p. 136.

he termed "inverse projection"; he claimed to have validated Wrigley and Schofield's findings by applying this new method to their basic data. More recently, Wrigley and Schofield have themselves advocated a variant of a method pioneered by Jim Oeppen, "generalized inverse projection."[4] However, such methods require reliable data on births, deaths, migration, age structure, and mortality by age for the appropriate period—though they differ in their exact demands for reliability. Lacking accurate source material, the advocates of these methods have had to adjust their back-projected data in various ways.

For example, to correct for the underregistration of births, Wrigley and Schofield inflated the number of baptisms by various ratios derived from a comparison of expected births with actual records of baptisms. The estimates of expected births were calculated by taking the various census age groups and adding the estimated number of those born into the groups who died or migrated in the period before the census. A crucial factor in this computation is the magnitude of the various age groups, because it is the starting point for the process of estimating expected births. A poor estimate of the number of people in each age group would affect the inflation ratios used to correct the figures for baptisms, and hence would affect back-projected estimates of birth rates.

Peter Lindert has argued that the Wrigley-Schofield findings were distorted by the changes they made to census age figures. He concluded that "life tables and nineteenth-century censuses suggest[s] that birth registration was worse before 1780 than after. Yet Wrigley and Schofield turn the suggestion upside down, arbitrarily revising the censuses instead."[5] Lindert has calculated the inflations they made to the birth rate in a tabular form, reproduced here in Table 1.

Lindert's disquiet at the transformation of the pattern of fertility through the use of these inflation ratios seems justified. The inflations adopted by Wrigley and Schofield progressively increase the birth rate, though the critical inflation is for "residual" nonregistration. This residual inflation increases the birth rate for the period of 1814 to 1818

[4] Wrigley and Schofield, *Population History*, p. xvii.
[5] Lindert, "English Living Standards," p. 138.

TABLE 2
INDIVIDUALS LISTED IN THE 1851 CENSUS BUT NOT FOUND IN THE BAPTISM REGISTER VERSUS THE CAMBRIDGE GROUP'S INFLATION RATIOS

Period	Percentage Not Found in Register (Razzell) (1)	Period	Wrigley & Schofield's Inflation Ratios (%) (2)
1761–1770	32.4	1760–1769	8.4
1771–1780	27.9	1770–1779	9.3
1781–1790	32.6	1780–1789	13.1
1791–1800	36.0	1790–1799	20.9
1801–1810	32.0	1800–1809	28.8
1811–1820	33.0	1810–1819	38.0
1821–1830	30.0	1820–1829	34.1
1831–1834	27.4	1830–1839	26.0

Sources: Razzell, "The Evaluation of Baptism," p. 129; and Wrigley and Schofield, *Population History*, p. 561.

from 32.69 to 41.92 per 1,000, transforming the pattern of fertility in the period. Before this residual adjustment Wrigley and Schofield's original data suggested a constant birth rate during the latter half of the eighteenth century; after it, a very significant increase was apparent. That increase was due entirely to the inflation ratios derived from their assumptions about the age structure of the population applied to the original data.

My own research also throws doubt upon those inflation ratios. I have compared census statements directly with the expected baptism register entries for individuals living in 45 parishes selected from all parts of England. Table 2 displays the two sets of figures. The figures in column 1 are based on direct empirical evidence; those in column 2 are derived from theoretical reconstruction.[6] The two series are radically different in their trends over time; the census–baptism register data show little or no change over the period, whereas Wrigley and Schofield's figures show a sharp deterioration in registration accuracy from 1781 onward.

The critical ingredient in the inflation ratios Wrigley and Schofield used was their adjustment of age structure data derived from the nineteenth-century censuses. They themselves pointed out that one of their major assumptions was "that the age data for the older age groups became progressively less trustworthy with rising age, until above the

[6] For column 1, I calculated the percentages that Wrigley and Schofield used to inflate baptisms in order to produce the number of births (excluding nonregistration due to delayed baptism). The census-parish comparison method has attracted criticism on three grounds: (1) the 1851 census misstated the birthplaces of individuals enumerated; (2) many parents had their children baptized in neighboring parishes; and (3) the 1851 census misstated names and ages. From unpublished research linking census, parish register, and civil registration data, it has been established that the "false negatives" arising from these three factors amounted to about 10 percent for the whole sample of 45 parishes. The "false negatives" were counterbalanced by "false positives" due to using overstrict criteria for successful matches and to infants dying before baptism. See Razzell, "Further Evaluation."

age of 70 very substantial corrections to the published totals are necessary."[7] This is not a minor step in their calculations. It is not only central to the question of baptism registration adequacy, but it can be crucial for estimates of population size using back projection. Older age groups in the nineteenth-century censuses form the starting point of back projection, and any change in their numbers makes a critical difference to estimates of population size because of the compounding of errors with each "pass" through the computer program. For example, Wrigley and Schofield reduced the size of the group aged 90 to 94 in 1871 by 44 percent; if they had chosen instead to reduce that age group by 40 percent, their estimate of the English population in 1541 would have been about 9 percent greater.[8]

How reasonable are Wrigley and Schofield's assumptions? When we examine age statements by comparing the census with baptism register entries, a very different picture emerges from that assumed for the back-projection program. For the census–parish register sample of 45 parishes, 88.8 percent of all adult ages in the 1851 census was accurate to within two years, 97.8 percent to within five years. Contrary to Wrigley and Schofield's assumptions, there was no deterioration in the accuracy of age statements above the age of 70; the reliability of age statements in the 70-to-80 age group was the same as for the total sample. Only in the 80-to-90 age group was there any decrease in accuracy. But even there, 74.5 percent of the ages were accurate to within two years, and 90.2 percent to within five years.[9] This conclusion is confirmed by Wrigley himself from his detailed work on the 1851 Colyton Census: "The generally high standards of statements of age is clear. Only a tiny percentage of ages were out by more than two years. . . . Even at advanced ages this holds true in general. . . . Only one of the 26 [cases aged over 70] mis-stated his age by more than three years."[10]

On the substantive issue of the increase of the eighteenth-century population, the evidence suggests no increase in the birth rate during the latter half of the century. Wrigley and Schofield, however, supported their argument about the central role of a rise in fertility by quoting data from their research on family reconstitution, which purports to show that a rise in fertility associated with a reduction in the age at marriage (rather than a fall in mortality) was responsible for eighteenth-century population growth. Although they expressed a caveat about the reliance on a very small number of parishes in their reconstitution work—about 13 from a total of about 10,000 have formed the basis of the sample to date—these scholars used their family reconstitution findings to under-

[7] Wrigley and Schofield, *Population History*, p. xiv.
[8] Lee and Lam, "Age Distribution Adjustments," p. 282.
[9] Razzell, "The Evaluation of Baptism," pp. 126, 127.
[10] Wrigley, "Baptism Coverage," p. 304.

pin the conclusions they reached from back projection. Yet there are also grounds for disquiet about the accuracy of their use of the reconstitution method. This is a theme of such importance as to deserve careful examination.

Family Reconstitution

Family reconstitution involves the detailed study of individual families at the parish level. Individuals are traced in the baptism, marriage, and burial registers, and certain assumptions are made to establish family links among the individuals traced. From those links data are generated on a range of demographic variables, including age at marriage, fertility, and mortality rates. Family reconstitution is only applicable to individuals who remained in their parish of origin, as those who left disappeared from local records. For example, in the case of marriage, those who migrated after baptism invariably married elsewhere and would be excluded from the age-at-marriage calculations. Wrigley and Schofield worked on the assumption that those who remained in a parish were representative of the whole population, including migrants.

Ever since Peter Laslett's well-known 1960s study of Clayworth and Cogenhoe, social historians have increasingly come to recognize just how mobile the English population was. A general study of migration in early modern England by Peter Clark and David Souden found that up to 80 percent of the population was mobile—the percentage varying by place and over time, with increased mobility during periods of population growth.[11] As migrants are excluded from reconstitution studies, these very high levels of migration mean that reconstitution cohorts include only minorities of the population.

Evidence suggests that, because of the association between migration and social status, these minorities were atypical. Clark and Souden found that "more respectable members of local society tended to be less mobile than small craftsmen, servants and labourers"—though this may have varied over time.[12] Most evidence on geographical mobility and social status shows that they were very strongly correlated. From his work with The Cambridge Group, Souden noted "the high mobility of labourers in the reconstitution material" and commented on the "high mobility of labourers and many craftworkers and the relative immobility of farmers and food retailers." He concluded that "the marked lifetime immobility of farmers—of yeomen and husbandmen—contrasted with labourers . . . would show the degree to which landholding, or its prospect, would condition movement."[13] Those included in the reconstitution cohorts—the stayers—were much more likely to be farmers

[11] Clark and Souden, *Migration and Society*, pp. 32, 122–23, 222.
[12] Ibid., pp. 122–23.
[13] Souden, *Pre-Industrial English Local Migration Fields*, pp. 250, 254, 310.

and other property owners, whereas the migrants were invariably laborers, servants, and other propertyless groups. Laborers, servants, and other impoverished groups formed a significant proportion of the population at this time, perhaps up to half the total. Their relative exclusion would raise major questions about the validity of reconstitution methodology.

Migration also serves to distort reconstitution calculations in a more technical way that can most easily be illustrated with respect to calculations of the average age at marriage. Wrigley's study of Colyton indicated that the proportion of women born and married in the parish fell from 43 percent in the period from 1560 to 1646 to 25 percent in 1720 to 1769 before rising to 31 percent between 1770 and 1837.[14] Such a significant shift in the amount of migration would affect calculations of age at marriage, if migration was not evenly distributed among the various age groups. For example, if for some reason a larger proportion of women in their late twenties migrated out of a parish, this would have the apparent effect of lowering the age at marriage: women marrying at older ages would have left the sample before they could be included in the reconstitution age-at-marriage calculations, and only the younger ones would be recorded. Thus, even where there were no real changes in the age at marriage, variations in migration patterns could create the illusion of change because of the calculation method used in reconstitution work. Without a detailed knowledge of migration, it is impossible to say precisely what effect it would have on age-at-marriage calculations. Clearly, the effect could be significant.

Various sources provide evidence that the number of widow and widower remarriages as a proportion of the total number of marriages fell from approximately 30 percent at the beginning of the eighteenth century to about 10 percent at the end.[15] Whether this reduction occurred as a result of falling mortality or of changes in the propensity to remarry is an open question, but the fall itself could influence the accuracy of reconstitution by reducing the number of older men and women marrying in a parish. Most parish registers do not give information on the marital status of the marrying parties; for men, this could lead to a systematic overstatement of first-marriage ages in the earlier period by accidentally including marriage ages of widowers. Large numbers of women of unknown marital status listed in the marriage

[14] Schofield, "Age-Specific Mobility," p. 262.

[15] Wrigley and Schofield, *Population History*, pp. 258–59. The parish registers of Stoke Poges, Eton, and Farnham Royal in Buckinghamshire; of St. Margaret's Rochester in Kent; and of Barnstable in Yorkshire give information on previous marital status during the civil registration period of 1653 to 1658. Total marriages of widows ranged between 25.7 and 37.0 percent. The marriage licenses of East Kent and West Sussex show a fall in the proportion of widows, from over 30 percent in the first half of the seventeenth century to approximately 10 percent in the early nineteenth century.

registers could also distort reconstitution findings, because of the greater likelihood of confused identity.

The problem of identity confusion also arises when parish register information is inadequate. The linking of baptism and marriage dates in reconstitution work is essentially speculative, based on the assumption that a similar name within a certain time period confers a common identity. Yet there are grounds for believing that this assumption is unjustified. As we will see, it was a widespread practice in England to give the name of a dead child to a subsequent sibling of the same sex, and many parish registers were defective in registering the baptism and burial of those subsequent siblings. The registration of burials—and possibly of baptisms—improved in at least some of the reconstitution parishes during the eighteenth and early nineteenth centuries, which might have affected calculations of the changing mean age at marriage. The Cambridge Group used identical names in the baptism and marriage registers as the basis for calculating marriage ages. The nonregistration of subsequent same-name siblings would inflate marriage ages by incorrectly linking the first dead sibling with the sibling of the same name listed in the marriage register. This would have been more significant in the earlier period, of course, because of the less adequate registration of same-name individuals.

There are therefore four serious grounds for questioning the validity of reconstitution methodology as it has been applied to English marriage data: (1) the sociologically unrepresentative nature of reconstituted cohorts due to the exclusion of migrants; (2) the technical distortion effects of migration on the calculation of reconstitution statistics; (3) the unknown effect of changes in the proportion of widows and widowers in the marriage registers; and (4) the effect of changing patterns of same-name sibling registration on the calculation of marriage ages.

Given the uncertain reliability of back projection and family reconstitution as they have been applied to English historical data, it is necessary to carefully examine other forms of demographic evidence for the seventeenth- and eighteenth-century period to see what they reveal.

Age at Marriage During the Seventeenth and Eighteenth Centuries

The mean age at first marriage for women in The Cambridge Group's reconstitution sample was at its highest for the period from 1650 to 1699—26.2 years.[16] In historical terms this is a high figure, and its magnitude is largely responsible for the subsequent fall in the age at marriage found by the group. It is therefore important to evaluate that mean carefully, as it represents the key element in the pattern of marriage ages generated by reconstitution.

Two forms of marriage were legal in England in the seventeenth and

[16] Wrigley and Schofield, "English Population History," p. 164.

eighteenth centuries: marriage by license and marriage by banns. Although both types were included in parish registers, marriage licenses were recorded separately by the ecclesiastical authorities and often contain a great deal more information (such as age at marriage) not found in parish registers. Marriage by license was marginally more expensive than marriage by banns, and therefore was more socially exclusive. In particular, laborers tended to marry by banns, though all other occupational groups appear to have been well represented by licenses.[17] However, the flexibility of marriage by license—it allowed marriage in any parish without having to call banns on three successive Sundays—meant that this type of marriage became very popular in the seventeenth and eighteenth centuries. For example, over 50 percent of all marriages in the Diocese of Canterbury were by license between 1677 and 1725.[18] Indeed, in some parishes in the Diocese of London at that time the proportion rose to over 80 percent.[19] For demographers licenses have the advantage of giving information on migrants as well as nonmigrants and of covering large groups of parishes; they therefore help overcome the problem of concentrating on individual, and possibly atypical, parishes.

The accuracy of age statements in marriage licenses seems to have been high. Vivien Elliott evaluated marriage ages in a sample of 69 cases of London licenses at the beginning of the seventeenth century: the averages were 23.47 years in the licenses and 23.50 years by reconstitution—that is, by comparing baptism and marriage dates in the parish register. A similar exercise for 50 Leicestershire marriages at the end of the same century yielded averages of 24.8 and 23.8 years, respectively, indicating a difference of about one year.[20] This may be due to inaccuracies in marriage license age statements or to a confusion of identities in the parish register as a consequence of same-name registration problems.

In the late seventeenth century, high-quality information is available from licenses taken from over 1,000 parishes in five counties in different regions of England: Kent, London, Nottinghamshire, Suffolk, and Yorkshire. Table 3 shows that the mean age at marriage in the four counties other than London lies within a narrow band of 23.60 to 24.44 years. The overall average age at first marriage for the five counties is 23.56 years, significantly lower than the mean age found in The Cambridge Group's reconstitution sample for the same period: 26.2

[17] Steel, *General Sources*, p. 227.

[18] The number of license marriages is listed in Cowper, *Canterbury Marriage Licenses* for 1894 and 1898. The total number of marriages in Kent is given in *Enumeration Abstract, 1841 Census*. The proportion marrying by license was 50.74 percent for the period between 1677 and 1725.

[19] See, for example, the St. Michael Cornhill, St. Mary Aldermary, and St. Helen's Bishopsgate marriage registers for this period.

[20] Elliott, *Mobility and Marriage*, pp. 291, 325.

TABLE 3
AGE AT FIRST MARRIAGE OF WOMEN LISTED IN LICENSES, 1660–1714

Period	Region	*N*	Mean Age at Marriage	Reconstitution Mean Age at Marriage, 1650–1699
1662–1714	Yorkshire	7,242	23.76	—
1660–1702	London	500	21.93	—
1661–1700	Kent	1,000	24.06	26.2
1670–1709	Nottinghamshire	3,284	24.44	—
1690–1709	Suffolk	356	23.60	—

Sources: For Yorkshire: Drake, "An Elementary Exercise," p. 444. For London: Armytage, *Allegations for . . . London* (selecting the first 100 cases from the beginning of each decade). For Kent: Cowper, *Canterbury Marriage Licenses*, 1876, 1898 (selecting the first 500 cases from each volume). For Nottinghamshire: Blagg and Wadsworth, *Abstracts of Nottinghamshire Marriage Licenses* (selecting all cases listed). For Suffolk: Bannerman, *Allegations for . . . Sudbury* (selecting all cases listed).

years. In the 1840s, the earliest years of civil registration, women's mean age at first marriage was about 25.[21] The data in Table 3 suggest, therefore, no fall in the mean age at first marriage, but on the contrary a long-term rise of about 1.5 years.

THE HISTORY OF MORTALITY

Because the evidence considered in the previous section offers no support for a decline in age at marriage—nor for a rise in fertility—it is necessary to look elsewhere to explain English eighteenth-century population growth. In this section I will argue that the key demographic change was a decline in mortality that was particularly marked in the first half of the eighteenth century.

Population studies covering the centuries prior to reliable civil registration largely depend on data derived from parish registers. These registers invariably include information on baptisms (not births), marriages, and burials (not deaths). The reliability of the burial registers is obviously crucial to the study of mortality. For their calculation of reconstitution mortality rates, Wrigley and Schofield assumed a burial registration accuracy of 100 percent. Yet evidence suggests that in certain respects burial registration was significantly more defective in the seventeenth and eighteenth centuries than at a later period.

I have developed a method for measuring the adequacy of burial registration that may be termed the "same-name evaluation technique." It is based on child-naming customs prevalent in early modern England. It was extremely rare to give two living children identical Christian names; for example, of 2,221 children named in sixteenth-century Essex

[21] *Registrar-General's Fifty-Eighth Annual Report*, p. ix; and *Registrar-General's Twenty-First Annual Report*, p. iii.

wills, only 0.5 percent of living siblings shared the same name. An examination of seventeenth-century census returns from different parts of the country revealed no clear cases of living brothers and sisters with the same names.[22] On the other hand, it was widely customary to pass a dead child's name on to the next-born sibling of the same sex.

A look at two parishes used intensively in reconstitution work, Hartland and Colyton, enables us to estimate the frequency with which this same-naming custom was observed. In Hartland in the period from 1725 to 1743, a sample was chosen from the parish register of 50 dead children whose parents bore subsequent children of the same sex. Thirty of the subsequent children—60 percent—were given the same name as their predeceased sibling.[23] In Colyton, a similar examination of the data has proved possible over a much longer period by means of a reanalysis of the reconstitution schedules from 1538 to 1851.[24] In 789 of the parish families, a child was baptized after the death of another of the same sex. Of those families, 508—64.4 percent—gave the name of a previously baptized dead child to a subsequent child. The changes over time in the proportion of same-named children were as follows: from 1538 to 1600, 54.9 percent; from 1601 to 1650, 55.5 percent; from 1651 to 1700, 76.9 percent; from 1701 to 1750, 70.0 percent; from 1751 to 1800, 73.5 percent; from 1801 to 1837, 63.4 percent; and from 1837 to 1851, 62.2 percent. These are sufficiently large proportions of the total number of families to form the basis of an evaluation of burial registration during the whole 400-year period covered by the reconstitution schedules.

The importance of same-naming to the study of burial register accuracy can be illustrated as follows. During the middle part of the eighteenth century, Thomas Turner, a Sussex shopkeeper, kept a detailed diary and compiled notes on his family's history.[25] He listed his children's births and deaths as follows:

Peter (born August 19, 1754; died January 16, 1755)
Margaret (born March 20, 1766)
Peter (born June 1, 1768)
Philip (born October 9, 1769)

[22] See Emmison, *Essex Wills, 1558–1565*. The censuses searched were the 1599 Ealing census, the Claywoth censuses for 1676 and 1688, and the 1695 Marriage Duty Act censuses for London, Bristol, Lyme Regis, Swindon, and Wanborough. The London census was published in Glass, *London Inhabitants Within the Walls*. The Bristol census is in Ralph and Williams, *The Inhabitants of Bristol in 1696*. Copies of the other censuses can be found in the library of The Cambridge Group.

[23] See the *Hartland Parish Register*.

[24] A computer printout of the reconstitution schedules of Colyton was kindly provided by Ros Davies of The Cambridge Group. The grouping of families is specified in that printout. Families with interpolated baptisms were not included in the sample because doing so would introduce bias into the analysis.

[25] See Jennings, *Diary of a Georgian Shopkeeper*, pp. 79–84.

Frederick (born December 8, 1771; died November 7, 1774)
Michael (born April 29, 1773)
Frederick (born May 3, 1775; died June 13, 1775)
Frederick (born December 17, 1776)

Turner's first wife died after the birth of his eldest son Peter, and he subsequently remarried. The list of his children reveals the pattern of same-naming: the first Peter and the first two Fredericks died, and the next child of the same sex was given the dead child's name. Thomas Turner had lived all his married life in the parish of East Hoathly, and it is instructive to compare this list of births and deaths with the record of baptisms and burials of his children in the East Hoathly parish register:[26]

Peter (baptized August 31, 1754)
Margaret (baptized April 23, 1766)
Peter (baptized June 28, 1768)
Philip (baptized November 5, 1769)
Frederick (baptized December 30, 1771)
Michael (baptized May 19, 1773)
Frederick (baptized May 14, 1775; buried June 13, 1775)
Frederick (baptized January 10, 1777)

All of Turner's children were baptized and registered in the parish, but only one of the three dead children was recorded in the burial register: the second Frederick, who died in 1775. Turner's diary reveals that Peter and the first Frederick were in fact buried in the neighboring parish of Framfield, where their grandparents had died and been interred.

The Cambridge Group's reconstitution rules work on the assumption that all family events occur within the parish of residence. Given this, the demographic history of the Turner family, in which two children were buried outside the parish, would be misrepresented. The group's reconstitution rules would generate a calculated child mortality rate of 12.5 percent (one out of eight children), whereas in fact the true mortality rate was 37.5 percent (three out of eight children).

The practice of same-naming, however, allows us to assess the adequacy of parish registers in registering the deaths of children. For example, though we would not know from the East Hoathly burial register what had happened to Peter and the first Frederick, the repetition of their names in the baptism register would tell us that they had died, even though no record of their burial was available. We can thus assess the reliability of burial registration of a particular parish

[26] I am grateful to the East Sussex Record Office for conducting a search of the East Hoathly parish register.

TABLE 4
ANALYSIS OF BURIAL REGISTRATION OF SAME-NAME SIBLINGS IN COLYTON, 1538–1837

Period	*N*	Number Found in Burial Register	Percentage of Cases Unregistered
1538–1600	95	62	34.7
1601–1650	121	71	41.3
1651–1700	114	86	24.6
1701–1750	84	54	35.7
1751–1800	94	60	36.2
1801–1837	77	64	16.5
1837–1851	38	34	10.5
Total	623	431	30.8

Note: All calculations were based on Colyton reconstitution schedules supplied by Ros Davies of The Cambridge Group. The identity of same names is specified in the schedules, and in every case those names were selected for analysis.

register by measuring the proportion of same-name baptisms against registered same-name burials.

Application of this technique to a sample of cases selected from the Hartland parish register reveals that the accuracy of burial registration varied over time. Two hundred children baptized with the same name as a subsequent sibling were selected in alphabetical sequence from the register index for the period of 1558 to 1837.[27] Sixty-three of them (31.5 percent) were missing from the burial register. The first 100 cases, in the period from 1558 to 1724, had an omission rate of 39 percent, whereas the second hundred cases, from 1725 to 1837, had a rate of only 24 percent. These provisional results suggest a significant improvement in burial registration in Hartland during the eighteenth century.

A similar analysis of the 508 families in the Colyton reconstitution schedules who gave two or more of their children the same name yields the results shown in Table 4. The omission rate for the whole Colyton sample—30.8 percent—is similar to that found in Hartland, and registration accuracy there also seems to have varied over time. The Colyton registers reveal a sharp improvement at the beginning of the nineteenth century, which is consistent with what is known generally about the relative accuracy of Anglican burial registration at the time of the introduction of civil registration.[28]

I have made a special study of the Colyton Anglican burial register between 1837 and 1851, the period immediately following the introduc-

[27] The initial identification of names was provided by the Hartland parish register index. In the earlier period only the father's name was available for establishing a correct identity, but when two or more families had the same name, place-names were used as an additional criterion.

[28] Glass estimated that about 20 percent of all deaths were omitted from Anglican burial registration in the early period of civil registration, but this figure was lower in rural parishes like Colyton. See Glass, "Population and Population Movements," p. 234.

tion of civil registration. The civil registration records there list the deaths of 199 children under the age of ten during this period. Of that number, 170 were registered in the Anglican burial register, giving an omission rate of 14.6 percent—slightly higher than the 10.5 percent figure found using the same-name technique for the same period. However, the civil registers included young infants who died before baptism and were therefore often denied full burial status by the church. If we exclude infants who died in less than 24 days—the approximate mean age of baptism in Colyton at the time—the burial omission rate declines to 10.8 percent.[29] We must not make too much of the almost identical findings of the same-name technique and the civil-Anglican burial register comparison method, as the sample in the former study is small. Nevertheless, the similarity in the results of these two methods indicates a degree of reliability.

There were a number of reasons why Anglican burial registration was so deficient before the nineteenth century. The major factor was probably the negligence of clerks and clergymen in registering burials that had occurred in their parish.[30] Of all the same-name cases in Colyton between 1538 and 1851, 30.8 percent were missing from the burial register. We can evaluate the accuracy of this figure by comparing it with the proportion of people dying in Colyton who left wills behind but whose names did not appear in the burial register. Information is available on 124 people who either lived in Colyton or specified burial in the parish churchyard there and who made wills between 1554 and 1797; of this number 35 (28.1 percent) were not recorded in the burial register.[31] The similarity between this and the same-name figure suggests a general underregistration of burials, of both adults and children, during the period.

We have seen, in the case of the Turner family, another reason for unrecorded burials: the interment of children in neighboring parishes—a practice described by Schofield as a "traffic in corpses."[32] This probably accounts for some of the missing burials in a parish like Colyton. In its reconstitution schedules, information is sometimes given on the residence of a family, and there is a correlation between place of residence and registration reliability between 1538 and 1837, the period covered by the schedules. Of 65 same-name cases in which the father was listed as living in the town of Colyton, 48 were found in the burial register, an omission rate of 26.2 percent. When families lived outside

[29] This analysis is based on a list of Anglican burials and civil registration deaths that took place in Colyton between 1837 and 1851. The list was kindly provided by Richard Wall of the Cambridge Group.

[30] See Tate, *The Parish Chest*, p. 49.

[31] See Smith, *Wills Proved in P.C.C. Relating to . . . Colyton*; Fry, *Calendars of Wills*, Vols. 1 and 2; and the *Colyton Parish Register*. Information is usually given on the dates of the making and proving of wills, which allows a precise check against the burial register.

[32] Schofield, "Traffic in Corpses."

the town, in hamlets and outlying farms, the omission rate was as high as 43.9 percent, only 83 out of 148 same-name cases being found in the burial register. Some of these missing cases were probably buried in neighboring parish churchyards that were closer to the outlying areas than was the Colyton parish churchyard. Children baptized in Colyton but buried in surrounding parishes would not appear in the reconstitution statistics of infant and child mortality, and their omission would lead to an understatement of mortality.

Wrigley and Schofield's assumption of the absolute accuracy of the parish registers used in their reconstitution work was based partly on their having carefully selected high-quality parish registers, eliminating those with obvious defects. In the case of baptism registration, their assumption may be justified—particularly as missing baptisms can be interpolated from information on child burials, and registers can be selected on the basis of having the right pattern of birth intervals (that is, baptisms of children in a particular family occurring approximately every two years).

No such interpolation or selection is possible with burial registers, however, and the evidence derived from the same-name technique as applied to Hartland and Colyton suggests that death registration was unreliable throughout the sixteenth- to eighteenth-century period. The deficiency was probably greater than that shown in Table 4. The same-name technique can only be applied to cases in which baptisms were accurately registered, and it is likely that children whose baptism registration was defective also had more deficient burial registration. As we have seen, neither does the technique allow for children who died before baptism, and many of them would not have appeared in the burial register.

The Cambridge Group's estimates of infant and child mortality rates for Hartland and Colyton in the seventeenth and eighteenth centuries are low by historical standards: in the range of 83 to 106 per 1,000 between 1600 and 1749, falling to 57 to 97 per 1,000 between 1750 and 1799.[33] The results of the same-name technique indicate higher rates for all periods. If we allow for the various factors just discussed, which would further inflate registration unreliability, it is likely that infant mortality in Hartland and Colyton in the seventeenth and eighteenth centuries has been underestimated by between 35 and 50 percent.

According to the group's figures, the average infant mortality rate for the 13-parish reconstitution sample for 1600 to 1749 was in the range of 161 to 169 per 1,000.[34] If we inflate this rate as indicated earlier, it would increase infant mortality to between 250 and 340 per 1,000. National infant mortality was about 150 per 1,000 under early civil registration in

[33] Wrigley and Schofield, "English Population History," p. 179.
[34] Ibid., p. 177.

the late 1830s, so it seems probable that infant mortality probably dropped significantly during the eighteenth and early nineteenth centuries. However, it is too early to reach firm conclusions about the overall direction of this type of mortality; further research is needed on the registration reliability of other reconstitution parish registers.

The uncertain reliability of parish registers increases the value of other forms of evidence on mortality during the seventeenth and eighteenth centuries. Nearly all of these data concern adult mortality. In a 1974 article on parental loss, Peter Laslett commented on an apparent decline in the number of orphans in the seventeenth and eighteenth centuries. Community surveys of eleven localities taken between 1500 and 1706 revealed a median of 25 percent (with a mean of 22.5 percent), whereas eight surveyed between 1724 and 1811 had a mean of 16.5 percent (with a mean of 15.9 percent). Laslett concluded that the decline in the number of orphans probably "arose because of shifts in demographic rates, particularly in mortality."[35]

Of the communities Laslett studied, perhaps the most famous is Clayworth, in Nottinghamshire. The disappearance of large numbers of people from this community between 1676 and 1688 was used to illustrate the high level of mobility at that time. What Laslett did not sufficiently stress is that, in the case of adult heads of household, most of them disappeared through death rather than migration. Of 95 heads of household living in Clayworth in 1676, 44 were no longer there in 1688; 10 may have left through migration, but the remaining 34 died between the two censuses.[36] Allowing for the effects of migration, those 34 deaths represent a mortality rate of 3.05 percent per annum, over twice the 1.39 percent adult mortality rate found in England under civil registration 150 years later.[37]

In his discussion of orphans, Laslett quoted the civil marriage returns for the Manchester area in the 1650s, which recorded the father's name, parish of residence, and father's mortality status. Using these data, it is possible to calculate the mortality rate of fathers. Of 380 spinsters married in the Manchester area between 1654 and 1660, the fathers of 226 were dead by the time of their marriage. That is, the fathers of 59.47 percent of these women were dead.[38] Assuming an average age at first marriage for women of about 23, this represents an annual mortality rate of fathers of 2.59 percent per annum, well above the figure found in early civil registration. The fathers of these women marrying in Manchester came from all parts of Lancashire as well as from other northern counties. There appears to have been little variation in mortality

[35] Laslett, "Parental Deprivation," p. 15.

[36] Laslett and Harrison, "Clayworth and Cogenhoe," p. 183.

[37] *Registrar-General's Ninth Annual Report, Appendix.*

[38] These figures were calculated from all marriages listed in the marriage register between 1654 and 1660. See the *Manchester Cathedral Parish Register.*

between different areas within Lancashire. Of the 49 fathers who came from Manchester itself, 61.22 percent were dead at the time of their daughter's marriage, a proportion close to that for the whole sample covering all areas. (Evidence from tontines, marriage licenses, and other material suggests that the urban-rural gradient postdates the seventeenth century.)

These data suggest a radical long-term decline in mortality between the seventeenth and nineteenth centuries. It also fits traditional ideas of a high mortality rate in the preindustrial era. However, it is at variance with The Cambridge Group's family reconstitution work on adult mortality, which found only a very modest rise of about three years in life expectancy for men at age 30 during the 250 years between 1550 and 1799.[39] Most of the problems associated with the reconstitution of marriage ages—unreliable parish registers, sociologically unrepresentative samples, and the technically distorting effects of migration—also apply to the study of adult mortality. With the adult mortality cohorts there is the additional problem of very small sample sizes. For example, approximately 21.5 percent of all females born in Colyton between 1560 and 1646 were included in the adult mortality cohort, with equivalent figures for 1720 to 1769 and 1770 to 1837 of 12.5 and 15.5 percent.[40] In other words, in some instances The Cambridge Group's mortality cohort was derived from only an eighth of the total population. Reliable conclusions about mortality cannot safely be based on such small samples.

There is, however, another source of information that allows a provisional assessment of adult mortality over the 300-year period between the sixteenth and eighteenth centuries: marriage licenses. The licenses issued in the Diocese of Canterbury are of particularly good quality and run continuously (except for the interregnum period of 1646 to 1660) from 1568 through to 1809 and beyond. The diocese covers the East Kent region and includes 289 parishes. Seventeenth-century marriage licenses record information on the parents of bachelors and spinsters at all ages, but particularly on those of young women. By canon law, the consent of parents or guardians was required before a marriage license could be granted; those marrying under 21 had to provide it in writing or in the form of a sworn affidavit.[41]

The allegations attached to the licenses issued from 1619 to 1646 and from 1661 to 1676 nearly always refer to parental consent, particularly for the former period: over 96 percent of licenses gave information on

[39] Wrigley and Schofield, *Population History*, p. 250.

[40] Insufficient evidence has been published to calculate exact figures, but for Colyton approximately half of the initial cohort of married women was included in the mortality sample: applying that ratio to the proportion of females included in the marriage sample yields the figure quoted in the text. See Wrigley, "Mortality in Pre-Industrial England."

[41] Steel, *General Sources*, pp. 226–68.

TABLE 5
PARENTAL MORTALITY BY AGE OF DAUGHTER IN EAST KENT, 1619–1646

Age of Daughter	Number in Sample	Father Alive, Consenting (%)	Father Dead, Mother Consenting (%)	Both Parents Dead (%)
16–20	280	58.2	23.2	18.6
21–25	484	42.1	23.1	34.7
26–30+	236	26.7	25.0	48.3
Total	1,000	43.0	23.6	33.4

Sources: Cowper, *Canterbury Marriage Licenses*, 1892, 1894, 1896, 1905, 1906; and Willis, *Canterbury Marriage Licenses*, 1967, 1969, 1971.

parental consent between 1619 and 1646. The richness of this information allows us to examine whether fathers or parents were alive or dead for virtually all those marrying by license in that period: 42.36 percent of the total population. The licenses give information on age and occupation, which allows a study of both of those variables.

Table 5 summarizes an analysis of parental mortality by age for a sample of 1,000 individuals.[42] It reveals a high level of parental mortality: one-third of these women had lost both parents by the time of their marriage, a proportion that increased to 48.3 percent for those 26 and older. So nearly one-half of women had lost both parents by their late twenties. In seventeenth-century Kent, only a minority of women—43 percent—had two living parents at the time of their marriage. These figures speak for themselves: adult mortality was very high in this period.

We can calculate the adult mortality rate of fathers by dividing the numbers dead by the average age of their daughters. Fully 57 percent of all fathers were dead at the time of their daughter's marriage, and they had died during a 23-year period (the average age at marriage of their daughters). This yields an annual mortality rate of 2.48 percent per annum, almost identical to that found from the Manchester marriage register for the period of 1654 to 1660. These fathers probably died over a fairly even period between the birth and marriage of their daughters: a small sample of 35 cases in which the date of death was given indicates that on the average fathers died 10.64 years before their daughter's marriage.

The long-term change in mortality can be measured by comparing

[42] In preparing Table 5, I adopted the following rules: (1) if a father was listed as giving his consent, he was assumed to be alive; (2) if a father was not mentioned, and a mother was stated as giving her consent, the father was assumed to be dead and the mother to be alive; and (3) if a guardian was listed as giving consent, both parents were assumed to be dead. In the majority of cases, particularly during the earlier periods, information is given directly on the mortality status of parents—for example, a mother giving consent is recorded as a widow of a lately deceased husband, or both parents are recorded as being dead. However, these rules should be checked through further research.

TABLE 6
MORTALITY AMONG PARENTS OF SPINSTERS UNDER AGE OF 21 MARRYING BY LICENSE IN EAST KENT

Period	Father Alive, Consenting (%)	Father Dead, Mother Consenting (%)	Both Parents Dead (%)	Total Number in Cohort
1619–1646	53.33	27.06	19.61	1,275
1661–1676	55.70	25.23	19.07	515
1677–1700	58.86	19.82	21.32	333
1751–1779	74.29	21.00	4.29	700
1780–1809	76.89	17.68	5.43	1,233

Sources: Cowper, *Canterbury Marriage Licenses*, 1892, 1894, 1896, 1898, 1905, 1906; and Willis, *Canterbury Marriage Licenses*, 1967, 1969, 1971.

these figures with those compiled under civil registration 200 years later. Among men living in Kent of roughly the equivalent age group (between 30 and 55), mortality was virtually halved between the early seventeenth and early nineteenth centuries: from 2.48 percent in 1619 to 1646 down to 1.31 in 1838 to 1844.[43]

The chronology of change in the pattern of mortality among the marriage license population can be traced through an analysis of the marriages of all women marrying under the age of 21. This information is available in the Diocese of Canterbury for all periods except 1701 to 1750. Table 6 depicts the exact chronology of decline in mortality. This table suggests a marked reduction in adult mortality from the mid-seventeenth to the mid-eighteenth century. The proportion of cases in which both parents were dead dropped particularly sharply: from 21.32 percent in 1677 to 1700 to 4.29 percent in 1751 to 1779. This was matched by the fall in the percentage of fathers dead—from 46.67 to 25.71 percent—representing a fall in mortality, all else being equal, of 44.91 percent. The reduction in mortality seems to have commenced after the 1660s, though the changes in the late seventeenth century appear to have been relatively slight. The main fall in mortality seems to have occurred between the end of the seventeenth and the middle of the eighteenth century.

For the earlier periods, information is invariably given in the Kent licenses on the occupation of both husbands and living fathers, though not usually for fathers already dead. This allows an occupational analysis of mortality, and Table 7 illustrates what is possible in this respect. Overall there is little correlation between the husband's occupation and parental mortality—except in the earlier period, which shows a lower rate for gentlemen and higher one for husbandmen, with a slightly higher mortality for gentlemen in the later period.

Although laborers and the unemployed are not covered by Table 7, groups such as husbandmen and fishermen were characterized by a

[43] See *Registrar-General's Ninth Annual Report, Appendix*, pp. 17–20.

TABLE 7
MORTALITY AMONG PARENTS OF SPINSTERS MARRYING UNDER 21 BY OCCUPATION OF HUSBAND IN EAST KENT, 1619–1809

Occupation, by Period	Father Alive, Consenting (%)	Father Dead, Mother Consenting (%)	Both Parents Dead (%)	Number in Cohort
Gentlemen and professionals				
1619–1646	60.49	16.10	23.41	205
1661–1700	61.83	19.85	18.31	131
1751–1809	72.33	20.12	7.55	159
Total	64.65	18.38	16.97	495
Yeomen and farmers				
1619–1646	58.76	25.18	16.06	274
1661–1700	57.99	15.98	26.03	169
1751–1809	84.54	12.08	3.08	207
Total	66.77	18.62	14.62	650
Husbandmen				
1619–1646	49.77	29.58	20.66	213
1661–1700	60.66	22.95	16.30	122
1751–1809	80.56	16.67	2.78	108
Total	60.27	24.60	15.12	443
Artisans and tradesmen				
1619–1646	54.18	28.48	17.92	491
1661–1700	50.61	29.45	19.94	326
1751–1809	74.31	20.40	5.29	397
Total	59.80	25.86	14.33	1,214
Mariners and fishermen				
1619–1646	58.33	25.69	15.97	144
1661–1700	55.34	29.13	15.53	103
1751–1809	75.95	22.15	1.90	158
Total	64.44	25.19	10.37	405

Sources: See sources for Table 6.

similar level of income and were certainly very much poorer than gentlemen and yeomen farmers.[44] The higher mortality among husbandmen indicates that economic forces may have been a factor in shaping mortality patterns in the earlier period. However, the fact that there were very substantial increases in life expectancy among all occupational groups during the eighteenth century suggests that economic factors were not primarily responsible for the reduction in mortality. For the later period we have information on a number of laboring families: of 91 women under the age of 21 marrying laborers in East

[44] Gregory King estimated that the average income of "common seamen" was £20 per annum, not significantly greater than the estimated income of "labouring people and out servants" (£15 per annum). See King, *Natural and Political Observations*, pp. 48, 49.

TABLE 8
AVERAGE NUMBER OF YEARS LIVED, MEMBERS OF PARLIAMENT, 1660–1820, BY AGE AT FIRST ENTRY

Date at First Entry	Average Number of Years Lived After Entry		
	Aged Under 29	30–39	40+
1660–1690	25.71 (429)	22.58 (458)	17.87 (633)
1715–1754	30.83 (541)	28.17 (422)	18.52 (347)
1755–1789	37.13 (480)	29.86 (354)	21.16 (431)
1790–1820	38.06 (571)	32.04 (432)	22.40 (572)

Notes: Calculations are to the nearest year and include only cases with full information on date of birth, first entry, and death. Figures in parentheses indicate numbers of cases.
Sources: Henning, *House of Commons 1660–1690*; Sedgwick, *House of Commons 1715–1754*; Namier and Brooke, *House of Commons 1754–1790*; and Thorne, *House of Commons 1790–1820*.

Kent from 1751 to 1809, 83.52 percent had fathers living at the time of their marriage—a figure second only to that for yeomen in the proportion of fathers still living. This finding is consistent with those on occupational mortality in the nineteenth century: laborers in agricultural counties in the post-1860 period had one of the lowest mortality rates recorded.[45]

Although no other reliable evidence covering the general population exists, a variety of information is available on special groups, which allows a supplemental assessment of changing mortality. One of the most accurate forms of data available is on Members of Parliament. Biographical information on M.P.s exists for the period from 1660 to 1820, except for 1691 to 1714. Date of birth, entry, and death to the nearest year is known for 94.58 percent of the 5,995 M.P.s who first entered Parliament in 1660 to 1690 and 1715 to 1820—an unrivaled level of demographic accuracy for the period.[46] A special study of these data is in process, but the preliminary findings are presented in Table 8.

There were sharp gains in life expectancy between 1660 to 1690 and 1715 to 1754, particularly for the younger age groups (under the age of 39). Mortality continued to fall from the period 1715/54 to 1755/89, though only among M.P.s under the age of 29.

The finding of a significant fall in mortality during the first half of the eighteenth century is supported by a number of studies. Perhaps the most important (and most neglected) is a study of government annuitants made by John Finlaison, the actuary to the National Debt Office, which was published in 1829. Finlaison's data derived from four

[45] See Haines, "Conditions of Work and Mortality Decline," p. 183. According to the East Kent license data, all rural occupational groups—yeomen, husbandmen, and laborers—had a lower parental mortality than the more urban ones in the late eighteenth century.

[46] See Henning, *House of Commons 1660–1690*; Sedgwick, *House of Commons 1715–1754*; Namier and Brooke, *House of Commons 1754–1790*; and Thorne, *House of Commons 1790–1820*. The proportion of total cases with information on birth, entry, and death by period are as follows: for 1660–1690, 95.72 percent; for 1715–1754, 89.42 percent; for 1755–1789, 95.76 percent; and for 1790–1820, 98.19 percent.

TABLE 9
MORTALITY RATES PER 1,000 OF ALL NOMINEES TO BRITISH TONTINES, 1693–1789

Age Group	Date of Tontine			
	1693	1745+	1773	1789
5–15	9.12	5.65	5.75	6.75
16–30	18.44	9.27	10.32	10.14
31–45	20.21	12.61	11.88	11.05
46–60	31.57	22.93	17.09	18.57
61–75	66.09	66.81	51.89	77.39

Source: Finlaison, *Report on Life Annuities*, pp. 66, 67.

tontines run by the British government in the eighteenth century. A tontine was a device to raise revenue; it involved the payment of annuities to subscribers based on the survival of their nominees. Subscribers buying tontine shares were allowed to nominate whomever they wished. Most of them nominated themselves or, more frequently, their children. The annuity paid out by the government depended on the survival of individual nominees—survivors shared a fixed annuity sum among themselves—and their deaths were monitored by the Exchequer until the last nominee died, in very old age. For example, the last survivor of the 1693 tontine died in 1783.

Although a self-selected group, the subscribers came from all parts of the country, and there is evidence that they were demographically representative of the social groups from which they originated.[47] The subscribers to the tontines were a mixture of aristocracy, gentry, merchants, and professional people, and though this was a limited social range, the precision and accuracy of the data helps counterbalance that limitation.[48] The smallest number of nominees was for the 1693 tontine (just over 1,000), but the numbers grew progressively throughout the eighteenth century. Table 9 summarizes the mortality experience of the four tontines.

There were marked falls in mortality among all age groups under the age of 60, most of which occurred between the first two tontines. For example, mortality among the 16-to-30 age group almost exactly halved between the 1693 and 1745 tontines. A majority of the nominees entered the tontines as children, though the survivors went on to be included in mortality calculations for the later age groups. The pattern of mortality revealed by the tontine data indicates that most of the reduction in mortality occurred in the first half of the eighteenth century.

[47] In the 1789 tontine, the government nominated over half of the nominees by lot, and their mortality rates were similar to that of the nominees of the subscribers. See Finlaison, *Report on Life Annuities*, pp. 7, 66, 67.

[48] In 1693 the proportion of subscribers listed as gentlemen (including aristocrats) was 59.1 percent; professionals, 11.2 percent; and merchants and others, 29.7 percent. The equivalent proportions in 1745 were 56.8, 10.5, and 32.7 percent, respectively. See *The British State Tontine of 1693*; and Leeson, *Guide to . . . British State Tontines*, p. 7.

TABLE 10
LIFE EXPECTANCY (IN YEARS) OF MALES AGED 25 YEARS

Social Group	Approximate Period				
	1600–1649	1650–1699	1700–1749	1750–1800	1800–1824
Tontine nominees	—	28.0	34.5	36.4	—
Aristocracy	25.4	26.9	31.8	36.4	37.2
Reconstitution sample	32.9	31.4	33.6	35.4	—
South-of-England Quakers	26.1	27.6	31.7	31.5	—
Scottish advocates	28.8	31.1	38.0	38.1	—
Fathers listed in marriage licenses	26.9	28.6	—	37.9	—
Members of Parliament	—	25.7	30.8	37.1	38.0

Notes: These figures were prepared with the help of Jim Oeppen. In the case of the marriage licenses, it was assumed that (1) the average newborn had a mother aged 32 and a father aged 35; and (2) the average child was 20 years old at marriage. Model North in Coale and Demeny was used for translating survivorship between the ages of 32 and 52 for women (35 and 55 for men) into life expectancy at age 25. For the reconstitution sample and the Quakers, conversion was made to life expectancy at age 25 by using the relationship between life expectancy at ages 25 and 30 in the Coale and Demeny Model North life tables. More details can be obtained from Jim Oeppen at The Cambridge Group.

Sources: The figures for tontines are from Finlaison, *Report on Life Annuities*; for the aristocracy, from Hollingsworth, "The Demography of the English Peerage," p. 56; for the reconstitution sample (men aged 30), from Wrigley and Schofield, *Population History*, p. 250; for the Southern Quakers (men aged 25–30), from Vann and Eversley, *Friends in Life and Death*, p. 229; for Scottish advocates, from Houston, "Mortality in Early Modern Scotland," p. 51; for fathers in marriage licenses, from data in this article. For Members of Parliament the figures used are those listed in Table 8 of this article; they include M.P.s younger than 29 when entering Parliament.

A number of more recent studies confirm this conclusion. Table 10 brings together all the available evidence, expressed in the form of male life expectancy at 25 years of age. The data are arranged in the sequence in which they were published. The overall finding is that, with the exception of the reconstitution sample and South-of-England Quakers, there was an increase of about ten years in adult life expectancy between the seventeenth and eighteenth centuries. Table 10 shows that the increase occurred throughout the whole eighteenth century, though more detailed analysis reveals particularly sharp gains at its beginning. Whether this fall in mortality was sufficient to account for the whole of population growth is a question that can only be answered by further research.[49]

[49] A ten-year increase in life expectancy at birth would more than adequately explain population growth between 1695 and 1841, assuming that fertility was high during the eighteenth century. Given that the marriage licenses indicate a low age at first marriage of women in the late

EXPLANATIONS FOR THE FALL IN MORTALITY

What were the reasons for this radical decline in adult mortality? I have previously argued that smallpox inoculation made a significant impact on mortality in the late eighteenth century. In rural areas, where the majority of the population lived, this would have led to a reduction in adult mortality as well as child mortality, in spite of a gradual increase in the virulence of the disease.[50] The data for Members of Parliament, the aristocracy, and the Quakers indicate a pronounced increase in life expectancy after 1750, which could be accounted for by the practice of inoculation during that time. However, smallpox inoculation was not practiced on any scale in the first half of the eighteenth century so cannot account for the marked fall in mortality found then. It is therefore necessary to consider other explanations for that period.

Real incomes probably rose for most of the population during the first half of the eighteenth century.[51] It is thus possible that this improvement played a part in reducing mortality. Certainly the evidence of higher mortality among husbandmen in the early seventeenth century would suggest that economic factors were important during this early period, but the weight of evidence suggests that they were not central in bringing about the overall fall in mortality. The substantial mortality gains among all the socioeconomic groups discussed in this article indicate that noneconomic forces were of primary importance. Only further research will definitively settle this issue.

It is possible that there was a spontaneous decline in the severity of various diseases at the end of the seventeenth century. However, there is no evidence for this; smallpox, for example, was increasing in virulence throughout the eighteenth century. Certain changes in the environment associated with economic development may have played a role in reducing mortality; for example, there is good evidence that malaria was present in the marshlands of southeastern England, and the draining and enclosure of those areas may have reduced mortality.[52] However, the disease was probably confined to restricted areas of the country.

We can provisionally explore one hypothesis that fits all the known evidence: that the main fall in mortality during the early eighteenth century occurred because of the marked improvement in domestic hygiene associated with the rebuilding of English housing at that time.

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seventeenth century, this assumption is not unrealistic. Developing a model of population change that reflects the mortality changes discussed in this paper is a priority for future research. I am grateful to Jim Oeppen for commenting on the implication of the changes in mortality for population growth.

[50] Razzell, *The Conquest of Smallpox*.

[51] Wrigley and Schofield, *Population History*, p. 643.

[52] Dobson, "The Last Hiccup of the Old Demographic Regime," p. 413.

It was linked with a move away from older building materials—in particular, earthen floors, which had been commonplace since medieval times in the houses of rich and poor alike. In the seventeenth century, according to M. W. Barley, even among the clergy, "Earth floors were almost universal; even if suitable stone was available locally for flagging the hall, the service rooms still had earth floors throughout this period . . . The use of brick for paving, as for infilling, belongs to the period after 1660."[53] In their history of English housing Bill Breckon and Jeffrey Parker draw attention to a neglected, if colorful, area of social history:

> Up to the 18th century . . . the ground floor of the house was simply beaten earth . . . dusty and strewn with straw, rushes or grasses . . . [with] some nastiness seeping into the floors, not only from dog and cat excrement but from human urine as well, for our ancestors were not too bothered about sanitation. Whatever its source, the result was that the floors soaked up material rich in nitre—the "saltpetre" used in making gunpowder. Since this was scarce, the Crown turned to floors as a rich source of much-needed war material, and empowered "saltpetre men" to enter people's homes, dig up and take away their floors.[54]

The demand for saltpeter for the manufacture of gunpowder was of such critical importance that these men were allowed to dig up the floors of bedrooms, halls, butteries, and other rooms in houses as well as the floors of churches, town halls, pigeon lofts, and stables.[55] This activity created passionate opposition, particularly when it involved the digging up of earth under the beds of invalids, pregnant women, and old people.[56] Some householders managed to avoid having their houses disturbed by bribing the government's men. However, the importance of this extraction from our point of view is that it indicates the highly unhygienic state of many English houses' floors in the seventeenth century. The "powers of seisin" of the saltpeter men were revoked in 1656, though the practice of using house floors as a source of saltpeter seems to have continued until the end of the seventeenth century, when its importation by the East India Company made the practice redundant.[57]

Barley gives a detailed account of the history of farmhouses and cottages, in which earthen floors persisted until the early eighteenth century. Church records for Lincolnshire and Bedfordshire reveal that in parsons' houses during Queen Anne's reign,

> Earthen floors were still very much the rule rather than the exception . . . some houses could be found with nothing else. . . . The next best thing was brick, and about half of the Lincolnshire houses had one room so paved . . . usually the hall.

[53] Barley, "Rural Housing in England," p. 727.

[54] Breckon and Parker, *Tracing the History of Houses*, pp. 135–36.

[55] Hodgetts, *The Rise and Progress of the British Explosives Industry*, pp. 12–28, 213–300.

[56] Ibid.

[57] See Clarke, *The Natural History of Nitre*, p. 21, for a reference to the continuation of the practice after the 1656 legislation.

> In Bedfordshire the majority of halls were paved, and so were about half the kitchens.[58]

The persistence of earthen floors into the late seventeenth century perhaps explains some unsanitary practices of the aristocracy during this period. When Charles II and his court spent the summer of 1665 in Oxford to escape the plague, they were castigated by the diarist Anthony Wood: "Though they were neat and gay in their apparell, yet they were very nasty and beastly, leaving at their departure their excrements in every corner, in chimneys, studies, colehouses, cellars."[59] That such unhygienic practices were commonplace is suggested by Pepys's diary; he himself used a chimney for not dissimilar purposes.[60] This behavior was probably due to the absence of toilets in most houses, even those of the rich, until the eighteenth century.[61]

Barley's work suggests that earthen floors were gradually replaced as brick was widely introduced for domestic house building, a process triggered by the great town fires that swept through England during the late seventeenth and early eighteenth centuries. The timing of the process of rebuilding in brick and tile coincides with the early-eighteenth-century decline of adult mortality previously discussed.[62] This rebuilding of houses appears to have enabled a revolution in domestic hygiene to take place. As early as 1727 De Saussure could write,

> The amount of water English people employ is inconceivable, especially for the cleansing of their houses. Though they are not slaves to cleanliness, like the Dutch, still they are very remarkable for this virtue. Not a week passes by but well-kept houses are washed twice in every seven days, and that from top to bottom; and every morning most kitchens, staircase, and entrance are scrubbed. All furniture, and especially all kitchen utensils, are kept with the greatest cleanliness.[63]

Whether this account was true of just London or the whole country is open to question, but certainly the eighteenth-century English acquired a reputation for domestic cleanliness that was reflected in the writings of other foreign visitors.[64]

CONCLUSION

The growth of population in eighteenth-century England was primarily due to a fall in mortality, which was particularly marked during the first half of the century. As the fall appears to have affected all

[58] Barley, *The English Farmhouse and Cottage*, p. 258.
[59] Quoted in Wright, *Clean and Decent*, p. 76.
[60] Hibbert, *The English*, p. 335.
[61] Ibid., pp. 196, 335.
[62] Jones and Falkus, "Urban Improvement and the English Economy," pp. 120, 145, 146.
[63] De Saussure, *A Foreign View of England*, p. 157.
[64] Wilson, *Strange Island*, pp. 119, 125, 129.

socioeconomic groups, it does not seem to be explained by economic improvements. The introduction of smallpox inoculation made a major contribution to the phenomenon, but the major hypothesis considered here is that there was a very significant improvement in domestic hygiene linked with the rebuilding of housing in brick and stone. This was triggered by the great town fires that swept England in the late seventeenth and early eighteenth centuries, but was also associated with a general shift in attitude toward hygiene.

This article poses major questions about population, economy, and society. More research is required before authoritative conclusions can be reached, particularly about the causes of population growth. Research using local censuses, parish registers, and marriage licenses will allow an analysis of variations in mortality by town and region and of changes over time. Additionally, detailed work will have to be undertaken on the history of hygiene and its impact on health and illness. Only when this research has been undertaken—which is likely to constitute a major project over a number of years—will it be possible definitively to explain population growth in eighteenth-century England.

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Essays in English Population History

To Lyn Melville-James

Essays in English Population History

Peter Razzell

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Introduction

I was first introduced to the subject of English population history as an undergraduate at Birmingham University in the early 1960s, during a series of lectures by David Eversley on eighteenth-century population growth. Eversley quoted Malthus's belief that population increase "had arisen more from the diminution of deaths than the increase of births",[1] and told us how most writers on the subject since Malthus – Rickman, Farr, McCulloch, Griffiths and Buer – had explained population increase in terms of falling mortality.[2] This emphasis on a fall in the death rate had been based on official population and parish register returns, but Marshall in the late 1920s and Habakkuk in the early 1950s, questioned this interpretation of the evidence, and suggested that fertility may have played a central role in population growth.

There was not only disagreement about the mechanisms of population change – the relative importance of mortality and fertility – but the traditional explanation of population increase had also been found wanting. Up to the 1950s, the consensus had been that population had grown mainly as a result of falling mortality, which in turn was due to improvements in medical and public health provision. In 1955 this consensus was challenged by Thomas McKeown and R. G. Brown. They argued that improvements in medicine did not occur as described by Griffiths, Buer and others, and had been ineffectual in treating disease and illness before the twentieth century.[3]

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[1] Quoted in T. H. Marshall, "The population problem during the industrial revolution: a note on the present state of the controversy" in D. V. Glass and D. E. C. Eversley (eds.), *Population In History* (1965).

[2] See Marshall, *op. cit.* and D. V. Glass, "Population and population movements in England and Wales, 1700 to 1850" in D. V. Glass and D. E. C. Eversley, *op.cit.*

[3] See Thomas McKeown and R. G. Brown, "Medical evidence related to English population growth in the eighteenth century", in Glass and Eversley, *op. cit.*

McKeown and Brown concluded that if medical improvements could not explain population growth, then economic factors – by default – must have been responsible.

Population was generally accepted to have grown most rapidly at the end of the eighteenth century. Yet there was serious doubt as to whether the average standard of living had been increasing during this period. In the early 1960s the controversy over the standard of living was in full swing, with no obvious resolution to the argument one way or the other. I left Eversley's lecture both fascinated and frustrated by the lack of an intellectual resolution to a problem of such obvious central importance: a major historical shift in population linked to the industrial revolution which had transformed English society – but a shift which could not be explained by current knowledge or thinking.

After graduating with a degree in sociology from Birmingham, I spent a post-graduate year at Chicago University, and with the help of Professor Janowitz, began investigating the social origins of army officers. I was interested in the transformation of English society during the industrial revolution period, and felt that an examination of patterns of social stratification in the army would reveal key elements in the changing social structure. I found that there had been an influx of sons of the gentry into the army at the end of the eighteenth century,[4] and became intrigued as to why this influx had occurred. This question led me to analyse the expectation of life of gentry families in Hertfordshire and Northamptonshire, which revealed a marked increase in life expectancy from the middle of the eighteenth century onwards.[5] This confirmed T. H. Hollingsworth's earlier finding of a significant fall in infant, child and adult mortality amongst the aristocracy during the eighteenth century.[6]

I searched the literature for factors which might explain increasing expectation of life amongst the gentry and aristocracy. For such wealthy groups, increases in the per capita consumption of food were unlikely to be relevant. The introduction of smallpox inoculation in the early 1720s, and its practice on a wide scale after the 1760s, seemed to fit the known

[4] P. E. Razzell, "Social origins of officers in the Indian and British Home Army: 1758–1962", *British Journal of Sociology*, Vol. 14 (1963).

[5] See Chapter 1.

[6] See T. H. Hollingsworth, "A demographic study of the British ducal families", *Population Studies*, Vol, 11 (1957).

evidence. I wrote up my findings,[7] elaborating the thesis that inoculation could account for most of the reduced mortality and increased population of late eighteenth century England.

This work attracted interest and attention, but it suffered from a lack of reliable demographic evidence. The central problem for all work in this field was the unknown quality of the raw data – baptisms, marriages, and burials – and the lack of reliable statistics of nuptiality, fertility and mortality. Without reliable evidence, it was impossible to critically assess the role of inoculation: was population growth mainly due to a fall in mortality, and was the chronology of this fall compatible with the introduction and practice of inoculation? Or was population increase mainly due to an increase in fertility, independent of the introduction of inoculation?

I decided to tackle one aspect of this problem of unreliable demographic evidence by comparing information in the 1851 and 1861 censuses with that listed in the parish registers. A sample of 45 parishes was selected with which to evaluate the reliability of baptism and burial registration.[8] From this research, I found a sharp fall in mortality during the first four decades of the nineteenth century, well after the period in which inoculation had been generally introduced. As a result, I began to revise my earlier conclusions about inoculation and its role in the reduction of mortality.

The problem of population growth became more rather than less complex. For further fruitful work, it was necessary to gather further reliable demographic evidence. I engaged in research on the reliability of baptism registration by comparing census, parish and civil register data, but this work was never published. I left academic life for about ten years to work in publishing, and in effect left unresolved an intellectual problem which had fascinated me for nearly twenty years.

Subsequently, I was reading through historical journals for a publishing project I was working on, when I came across an article by Tony Wrigley in *Past And Present*, in which he claimed to have resolved the conundrum of eighteenth century population growth.[9] On the basis of the Cambridge Group's research findings, he and Roger Schofield argued that there had been a marked fall in the age at marriage, leading to an increase in fertility

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[7] See Chapter 1 of this book, and Peter Razzell, *The Conquest Of Smallpox* (1977).

[8] See Chapter 4 of this book.

[9] E. A. Wrigley, "The growth of population in eighteenth-century England: a conundrum resolved", *Past And Present*, Vol. 98 (1983).

and population during the eighteenth century,[10] and this overall conclusion had become something of an orthodoxy in the field of early modern English population history.

The Cambridge Group's presentation of reliable raw material on baptisms, marriages and burials, along with family reconstitution data, was clearly an important contribution to the history of population. I had, however, been unconvinced by the methods used to process this data when Wrigley and Schofield's early findings were published in the 1970s, and indeed this had been part of the stimulus for my own work on parish register reliability. The claim that the population conundrum had been resolved presented a challenge, and I set out to assess its validity. Although impressed by the empirical and intellectual scope of the Cambridge Group's enterprise, I found myself unconvinced by the central arguments of *The Population History Of England.* This was partly the result of my own earlier work on parish register reliability – see Chapters 4 and 5 of this book – which had cast doubt on the assumptions made by the Cambridge Group on the adequacy of parish registers in the late eighteenth and early nineteenth century.

My new research culminated in a critical review of the Cambridge Group's work, which has been recently published as an article in the *Journal Of Economic History.* (Chapter 7 of this book.) The essence of this critique is that Wrigley and Schofield have made a large number of theoretical assumptions which are not supported by empirical evidence. For example, they have made major adjustments to the 1871 Census figures on age structure – which is the starting point of their back projection programme – and yet the detailed comparison of censuses with parish and civil registers suggests that age statements in nineteenth century censuses were of a high order of reliability. (This is discussed in Chapters 4 and 5.) I concluded that the balance of evidence did not support the Cambridge Group's argument, but on the contrary favoured the classical view that mortality was the most important factor in population change. I have argued in Chapter 7 that much of this fall in mortality occurred at the *beginning* of the eighteenth century, and that the most likely explanation of the fall during this period is an improvement in domestic hygiene associated with the rebuilding of houses in brick and tile.

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[10] E. A. Wrigley and R. S. Schofield, *The Population History Of England* (1981).

The conclusion about falling mortality was mainly based on non-parish register evidence, as parish registers are of such questionable reliability. David Glass had begun detailed empirical research on the quality of parish registers by comparing them with information on births and deaths from tax returns. Unfortunately, these sources of data were not independent of each other, and Professor Glass's results were of limited value. However, the methodology of comparing data from a number of sources – the principle of triangulation – is very appropriate for the evaluation of parish registers. I have begun a programme of checking the quality of burial registers by comparing them with wills, poor law records, bishops transcripts and monumental inscriptions. I have also developed a method – the same-name technique – to measure both baptism and burial registration reliability. The preliminary results of this research will be found in Chapters 7 and 8. As a result of this work, I believe that it will eventually be possible to come to definite conclusions about the population history of England.

In the meantime, I am presenting my essays on English population history, primarily to re-open the debate on eighteenth century population growth. This edited collection of essays, written over the last thirty years, is presented in the sequence in which the essays were written. As I have modified a number of my views, I have rewritten parts of the original articles, but most of these changes are relatively minor, involving style and presentation, rather than substance. I have included a previously unpublished essay on the further evaluation of baptism registration, which forms Chapter 5. In addition, I have written a concluding chapter specially for the book, which discusses the questions of the reliability of demographic evidence and explanations of eighteenth-century population growth, and draws together a review of work by other scholars with some of my own recent research findings.

Most of the essays in the book are of a non-technical nature, and are suitable for the reader with a general interest in English economic and social history. Others – in particular Chapters 4 and 5 – are rather technical, and the non-demographic reader may prefer to read them lightly, or perhaps skip them altogether. I hope I have succeeded in conveying some of the intellectual excitement involved in pursuing a question that has been so central to English economic and social history, and which still remains unresolved.

Peter Razzell

Chapter 1

Population Change in Eighteenth-Century England: A Reappraisal[1]

This was my first essay on English population history. I argued in this article that the practice of smallpox inoculation could in principle account for the whole of the increase in population at the end of the eighteenth century. Since this article was written, I have modified my view and no longer believe that inoculation was the sole major determinant of population growth. I have changed my thinking for two reasons. First, new research on the comparison of census and parish registers (see Chapter 4) suggested that there had been a significant fall in mortality during the first four decades of the nineteenth century, after the general introduction of inoculation. Second, new evidence has emerged (see Chapter 7) to suggest that there was a substantial fall in adult mortality at the beginning of the eighteenth century, before the introduction of inoculation.

The article still stands as a detailed empirical investigation of one highly effective prophylactic eighteenth-century medical practice well before the twentieth century, which clearly had a significant impact on mortality.

Two traditional explanations have been proposed for the acceleration of population growth which occurred in the middle of the eighteenth century. First, the neo-Malthusian view that it was a consequence of the industrial and agricultural revolutions through an improved standard of life. Second, that it was the result of various medical innovations independent

[1] First published in *The Economic History Review*, Vol. 18 (1965).

of these revolutions. The problem posed by these competing interpretations is central to English economic and social history: did the industrial and agricultural revolutions create their own future labour force and expanding numbers of consumers, or were they themselves children of a population revolution which preceded them?

Economic historians have attempted to answer this question by estimating population, birth- and death-rates at decennial intervals throughout the eighteenth century. Professor Krause, however, has questioned the validity of this method for the period before 1781 when national aggregate statistics of Anglican baptisms and burials are available only for every tenth year from 1700 to 1780. He has pointed out that the use of one conventional assumption about English demographic data with reference to Sweden would exaggerate the amount of actual increase of population in that country between 1750 and 1780 by over 61 per cent.[2] Krause has attempted to use the statistics of annual baptisms and burials from 1780 onwards by making certain questionable assumptions about changes in the baptism/birth and burial/death ratios during the period 1781–1850. He concluded that a rise in the birth-rate rather than a fall in the death-rate was "the major variable in English demography".[3]

This has led the medical historians McKeown and Record to state that "the data [on mortality and natality] are so treacherous that they can be interpreted to fit any hypothesis, and it seems preferable to rely on assessment of the sensitivity of the birth-rate and death-rate, and their relative effectiveness, in a period when both rates were high."[4] This they had done in their own work and after reviewing the history of all the major diseases and preventive measures taken against them, concluded that the "fall in the death-rate during the eighteenth and nineteenth centuries was not the result of medical treatment as Griffiths and others had supposed. Only in the case of vaccination against smallpox is there any clear evidence that specific therapy had a substantial effect on the prevention or cure of disease earlier than the twentieth century. The decline in

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[2] J. T. Krause, "Changes in English fertility and mortality, 1781–1850", *The Economic History Review*, Vol. 11 (1958–9), p. 53.

[3] *Ibid*, p. 69.

[4] T. McKeown and R. G. Record, "Reasons for the decline in mortality in England and Wales during the nineteenth century", *Population Studies*, Vol. 16 (1962), pp. 94–5.

mortality from diseases other than smallpox was due to improvement in living conditions, and to changes in virulence and resistance upon which human effort had no influence."[5]

Krause, however, has pointed out that vaccination did not become really widespread until the 1840s and has argued that the average standard of living probably deteriorated slightly between 1780 and 1821 when population was increasing very rapidly.[6] Chambers, in his study of the Vale of Trent region, examined the relationship of food-supply to mortality-rates and concluded that population "was vulnerable to disease, but not as a result of famine. Epidemics could do their own work without its aid, nor, it would seem, did they require the assistance of gin."[7] A similar conclusion was reached by Pickard after analysing the relationship between food prices and changes in mortality and natality in eighteenth-century Exeter.[8] It should also be remembered that from 1838 to 1875, when the standard of living was undoubtedly rising rapidly, the overall death-rate was virtually constant.[9] It is in the light of all these contradictory facts that McKeown and Record have been reduced to making the following desperate statement: "When we have eliminated the impossible [medical explanations of population growth], whatever remains [economic explanations], however improbable, must be the truth."[10]

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[5] McKeown and R. G. Record, "Medical evidence related to English population changes in the eighteenth century", *Population Studies*, Vol. 9 (1955), p. 139.

[6] Krause, *op. cit.*, pp. 63–5.

[7] J. D. Chambers, "The Vale of Trent, 1670–1800", *The Economic History Review, Supplement 3*, p. 29.

[8] R. Pickard, *Population And Epidemics Of Exeter* (1947), p. 67.

[9] B. R. Mitchell and P. Deane, *Abstract Of British Historical Statistics* (1962), pp. 36, 343–58.

[10] McKeown and Record, *op. cit.*, pp. 94, 95.

I

This paper is intended as a summary of research to date on the causes of the increase in population in eighteenth-century England.[11] Before discussing these causes it is necessary to estimate the size of population during the eighteenth and early nineteenth centuries, in order to appreciate the magnitude of change during this period. The estimates of population used in this paper are those derived from the returns of marriages made from several thousand parishes which were published by Rickman in 1841.[12] These estimates have several advantages: (a) unlike baptisms and burials, the overwhelming majority of dissenters' marriages took place in the Anglican church;[13] (b) the registration of marriage is generally considered to have been the most reliable;[14] (c) the estimates are based on three-year clusters of returns rather than single years, a procedure which is much more likely to reduce fluctuations of the marriage-rate from one time to another.[15] The basis of Rickman's own estimate was the assumption that the ratio of the number of marriages to total population in the eighteenth century, was the same for the periods 1699–1701 and 1749–51, i.e. that

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[11] The paper is really a series of hypotheses illustrated occasionally by statistical and other evidence. It is hoped to incorporate detailed evidence into a monograph at a later date. (1994): Published as *The Conquest Of Smallpox* (1977).

[12] Rickman's figures for marriages were generally derived from over 4,000 parish registers. See G. Talbot Griffiths, "Rickman's second series of eighteenth century population figures", *Journal Of The Royal Statistical Society*, Vol. 92 (1929), p. 263.

[13] The best confirmation of this is to be found in the *Report On Non-Parochial Registers*, (Parliamentary Papers 1837–8, XXVIII), where it is seen that there were virtually no non-Anglican marriage registers kept for the eighteenth century.

[14] See J. C. Cox, *The Parish Registers Of England* (1910), p. 76; W. E. Tate, *The Parish Chest* (1946), p. 65; G. Talbot Griffiths, *Population Problems Of The Age of Malthus* (1926), p. 33.

[15] An examination of the Swedish statistics for the eighteenth century, for example, shows that three-yearly clusters fluctuated far less than single years in terms of the marriage-rate. See *Historical Statistics of Sweden, 1720–1950* (1955), pp. 39–41. The long-term marriage-rate in Sweden was remarkably stable between 1751 and 1825. See G. Sundbarg, *Sweden, Its People And Its Industry* (1904), p. 96.

the marriage-rate was constant between 1700 and 1800. It is impossible to test this assumption in any detail, although there are a few scattered statistics available to suggest that it is not too unreasonable.

Table 1. The Marriage Rate Throughout the Eighteenth Century[16]

Place	*Total Population*	*Approximate Period*	*Marriage Rate Per 1,000 Population*
7 market towns	27,043	1724–36	8.7
54 villages	19,607	1724–36	8.4
11 towns	37,541	1770s	8.5
England & Wales	8,892,436	1795–1805	8.8

These figures must not be taken too literally, as they refer to places of different sizes and locations; the figure for 1795–1805 is somewhat arbitrary because of the flaws in the registration of both marriages and population.

However, the figures for marriage-rates indicate that there were no marked long-term changes in the marriage-rate throughout the eighteenth century. This conclusion is confirmed by at least one local study of population change during the same period.[17] The estimates of population size from the returns of the number of marriages are as follows:

[16] Thomas Short, *New Observations On Bills of Mortality* (1751), p. 133; J. Howlett, *Observations On The Increased Population ... Of Maidstone* (1782), p. 82. I have excluded from the 1795–1805 population figure the numbers in the army and navy; also I have not corrected for under-enumeration, as a few marriages were also not registered because of the non-Anglican marriage of Quakers, Jews, and Roman Catholics, as well as various illicit marriages in sea-ports and elsewhere. For the source of the population figure see *Census Of Great Britain, 1851*, pp. xxiii, xxvi.

[17] Chambers, *op. cit.*, pp. 54, 55.

Table 2. Estimated Population Size in Eighteenth-Century England and Wales[18]

Period	*Estimated Population (nearest 1,000)*	*Average Annual Rate of Percentage Increase*
1700	5,307,000	
1750	5,895,000	+ 0.2%
1801	9,337,000	+ 1.1%
1851	17,719,000	+ 1.8%

Although we have indicated that the marriage-rate was only stable during the eighteenth century, it is possible to check the earlier population estimates with figures derived from an independent source. Gregory King estimated the population of England and Wales to be 5.5 millions in 1695, an estimate which Professor Glass thinks may be slightly too high.[19] King's estimate was based on hearth-tax returns and local censuses conducted in connection with the tax on marriages; it is similar to the one we have made for 1700 on the basis of the marriage returns. The population increased relatively slowly up to 1750, after which it increased rapidly and steadily right through to the end of the nineteenth century. It is the causes of this rapid and consistent increase which are the subject of this paper.

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[18] These estimates are recomputations of Rickman's figures. The following adjustments were made: (1) 5 per cent was added to the 1801 enumerated population because of estimated under-enumeration. See Krause, *op. cit.*, p. 60. (2) Rickman took the number of marriages in the single year 1800 as the basis of his marriages/population ratio. This has been recomputed on the basis of the years 1800–02 so that the basic ratio is derived from a three-year cluster of marriages like all the previous periods. The original estimates are those Rickman arrived at by treating England and Wales as one unit, and may be found in Griffiths's article in *Journal Of The Royal Statistical Society*, Vol. 92 (1929), p. 263. See also J. Rickman, *Parishes Possessing Registers Extant 1570 And 1600 With Their Population In 1801*, (Document M. 74 10 in the General Register Office Library). (3) No allowance was made for the numbers in the armed service. The population figures are not intended as exact estimates, but rather as indications of the magnitude of change in the size of the population during the eighteenth and early nineteenth centuries. For the source of the 1801 and 1831 figures, see *Census of Great Britain, 1851*, pp. xxii, xxiii, xxvi.

[19] D. V. Glass, "Gregory King's estimate of the population of England and Wales, 1695", *Population Studies*, Vol. 3 (1950), p. 358.

Ideally, we should want to analyse the aggregate birth- and death-rates, age-specific fertility and mortality-rates. Unfortunately, the paucity of accurate information means that we can only collect data of a piecemeal kind, which at least points in the direction of certain conclusions. It has already been indicated that the aggregate marriage-rate changed but little during the eighteenth century. This conclusion is consistent with the fact that the age at marriage of spinsters appears to have been virtually constant during the same period.

Table 3. Mean Age at Marriage of Spinsters, 1615–1841[20]

Period	*Region*	*Mean Age at Marriage*	*Number in Sample*
1615–1621	Wilts., Berks., Hants. & Dorset	24.6	280
1662–1714	Yorkshire	23.8	7,242
1701–1736	Nottinghamshire	24.5	865
1741–1745	Surrey	24.9	333
1749–1770	Nottinghamshire	23.9	700
1796–1799	Sussex	24.1	275
1839–1841	England & Wales	24.3	14,311

These provisional findings indicate that eighteenth-century population increase was not brought about by a lowering of the age at marriage or by an increase in the marriage-rate.

If changes in marriage patterns were not responsible for population growth, what is the evidence that a reduction in mortality was involved? Although no reliable information for the general population is available, it is possible to construct good-quality data for special groups. Two detailed genealogical volumes for county families have been published in the Victoria County History series for the counties of Hertfordshire and

[20] Rev. E. Nevill (ed.), *Marriage Licences Of Salisbury, 1615–1682*; M. Drake, "An elementary exercise in parish register demography", *The Economic History Review*, Vol. XIV (1962), p. 444; T. M. Blagg and F. A. Wadsworth (eds.), *Nottinghamshire Marriage Licences*, (The Index Library, British Record Society); R. Bax (ed.), *Allegation For Marriage Licences Issued By The Commissary Court of Surrey, 1673–1770* (1907); D. Macleod (ed.), *Sussex Marriage Licences, 1775–1800*, (Sussex Record Society, Vol. XXXV, 1929); *The Registrar General's Fourth Annual Report* (1842), p. 10.

Northamptonshire. The following calculations of average age lived from birth were made from the information in these volumes.

Table 4. Changes in the Average Age Lived From Birth (County Families)[21]

Cohort born	*Average Age Lived From Birth (Males)*	*Number in Sample*
1681–1730	37 years	138
1731–1780	48 years	130
1781–1830	50 years	162

The results of this study were compared with those published by Hollingsworth in his paper on the demographic history of ducal families, as well as the results of his unpublished research into the whole of the aristocracy.[22] All these studies point to the same conclusion: that expectation of life for cohorts born from circa 1740 onwards rose significantly, the saving of life occurring mainly among infants, children, and young adults.[23] A more detailed analysis of the 'county family' material illustrates the sharpness of this rise.

Table 5. Changes In The Average Age Lived (County Families)

Cohort Born	*Average Age Lived From Birth (Males)*	*Number in Sample*
1680–1699	36 years	92
1700–1719	38 years	89
1720–1739	35 years	86
1740–1759	48 years	76

Unfortunately it is impossible to construct similar tables for the general

[21] Samples were taken from the Northants and Herts genealogical volumes of the *Victoria County History* series published in 1906 and 1907. Figures were computed to the nearest year.

[22] T. H. Hollingsworth, "A demographic study of the British ducal families", *Population Studies*, Vol. XI (1957).

[23] Hollingsworth's figures for the whole aristocracy, which are based on much larger cohorts, indicate that the rise in life expectancy was somewhat more gradual than this, and began at the beginning of the eighteenth century. The chronology of the rise in expectation of life is dealt with in more detail in the Chapter 7 of this book.

population during the same period. It is probable that there was an equivalent rise among the general population, for the mean expectation of life at birth derived from Gregory King's life-table for Lichfield in about 1695 was 32.0 years,[24] whereas according to the English life-table constructed by Farr in 1841 it was 41.2 years.[25] If these figures are representative, the aristocracy and gentry always had a higher life expectancy than the general population but managed to increase their relative advantage slightly throughout the eighteenth and early nineteenth centuries.

What are the possible causes of the increase in expectation of life throughout the eighteenth century? For obvious reasons, an explanation in terms of increased food supplies is inappropriate for social groups such as the gentry and aristocracy. There is one major plausible explanation which fits the known evidence: the introduction and use of inoculation against smallpox during the eighteenth century. Inoculation must formally be contrasted with the nineteenth-century practice of vaccination. Inoculation is the injection of smallpox virus taken from the vesicle of a person suffering from smallpox, whereas vaccination is the injection of cowpox virus. The two injections are conventionally distinguished by the different symptoms they produce. Inoculation is thought of as giving rise to pustular eruptions in different parts of the body as well as at the site of injection, and is viewed as a mild form of natural smallpox, inasmuch as it is believed to spread the natural disease from the inoculated person to other unprotected people. Vaccination only gives rise to a vesicle at the site of the injection and is not infectious to other unprotected people.[26]

II

Inoculation was originally practised sporadically and on a very limited scale as a part of folk medicine, mainly in Oriental and African countries. It was introduced into England in 1721, when Lady Mary Wortley Montagu had her daughter inoculated in London, although it had been known by report for some years previously. It was practised on only a very limited

[24] See Glass, *op. cit.*, p. 368 for the reliability of this figure.

[25] *Fifth Annual Report Of The Registrar General* (1843), p. 29.

[26] The relationship between vaccination and inoculation is discussed in detail in the next chapter.

scale during the 1720s and 1730s, owing mainly to the fact that the very severe technique of inoculation caused several deaths. Between 1721 and 1728 there were 897 people known to have been inoculated, 17 of whom were suspected to have died from inoculated smallpox. In the early 1740s the practice was revived again mainly as a result of the use of a safer technique involving milder injections of virus. However, because the medical profession had elaborated inoculation from its original simplicity into a very complex operation involving both a fortnight's preparation and convalescence, often in a special isolation hospital, the practice became very expensive, and was consequently restricted to the rich. Although the London Smallpox Hospital was founded in 1746 to offer charitable inoculations to the poor, most of its clients in the early period tended to be servants of the subscribers to the foundation of the hospital.

During the 1750s the overseers of the poor began to pay the cost of inoculation for all the poor within their parish; this usually took place as a response to the threat of a smallpox epidemic which provoked mass inoculation among all members of the parish. In addition to these mass inoculations there were many individuals who were inoculated at their own expense. Thus Kirkpatrick wrote in 1754: "But since we have certain accounts that the populace, who were at first strongly predisposed against this practice, and who so rarely stop at the Golden Mean, are rushing into the contrary extreme; and go promiscuously from different distances to little Market Towns, where without any medical advice, and very little consideration, they procure inoculation from some operator, too often as crude and thoughtless as themselves ..."[27] This popularisation of inoculation was made possible by its cheapness through the activities of local surgeons and apothecaries.[28]

However, inoculation did not become really widespread until after the 1760s, for, according to one source, only 200,000 people had been inoculated in England by 1766.[29] The main reason why inoculation was not more

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[27] J. Kirkpatrick, *The Analysis Of Inoculation* (1754), pp. 267, 268.

[28] This was achieved through the simplification of inoculation, culminating in the abandonment of preparation and convalescence by Lewis Paul Williams (a Leicestershire surgeon) in 1763. See *Northampton Mercury*, 15 December 1768; *The British Medical Journal*, Vol. 11 (1910), pp. 633–34.

[29] See A. C. Klebs, "The historic evolution of variolation", *Bulletin Of The John Hopkins Hospital*, Vol. XXIV (March 1913), p. 82. The basis of this estimate is unknown.

widespread was the occasional mortality still associated with the operation. This situation was changed in the 1760s when the Sutton family began to inoculate by injecting the minimal amount of virus into the arm with the very lightest of scratches. The result was that "if any patient has twenty or thirty pustules he is said to have the smallpox very heavy",[30] thus ensuring a negligible risk of death. The Suttons claimed in 1768 "that about fifty-five thousand had been inoculated by them since the year 1760; of which number only six had died".[31] The 'Suttonian Practice' consisted of Robert Sutton, an apothecary and surgeon at Framlington Earl, Norfolk, and several of his sons, as well as a very large number of non-family partners; the practice extended to most counties and several foreign countries.[32] The most famous son was Daniel Sutton, who, because of his very spectacular feats of inoculation,[33] was chiefly responsible for popularizing the Suttonian method. By the end of 1776 they claimed to have inoculated 300,000 people,[34] a claim which is very plausible in the light of the very large number of partners they had. They offered to inoculate the rural poor gratis on the condition presumably that the rest of the parish were also inoculated by them; certainly the Suttons appear in the account books of innumerable overseers who paid them for mass inoculations in their parishes.

The Suttonian method was soon taken up by the rest of the medical profession, as well as by amateur inoculators who began to proliferate very rapidly. Thus Houlton wrote in 1768 "that in every county of England you meet advertisements of these pretenders and itinerants. ... Some of them as before observed, advertise that they inoculate according to the Sutton method; while others have the modesty to deck their imposition with the style of 'The Suttonian art improved'."[35] Some of these "pretenders and itinerants" were undoubtedly professional surgeons and apothecaries, such

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[30] Creighton, *op. cit.*, p. 476.

[31] R. Houlton, *Indisputable Facts Relative To The Suttonian Art of Inoculation* (1768), p. 10. The negligible risk of death from inoculation after the 1760s is confirmed by a great deal of evidence.

[32] *Ibid*, pp. 21–23.

[33] During a mass inoculation at Maldon, Essex, he inoculated 487 people in one day, none of whom died.

[34] W. R. Clayton, "Notes on the history, incidence and treatment of smallpox in Norfolk", *Norfolk Archaeological Society*, Vol. XXX, p. 7.

[35] Houlton, *op. cit.*, p. 24.

as Dimsdale, who was converted to the Suttonian method by its superiority over the older technique; another professional medical practitioner who later inoculated with the Suttonian method before discovering vaccination was Edward Jenner, who had been inoculated in the old method as a boy during the mass inoculation at Wootton-under-Edge in 1756. Others of the imitators of the Suttonian method were "a certain tribe of empirics and other unexperienced Practitioners",[36] such as the livery servant who left his employment in about 1768 to become a full-time inoculator,[37] and the farrier and blacksmith who inoculated 170 people in the neighbourhood of Norwich in 1769.[38] The occupations of the amateur inoculators ranged from farmer to customs-officer, and some set up schools in their own method of inoculation.

Inoculation was practised much more extensively and earlier in rural areas and small towns than in large towns and cities. Haygarth, writing in 1780, stated that

> whole villages in this neighbourhood (Chester) and many other parts of Britain, have been inoculated with one consent. And it cannot be supposed that the inhabitants of towns are more ignorant or more obstinate. There is not a reasonable doubt that our poor fellow citizens would eagerly and universally embrace a proposal to preserve their children from death and deformity, if the intelligent and the opulent would humanely exert their influence and assistance to carry it into execution.[39]

Although the relative lack of provision of charitable inoculation was one of the major reasons why it spread only slowly in the large towns, another reason was because of the differing structure of smallpox epidemics in town and countryside. In the large towns where the disease was endemic the majority of smallpox deaths were of infants and young children; this tended to engender a fatalistic attitude about the inevitability of catching the disease. This was recognized by Haygarth:

> the lower class of people [in Chester] have no fear of the casual [natural] smallpox. Many more examples occurred of their wishes

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[36] M. G. Hobson, *Otmoor And Its Seven Towns* (1961), p. 20.

[37] W. Watson, *An Account ... Of Inoculating The Smallpox* (1768), pp. 71, 72.

[38] *Gentleman's Magazine*, Vol. XXXIX (1769), p. 16.

[39] J. Haygarth, *An Enquiry How To Prevent The Smallpox* (1785), p. 164.

> and endeavour to catch the infection, than to avoid it. This ... prejudice ... probably prevails in other large towns, especially in those which are so large as perpetually to nourish the distemper, by so quick a succession of infants as constantly to supply fresh subjects for infection.[40]

This he contrasted with "small towns and villages, especially where placed in remote situations, the young generation grow up to have a consciousness of the danger before they are attacked by the dreadful disease."[41] This consciousness was also based on the greater fatality of smallpox in isolated areas. One of its results was seen at Blandford, Dorset, in 1766 when a very malignant epidemic of smallpox broke out and "a perfect rage for inoculation seized the town."[42] In the small town or village it was possible for everybody to compare the spectacular differences in mortality of the inoculated and uninoculated during a smallpox epidemic, whereas in a large town it was very difficult to familiarize the poorer classes with the benefits of inoculation owing to the dispersed and piecemeal nature of smallpox mortality.

The relatively slow spread of inoculation in the large towns should not be exaggerated in importance, for only a small minority of the total population lived in such areas. It appears that inoculation was making rapid headway in towns by the very end of the eighteenth century.[43] In the small towns and villages inoculation appears to have been universally practised well before the end of the century. There are innumerable references to mass inoculations in local histories and medical writings for every decade from about 1750 onwards.[44] One of the reasons why parish authorities were so willing to pay for inoculation of their poor was because of the great expenses involved in isolating and nursing the

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[40] J. Haygarth, *A Sketch Of A Plan To Exterminate The Casual Smallpox* (1793), p. 186.

[41] *Ibid*, p. 186.

[42] Creighton, *op. cit.*, p. 513.

[43] Many of these large towns founded dispensaries during the late eighteenth century which provided charitable inoculation. Although the London Smallpox Hospital only inoculated 36,378 people between 1746 and 1805, practitioners such as Daniel Sutton specialized in the inoculation of "the families of artificers, handicraftsmen, servants, labourers" in the Metropolis.

[44] See the appendix on pp. 36–7.

sick during an epidemic of the natural smallpox. The costs were sufficiently great to make many parishes compel everyone within their jurisdiction to be inoculated.[45]

One observer noted in 1771 "that inoculation, which was heretofore in a manner confined to people of superior ranks, is now practised even in the meanest cottages, and is almost universally received in every corner of this kingdom".[46] According to Dimsdale, writing in 1776,

> in the county of Hertford, there have been two methods of public or general inoculation; one to inoculate, at a low price, as many of the inhabitants of any small town or village, as could be persuaded to submit to it, and at the same time were able to pay, refusing all those who had it not in their power to procure the money demanded. The other method has been, where the inhabitants of a town, or a district, of all denominations, have agreed to be inoculated at the same time, the parish officers or some neighbouring charitably disposed persons, having first promised to defray the expense, and provide subsistence for such of the poor, as unable to pay for themselves.[47]

To some extent the emergence of the amateur inoculators served the needs of the poor, who were unable to afford the price of professional inoculation and whose parish was unwilling to pay for a mass inoculation. A supporter of inoculation summed up the extent of the practice by writing in 1805 that "smallpox inoculation was a well-known, proved, and absolute prevention from receiving the natural Smallpox infection, as millions of people now living can testify".[48] Inoculation did not disappear with the introduction of vaccination. On the contrary, it remained very popular, especially with the poorer classes, who were very prejudiced against vaccination. Ironically, inoculation and vaccination appeared to have supplemented one another, in that virtually all of the population during the first half of the nineteenth century were protected by one injection or the

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[45] See S. and B. Webb, *English Local Government - English Poor Law History*, 1 (1927), p. 306; M. F. Davies, *Life In An English Village* (1909), p. 74; E. G. Thomas, *The Parish Overseer In Essex, 1597–1834* (London M.A. Thesis, 1956), p. 394.

[46] *Medical Transactions*, Vol. 11 (1772), p. 279.

[47] T. Dimsdale, *Thoughts On Partial And General Inoculations* (1776), p. 29.

[48] W. Rowley, *Cowpox Inoculation No Security Against Smallpox* (1805), p. 4.

other, sometimes by both.[49] Inoculation was eventually banned by law in 1840 at the instigation of the supporters of vaccination, who believed inoculation spread natural smallpox to the unprotected.

Inoculation was very extensively practised in other countries, in which it was encouraged by legal enactments during the latter half of the eighteenth century – such as Sweden, Russia, and Austria. It appears to have been particularly popular in Ireland where itinerant tinker inoculators proceeded "from village to village several times during the year for the purpose of inoculating the infantile population."[50]

III

In order to determine the significance of inoculation it is necessary to discuss the history of smallpox mortality prior to its effective introduction. By smallpox mortality we mean the proportion of every 100 children born who died from the disease during their lives. There are two methods of estimating such smallpox mortality: (i) multiplying the extent of the disease by its case-fatality rate (allowing for children who would have died before they had a chance to catch the disease); (ii) counting the number of smallpox deaths and expressing it as a proportion of the number of births. Such information is occasionally to be found in parish registers. In a period of static population growth the proportion of smallpox deaths to all deaths will approximate to the ratio of smallpox deaths per number of births. In order to estimate smallpox mortality we will use both methods outlined above. First, however, it is necessary to discuss the problem in interpreting smallpox statistics.There are five major difficulties in using figures of smallpox mortality:

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[49] For an invaluable description of the history of inoculation and vaccination during the first two decades of the nineteenth century, see Dr J. Forbes, "Some account of the small pox lately prevalent in Chichester and its vicinity", *London Medical Repository* (September 1822), pp. 211–15. Vaccination was not introduced into the area until 1812, although all the population appears to have been protected by inoculation at least as early as the beginning of the nineteenth century.

[50] W. Wilde, "Reports on tables of deaths", *Population Census Of Ireland 1851*, (Parl. Pap., 1843, XXIV), p. xii.

(1) The existence of a type of smallpox, known as fulminating smallpox, which does not manifest the classical pock symptoms because of the rapidity with which it kills its victims. It has been discovered only relatively recently, for as a current medical authority on smallpox has observed, "this is 'sledge-hammer' smallpox, and the diagnosis both clinical and at autopsy is impossible unless smallpox is thought of and unless laboratory facilities are available and used to grow the virus".[51] It is impossible to estimate what proportion of all smallpox deaths were of the fulminating kind; generally it would be highest in very isolated communities which lacked a pool of antibodies derived from frequent epidemics.

(2) The variation in the fatality from smallpox in different types of area. This was recognized by Lettsom when he wrote "that in some countries, and even some counties of England, the infection does not appear for the space of some years; but when it does appear, it is more fatal; owing probably to this, that in great towns the infection being always prevalent, it is caught without the accumulated changes of air peculiarly favourable to epidemics; whereas when it comes at stated periods its malignity seems to be augmented by some unknown but deleterious state of the atmosphere".[52] This, we now know, was due to the creation of a pool of antibodies in the large towns through constant recurrence of smallpox epidemics, which it has already been noticed occurred to a lesser extent in isolated areas.

(3) A large number of smallpox deaths were unregistered for other reasons. Lettsom, who had a great deal of experience with the health of the poor in London, estimated that smallpox mortality was nearly twice that recorded in the Bills of Mortality. One major reason was the confusion of disease with the symptoms that it gave rise to, so that a number of young infants dying from smallpox were registered as convulsions deaths.[53] Very young infants are known to be vulnerable to fulminating smallpox[54] and it appears that this could be partly the explanation of

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[51] C. W. Dixon, *Smallpox* (1962), p. 9.

[52] T. J. Pettigrew, *Memoirs Of The Life And Writings Of The Late John Coakley Lettsom* (1817), Vol. 2 , pp. 121, 122.

[53] Creighton quoting Lettsom, stated that "the genetic article 'convulsions' ... swallowed up, in his opinion, a large number of the smallpox deaths of infants." Creighton, *op. cit.*, p. 534.

[54] Dixon, *op. cit.*, p. 324.

this mis-registration.[55] Lettsom also pointed out that from smallpox "some have been deprived of sight; many have been afflicted with the evil and scrofulous complaints, to which they had previously been strangers; many have been disabled in their limbs ... at length, emaciated and debilitated, they have sunk under their miseries, and filled up the amazing list of consumptions; many of which originated from the violence of Natural Smallpox."[56] Smallpox mortality was also much higher when the disease converged with epidemics of other diseases; and, obviously, some of the increased mortality would be ascribed to other diseases.

(4) Pregnant women are particularly vulnerable when attacked by smallpox,[57] the great majority of their children dying because of such an attack. According to Dixon, "in forty-six cases where the infant's condition is recorded [when the mother has been attacked by smallpox], twenty-six were stillborn, and of the twenty born alive, eleven died later".[58] Most of the stillborn children and many infants who died soon after birth were probably not recorded in the parish registers, as they would not have been baptized. Those deaths which were recorded were probably often attributed to some causes other than smallpox, for example, convulsions. According to a doctor of the Bristol Royal Infirmary during the middle years of the eighteenth century, "the female sex whose cases from about 12 years of age to 50 become more dangerous on account of their menstrual discharges, which sometimes coming on in the beginning or State of the Disease proves fatal".[59] Thus the group of potential mothers was particularly vulnerable to death from smallpox, a fact that we shall discuss later in connection with changes in the birth-rate.

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[55] See J. Haygarth, *A Sketch Of A Plan To Exterminate The Casual Small-Pox* (1793), p. 141: "The disease most fatal to infants is convulsions, arising from various causes; one of them is the small-pox. The two circumstances will explain the reason why, under one year old, the proportion of deaths by the smallpox is less than in subsequent periods ...".

[56] Pettigrew, *op. cit.*, Vol. 1, p. 6.

[57] Dixon, *op. cit.*, p. 326.

[58] *Ibid*, p. 113.

[59] *Bristol Infirmary Biographical Memoirs*, Vol. 1, p. 59.

(5) Many people who died of smallpox appear to have been buried in non-consecrated burial pits near the pest-houses or infirmaries used for isolating those sick of the disease. In the Maidstone parish register the incumbent summarized the burials for the year 1760 with the following entry: "Total Burials – 223. Of the Small Pox from Dec. 13 – 59 besides. These carried out of Town 102." It is quite clear from examining the average number of burials in Maidstone that these 102 smallpox victims were not a part of the total 223 burials, a conclusion confirmed by examining the ages of those buried in the churchyard. It is thought that they were buried out at the pest-house because it was quite common practice in the eighteenth century for hospitals to bury their own dead. Both the Northampton and London bills of mortality had yearly returns of the number of people buried in local infirmaries. People responsible for isolating and nursing smallpox victims were also considered responsible for burying them[60] and this was because people were so terrified of smallpox that they feared contact with the corpses themselves; there are references in the literature of incumbents refusing to perform the burial rites and relatives refusing to attend funerals.[61] The existence of non-consecrated burial grounds not only poses a problem for the construction of smallpox mortality statistics but also for demographic studies which assume that burials entered in parish registers represent the total number of deaths.

We are now in a position to estimate total smallpox mortality. There are two methods in arriving at such estimates, the first being to multiply the extent of smallpox by its case-fatality rate. As to the extent of the disease, most writers regarded it as a universal affliction to which all were subject at some time or other, for example, D'Escheray, in his writings on smallpox in England, observed in 1760 that "this distemper spares neither Age nor Sex, Rich and Poor are equally exposed to its influence. What is the most unaccountable, and so wide from all other fevers, is, that the Difference of Constitution is no preservative against its Attack, insomuch, that very few escape it, at one time or other."[62] This universality of smallpox is consistent

[60] See for example W. Le Hardy (ed.), *Calendar To The Herts Sessions Books, 1752–1799*, VIII (1935), p. 226.

[61] See for example Document I.C.1185, 1679 in the Northampton Record Office.

[62] D. D'Escheray, *An Essay On The Smallpox* (1760), p. 2.

with what we know about the nature of the disease; for example, Dr J. F. D. Shrewsbury, the bacteriologist, has written that smallpox is "the most highly infectious of the transmissible diseases of man".[63] It appears from statistical evidence that smallpox was endemic in London as early as at least the sixteenth century; in fact, the disease was so endemic as to be found regularly every week in the bills of mortality during the seventeenth and eighteenth centuries. Smallpox deaths occurred in other large towns during the eighteenth century at least every year. Thus London, and other large towns to a lesser extent, served as smallpox reservoirs from which the disease was constantly exported to the countryside.

The case-fatality rate of smallpox may be estimated from a series of smallpox censuses sponsored by the Royal Society during the 1720s. The figures compiled were for the number of total cases of smallpox sickness with the resulting numbers of deaths in thirty places. Of the 13,192 cases of people suffering from smallpox, 2,167 died: an average case-fatality rate of 16.5 per cent.[64] This figure should be interpreted in the light of the difficulties in using smallpox statistics which we have already discussed. Three of the difficulties are relevant: (i) the figures would exclude cases of fulminating smallpox, the mortality from which is nearly 100 per cent; (ii) large numbers of unregistered deaths would have been excluded, in the ways described by Lettsom; (iii) variations in the fatality of smallpox varied from one type of area to another. With reference to the last difficulty, most of the censuses were conducted in market towns, many of them in Yorkshire and centres of industrial activity. These were towns of very frequently recurring epidemics, which consequently had a lower case-fatality rate than places such as the isolated villages in Worcestershire studied by Eversley. He has written that during the smallpox epidemic of 1720–30 in the area of Bromsgrove "a conservative estimate of the net loss of population at Hanbury is 164 out of the 716 alive in 1715".[65] This was similar to the epidemics in the Shetland Islands, where "formerly the smallpox occasioned the most dreadful ravages in these islands frequently carrying off a fifth part of the inhabitants."[66] In 1720, "the disease was so

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[63] Private Communication, 1964.

[64] For details of the censuses, see Creighton, *op. cit.*, pp. 518, 519.

[65] D. E. C. Eversley, "A survey of population in an area of Worcestershire", *Population Studies*, Vol. 10 (1956–7).

[66] J. Sinclair, *The Statistical Account Of Scotland*, Vol. 2 (1792), pp. 569–70.

fatal as to be distinguished by the name of the mortal pox. On this occasion tradition tells us, in the remote Island of Foula, probably inhabited by about two hundred people, it left only four to six to bury the dead".[67] This type of spectacular smallpox mortality was to be found in other extremely isolated places where the population had no pool of antibodies to protect them.[68]

One contemporary medical observer noted "that when the smallpox is epidemic, entire villages are depopulated, markets ruined, and the face of distress spread over the whole country".[69] Certainly epidemics of the fatality of the one in Hanbury occurred quite often.[70] As about 23 per cent of the total population of Hanbury was wiped out, the case-fatality rate must have been considerably higher than this, for many of the older members of the village must have had smallpox when they were younger. Thus it appears that the case-fatality rate of 16.5 per cent derived from the smallpox censuses in the market towns is too low for the country as a whole. It is impossible to estimate total smallpox mortality using the present method; suffice it to say that smallpox was a universal disease with a recorded case-fatality rate varying from 16.5 per cent to 97 per cent.

The other method of estimating smallpox mortality is to use the parish registers and bills of mortality. Ideally, we would like to express the number of smallpox deaths as a proportion of the number of births. This is not always possible because of the lack of information about births and the deficiencies in registration. When it is not possible the proportion of smallpox deaths to all deaths will be used, as it will generally approximate to the smallpox deaths/births ratio because of the relatively equal number of births and deaths during a period of static population. The smallpox mortality-rate in the eighteenth century varies from 11.6 smallpox deaths

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[67] Robert Cowie, *Shetland: Descriptive & Historical* (1871), pp. 73–5. See also Sinclair, *op. cit.*, Vol. XX (1798), p. 101, for another description of this epidemic.

[68] See E. W. and A. E. Stearn, *The Effect Of Smallpox On The Destiny Of The American Indian* (1945); also *Royal Commission On Vaccination, 1st Report* (1889), pp. 109, 110.

[69] James McKenzie, *The History Of Health* (1760).

[70] See the *Parish Register Of Burford* in 1758; also *Gentleman's Magazine*, Vol. XLII (1772), p. 542. Many of the mass inoculations suggest that a very large proportion of village populations were vulnerable to smallpox. For example, at Irthlingborough, Northants, "upwards of Five Hundred People" were inoculated in 1778, whereas the total population was only 811 by 1801.

per 100 births in London during 1730–39,[71] 20 per 100 deaths in Dublin during the two approximate 30-year periods 1661–90 and 1715–46,[72] to an extreme proportion of 50 per 100 deaths in Great Chart, Kent, during 1688–1707.[73] The majority of records (mainly for towns) yield an average figure of about 15 per cent of all births and deaths due to smallpox during the first half of the eighteenth century. All of the difficulties outlined earlier in the paper apply to these statistics, and all of them would tend to increase actual smallpox mortality over recorded mortality, for example, Lettsom's estimate of the true smallpox mortality in London would raise the figure for 1730–39 from 11.6 smallpox deaths per 100 births to over 20 per 100, this being in an area where smallpox mortality was at its lowest due to the endemic nature of the disease. Once again it is impossible to estimate exactly the magnitude of smallpox mortality, but for the time being it will be sufficient to note that recorded smallpox deaths accounted for between 11.6 and 50 per cent of all those born and dying, and that actual smallpox mortality was possibly twice as large as that actually recorded.

IV

The possible effectiveness of inoculation in reducing smallpox mortality has been rejected by previous historians on two basic grounds: (i) the argument that inoculation spread natural smallpox to the unprotected; (ii) the continuance of smallpox deaths in the bills of mortality of some of the large towns. There are several grounds for questioning the argument that inoculation spread natural smallpox: (a) smallpox was already a universal disease before the introduction of inoculation; (b) inoculation had become so widespread by the end of the eighteenth century that only a relatively small proportion of the population was left unprotected; (c) experimental and other evidence is available to show that inoculation

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[71] J. Marshall, *Mortality Of The Metropolis* (1832).

[72] J. Fleetwood, *History Of Medicine In Ireland* (1951), p. 65; Dr J. Rutty, *A Chronological History Of The Weather, And Of The Prevailing Diseases In Dublin* (1770).

[73] M. C. Buer, *Health, Wealth, And Population In The Early Days Of The Industrial Revolution* (1926), p. 190.

did not spread natural smallpox to the unprotected.[74] This conclusion is supported by the fact that early vaccination was in reality a more attenuated form of inoculation.[75]

Smallpox did continue to kill substantial numbers of children in some of the large towns during the late eighteenth century. This fact has misled medical historians for two reasons: (i) the total population increased very rapidly in these places, and if the number of smallpox deaths is expressed as a proportion of the number of children at risk a reduction in smallpox mortality is seen to have taken place; (ii) as we have already seen, these large towns were atypical, in that inoculation spread much later in them than elsewhere. This was stated quite explicitly by Howlett in 1781:

> It may be thought, at first sight, that the healthiness of London is more increased than that of country towns ... But it must be remembered that the diminished mortality in the latter appears to be chiefly owing to the salutary practice of inoculation; whereas in the former, for want of universality, it has hitherto been of little advantage ... In provincial towns and villages, as soon as this disorder makes its appearance, inoculation takes place amongst all ranks of people; the rich and poor, from either choice or necessity, almost instantly have recourse to it; and where two or three hundred used to be carried to their graves in the course of a few months, there are now perhaps not above 20 or 30.[76]

An illustration of this reduction of smallpox mortality is to be found at Maidstone in Kent.

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[74] (1994): I have changed my view on this subject. The very severe forms of inoculated smallpox were probably capable of spreading natural smallpox.

[75] It is impossible in this paper to document this very controversial statement. The subject is of sufficient importance to warrant a separate paper. Inoculators were able to produce a single vesicle at the site of injection identical to that of vaccination, through a process of attenuation. Inoculation was superior to vaccination in that it conferred life-long immunity against further attacks of smallpox owing to the larger amount of virus injected. (1994): See the next chapter of this book, and Peter Razzell, *Edward Jenner: The History Of A Medical Myth* (1977), for a further discussion of this subject.

[76] Rev. J. Howlett, *An Examination Of Dr Price's Essay On The Population Of England And Wales* (1782), p. 94.

Table 6. Smallpox Mortality at Maidstone, 1754–1801[77]

Period	*Smallpox Burials*	*All Burials*
1752–1763	252	1,703
1762–1771	76	1,426
1772–1781	60	1,549
1782–1791	91	1,676
1792–1801	2	2,068

A mass inoculation in Maidstone was conducted by Daniel Sutton in 1766, and its effects were described by Howlett in a pamphlet by him in 1782:

> Upon casting an eye over the annual lists of burials, we see that, before the modern improved method of inoculation was introduced, every 5 or 6 years the average number was almost doubled; and it was found upon enquiry, that at such intervals nearly the smallpox used to repeat its periodical visits ... in the short space of 30 years it deprived the town of between five and six hundred of its inhabitants; whereas in the 15 or 16 years that have elapsed since that general inoculation it has occasioned the deaths of only about 60. Ample and satisfactory evidence of the vast benefits the town has received from that salutary invention.[78]

Many other statistical sources could be cited to prove the effectiveness of inoculation,[79] the most detailed being for Boston, USA, during the

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[77] Taken from the Parish Register Of Maidstone, lodged in All Saints Church, Maidstone. Smallpox deaths disappeared from the register after 1797. This gradual decline of smallpox cannot be attributed to a decrease in the virulence of the disease, as all the evidence points to the opposite conclusion, i.e. an increase in its virulence, e.g. the case-fatality rates at the London Smallpox Hospital were as follows in 1746–63, 25%; 1775–99, 32%; 1836–56, 35%. See the *Royal Commission On Vaccination, 1st Report* (1889), p. 74, and the *Royal Commission On Vaccination, 3rd Report* (1890), p. 100.

[78] J. Howlett, *Observations On The Increased Population ... Of Maidstone* (1782), p. 8.

[79] For the sources of these statistics see: the parish registers of Basingstoke (Hants.), Calne (Wilts.), Milton Ernest (Beds.), Whittington (Salop.), Selattyn (Salop.), Boston (Lincs.). For other statistics see "An abridgement of the observations on the Bills of Mortality in Carlisle 1779–87" by Dr Heysham in W. Hutchinson, *The History Of Cumberland* (1794), pp. 668–75.

eighteenth century, from which it is possible to attribute the reduced mortality directly to inoculation.[80]

Contemporaries were very well aware of the effect of inoculation; for example, in *She Stoops To Conquer* written in 1773, Mrs Hardcastle says to Hastings: "I vow since Inoculation began, there is no such thing to be seen as a plain woman. So one must dress a little particular; or one may escape in the crowd." Arthur Young, in his essay on population in 1781, wrote:

> In several of these parishes where population had for some periods been rather on the decrease, a great change has taken place lately, and the last ten years are found to be in a rapid state of progression; as considerable drains of men have been made from almost every parish in the kingdom for the public service in that period, I should not have expected this result, and know nothing to which it can be owing, unless the prevalence of inoculation, which certainly has been attended with a very great effect.[81]

References to the effects of inoculation on mortality appear in the reports on agriculture made by local observers to the Board of Agriculture at the end of the eighteenth century; for example, Plymley writing on Shropshire observed in 1795: "I may further add, that since the year 1782, when these observations were made, the population of this parish has been increasing: most certainly inoculation for the Smallpox ... has been most essential to population throughout this kingdom".[82] John Holt of Lancashire wrote in 1795: "One reason, why persons in large manufacturies in Lancashire, do not frequently die in great numbers ... is that they have (in general) been inoculated in their infancy. Inoculation is the most effectual

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[80] The number of inoculations in this town increased from 287 in 1721 to 9,152 in 1792, which was the vast majority who had not had smallpox before. Smallpox mortality fell from 175 smallpox deaths per 1,000 living population in 1677–8 to 15 per 1,000 in 1792, and this was in spite of the fact that the virulence of the disease generally increased throughout the period. See J. Blake, *Public Health In The Town Of Boston (Mass.), 1630–1822* (1959), p. 244; H. R. Viets (ed.), *A Brief Rule To Guide The Common People Of New England* (1937), p. xxxv; *Royal Commission On Vaccination, 6th Report* (Parl. Pap. 1896, XLVII), p. 762. See the Table on this subject in the next chapter.

[81] A. Young, *Annals Of Agriculture*, VII (1786), p. 455.

[82] J. Plymley, *General View Of The Agriculture Of Shropshire* (1803) pp. 343, 344.

of all expedients for preserving the short lived race of men – many gentlemen pay for inoculation of the children of the poor in their own neighbourhood".[83] In 1796 a correspondent to the *Gentleman's Magazine*, observed:

> the increase of people within the last 25 years is visible to every observer. Inoculation is the mystic spell which has produced this wonder ... before that time it may be safely asserted, that the malady, added to the general laws of nature, did at least equipoise population. It is now 30 years since the Suttons and others under their instructions, had practised the art of inoculation upon half the kingdom and had reduced the chance of death to 1 in 2,000.[84]

Another gentleman observed later in 1803 that "one very great cause of increasing population may be ascribed to the success of inoculation for the Smallpox. One in four or five, or about 200 to 250 in a thousand, usually died of this loathsome disorder in the natural way of infection ... so that this saving of lives alone would account for our increasing number, without perplexing ourselves for any other cause".[85]

The claims made by some contemporaries made on the beneficial effects of inoculation on population growth require careful evaluation. Unfortunately there is virtually no reliable demographic data by which this can be done. An analysis of the 'county family' life tables suggests that a reduction of about 25 per cent in mortality among the younger age-groups could account for the whole increase in expectation of life between 1681–1730 and 1781–1830. The same conclusion probably applies to both the ducal families and the whole of the aristocracy. For the population as a whole there is no data sufficiently reliable to test the hypothesis directly. However, it is possible to construct a simple hypothetical model whose limits are defined by the small amount of reliable information available to us. In 1697 Gregory King constructed a 'life table' for Lichfield; Professor Glass has written that "it would appear that by taking Lichfield as a basis, King began with a collection of statistics which were probably not markedly untypical, and then adjusted more acceptably as an indication of

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[83] J. Holt, *General View Of The Agriculture Of Lancaster* (1795), p. 208. n. 2.
[84] *Gentleman's Magazine*, Vol. LXVI, 1 (1796), n. 112.
[85] *Gentleman's Magazine*, Vol. LXXIII, 1 (1803), p. 213.

national structure".[86] Using King's 'life table', it is possible to construct a hypothetical population reproduction model for our period.

Table 7. Female Population Reproduction, 1750–1855[87]

Numbers Surviving to the Following Ages (Years)	*Numbers Surviving in the Following Years*							
	1750	*1765*	*1780*	*1795*	*1810*	*1825*	*1840*	*1855*
0	1,000	1,071	1,237	1,468	1,762	2,116	2,538	3,045
15	620	680	793	952	1,138	1,366	1,640	1,967
30	450	480	559	659	798	956	1,146	1,376
45	315	325	357	422	498	603	722	866
60	190	190	196	215	255	300	364	435
75	50	50	50	52	57	67	79	85
90	0	0	0	0	0	0	0	0
Population Index	2,125	2,260	2,573	3,034	3,627	4,350	5,220	6,251

The above model was constructed on the following assumptions: (i) increase in the female population was proportionate to the increase in total population; this ignores the effects of the relationship between the number of males and females, for example, the proportion of married women who were widowed; (ii) of 1,000 female children born before 1750, the numbers surviving to various ages were the same as in King's 'life table'; (iii) the population was static before 1750, based on an age-specific birth-rate of 1 female child born for every 13.7625 women living between 15 and 45; (iv) the age-specific birth-rate remains constant throughout the whole period; (v) of every 1,000 born, lives were saved in the following manner:

[86] D. V. Glass, "Gregory King's estimate of the population of England and Wales, 1695", *Population Studies*, Vol. 3 (1949–50), p. 568.

[87] This population index is the sum of the average number of people living in each age period, i.e. I have not bothered to multiply by 15 throughout.

Ages	*Period*			
(Years)	*1750–65*	*1765–80*	*1780–95*	*1795–1810*
Under 15	60	60	20	15
15–30	30	30	5	5
30–45	10	10	5	0

In all, it is assumed that 250 lives were saved out of 1,000 born. According to our earlier estimates of population growth, it almost exactly trebled between 1750 and 1851. In our model it does not quite do this, but we assumed that population was static before 1750, whereas according to the earlier estimates it was increasing about 0.2 per cent per annum between 1700 and 1750. If an allowance is made for this pre-1750 growth, population in our model increases by 3.2 times between 1750 and 1851; the greater the allowance made for pre-1750 growth, the more the model population increase will exceed that as estimated.

The point of the model is not to describe exact changes in the population structure, but rather to estimate the magnitude of lives required to be saved in order to generate the rate of increase in estimated population. The assumptions are thought to be realistic because: (a) the crude birth-rate appears to have been very similar between the 1690s and the 1840s;[88] (b) the saving of life (250 out of 1,000 born) assumed is very similar to that which took place among the gentry and aristocracy.

In order for inoculation against smallpox to account for the whole of the population increase, smallpox mortality before inoculation must have been about 310 deaths per 1,000 born, for of the 250 lives saved of every 1,000 born in our model, about 45 would have died of other diseases during the same period, while smallpox accounted for about 1.5 per cent of deaths

[88] The birth-rate was estimated as 34.5 births per 1,000 living during the 1690s by Gregory King and 35.2 per 1,000 during 1841–5 by Professor Glass from civil registration returns. See G. King, "Natural and political observation 1696" in George Chalmers, *An Estimate Of The Comparative Strength Of Great Britain* (1804), p. 44; and D. V. Glass, "A note on the under-registration of births in Britain in the nineteenth century", *Population Studies*, Vol. 5 (1951), p. 85. Professor Glass has written about the basis of King's estimate: "the statistics collected were more comprehensive than any provided previously and, indeed, than any subsequent statistics prior to the establishment of the full mechanism of censuses and civil registration in the nineteenth century." See D. V. Glass, "Gregory King and the population of England and Wales", *Eugenics Review*, XXXVII (1946), p. 175.

of all born during 1838–40,[89] when civil registration was first introduced. It is impossible to state definitely that smallpox mortality before inoculation was as high as 310 deaths per 1,000 born, but we may conclude from our earlier discussion that this is certainly a plausible figure. It must be remembered that much of this saving of life would have been indirect, in so much as the elimination of smallpox attacks probably increased the expectation of life of those who did not die of the disease. Also the vulnerability of mothers and other young adult females to smallpox could have meant that the elimination of the disease led to an increase in the birth-rate; for example, at Basingstoke (Hants.) the average number of baptisms in the ten years before the smallpox epidemic in 1741 was 69.6, whereas in the following ten years it fell to 45.5 (a much greater fall than the average number of deaths and therefore presumably the population), which was possibly due to the fact that one-half of the smallpox deaths occurred among adults.[90]

Although it is not possible to analyse in any detail the history of other diseases, it is possible to draw some conclusions from bills of mortality. For example, in Northampton there was no major epidemic of any disease, other than smallpox, during the hundred-year period after 1736 when records were kept.[91] Smallpox epidemics occurred every seven years on average in Northampton before the introduction of inoculation; the listing of diseases and epidemics was very similar in a place like Maidstone; that is to say, recurrent severe smallpox epidemics were the only causes of sharp rises in mortality rates. This would indicate that the peaks in mortality found in many local studies were due to smallpox and that they disappeared only with the introduction of inoculation.

Ideally one would like to trace the history of all diseases in order to evaluate their importance in contributing to total mortality, but unlike smallpox, most other diseases prevalent in the eighteenth century are not

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[89] See Creighton, *op. cit.* This figure includes chickenpox deaths, which are assumed to approximate to omissions due to fulminating smallpox, etc.

[90] See the Basingstoke Parish Register. A rise in the age-specific birthrate was not allowed for in the population reproduction model for two reasons: (i) simplicity and economy; (ii) the very long-term stability of the estimated crude birth-rate. Thus any increase in the birth-rate has been absorbed for analytical reasons into a fall in the death-rate.

[91] See the Northampton Bills Of Mortality in the British Library.

sufficiently distinctive to be analysed statistically. In their returns to Sir John Sinclair for the *Statistical Account of Scotland*, many incumbents discussed the history of diseases in their parish. No disease, other than smallpox (due to inoculation), was described as having declined or disappeared, except ague, which is very frequently mentioned as having diminished during the latter half of the eighteenth century. Recently one medical authority has questioned whether malaria was ever endemic in Britain.[92] However, the incumbents so consistently mention that the disappearance of ague was linked with the draining of marshes and the reclamation of swamp-land, that one is led to suspect that the disease they described was malaria, a suspicion confirmed by their descriptions of the disease. Buèr, in her discussion of malaria, maintained that although "its direct effect on the death-rate was small, its indirect effect must have been great".[93] Certainly it rarely appeared in the bills of mortality and parish registers as a cause of death even during the early eighteenth century. Malaria in England is a subject which warrants further investigation.[94]

Although this paper stressed the importance of inoculation against smallpox as a cause of population growth during the eighteenth century, this does not rule out the role of other factors and explanations.[95] However, other explanations currently lack evidence in their favour. Inoculation against smallpox could theoretically explain the whole of the increase in population, and until other explanations are convincingly documented, it is an explanation which must stand as the best available.[96]

Although the industrial and agricultural revolutions probably did not bring about the increase in population in the eighteenth century, they did at least enable population to grow unchecked. In Ireland, where such revolutions did not take place, the Malthusian check of mass starvation was the result of a rapidly increasing population without concomitant changes in the structure of the economy. The main achievement of the

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[92] McKeown and Brown, *op. cit.*, p. 124, n. 4.

[93] M. C. Buer, *op. cit.*, p. 212.

[94] (1994): Since this was written, Mary Dobson has embarked on a major study of malaria in the marshland area of south-east England.

[95] For example, the effects of the changing distribution of population between rural and urban areas.

[96] (1994): Recent evidence has come to light – considered in later chapters – which suggests that a fall in mortality may have predated the introduction of inoculation.

industrial and agricultural revolutions in their earlier phases was the maintenance of the standard of living in a period when population was growing for reasons largely unconnected with the revolutions themselves.

APPENDIX

In order to indicate the extent of mass inoculations, a sample was taken of those references to them in local histories, medical commentaries, accounts of the Overseers of the Poor, local newspapers, and other sources. It is in no sense comprehensive or representative, but merely a series of isolated examples culled from the literature, mainly from the South of England. The name of the town is given first, followed by the date of the mass inoculation:

Guildford, Surrey, 1740s. Salisbury, Wilts., 1751–2. Bradford-on-Avon, Wilts., 1752–3. Blandford, Dorset, 1753, 1766. Wootton-under-Edge, Glos., 1756. First Regiment of Foot Guards, 1756. Beaminster, Dorset, 1758, 1780, 1791. Maldon, Essex, 1764. Maidstone, Kent, 1766. Marnham, Notts., 1767. Rye, Sussex, 1767. Neighbourhood of Norwich, 1769. Burton, Lincs., 1770. Berkhamstead and surrounding villages in Herts., 1770. Corsley, Wilts., 1773; Meopham, Kent, 1776. Bedford, Beds., 1777. Ware, Herts., 1777. Great Clivall, Essex, 1778. Irthlingborough, Northants., 1778. Villages in the neighbourhood of Carlisle, Cumberland, 1779, 1781. Cricklade, Wilts., 1783. Painswick, Glos., 1786. Knowle, Kent, 1787. Weston, 1788. Northwold, Norfolk, 1788. Cowden, Kent, 1788. Luton, Beds., 1788. Bozeat, Northants., 1789. Chislehurst, Kent, 1790, 1799. Toddington, Beds., 1790, 1801, 1824. Weston, Norfolk, 1791. Eaton Socon, Beds., 1793, 1800, 1808. Hevingham, Norfolk, 1794. Berkeley, Glos., 1795. Hastings, Sussex, 1796–7. Dursley, Glos., 1797. Three villages near Gillingham, 1797. Tenterden, Kent, 1798. Rayne, Essex, 1806. Chichester, Sussex, 1806, 1812, 1821.

Under Dimsdale's influence, mass inoculations increasingly became 'general' rather than 'partial'.[97] General inoculations usually involved a degree of compulsion, as was described by Cowper, the poet, in 1788:

[97] See, for example, T. Dimsdale, *Remarks On 'A Letter to Sir R. Barner ...'* (1779), p. 13; and Walker, *op. cit.*, p. 467, n.

"The smallpox has done, I believe, all that it has to do at Weston. Old folks, and even women with child, have been inoculated. ... No circumstances whatsoever were permitted to exempt the inhabitants of Weston. The old, as well as the young, and the pregnant, as well as they who had only themselves within them, have been inoculated ..."[98] An example of the effects of general inoculation is to be found at Calne, Wilts. A local surgeon, Mr Wayte, described in 1795 a general inoculation as follows: "In September, 1793, when the poor of the parish were inoculated. ... We inoculated six hundred and upwards ... Besides the poor, I inoculated about two hundred (private) patients.... Now in inoculating a whole parish, we have no choice of patients, all ages, and the sickly as well as others, were inoculated; but these were mostly children, as I assisted in inoculating the whole parish, about twelve or thirteen years ago."[99] According to the Calne parish register, the number of smallpox deaths declined as follows: 1723–42: 205; 1743–62: 122; 1763–82:54; 1783–1802: 8. The last mention of smallpox deaths is in 1793 when there were 6; previous to this there had been a minor epidemic in 1782 involving 10 deaths (this was the epidemic which provoked the earlier general inoculation mentioned by Wayte). These late eighteenth-century epidemics should be compared with the major ones in the early eighteenth century, for example, in 1732 there were 173 people in Calne registered as having died from smallpox.

[98] S. & B. Webb, *op. cit.*, p. 306, n. 2.

[99] Thomas Beddoes, "Queries respecting a safer method of performing inoculation", in Don A. De Gimbernat (Beddoes translated), *A New Method Of Operating For The General Hernia* (1795), pp. 56–9.

Chapter 2

Edward Jenner: The History of a Medical Myth[1]

This article derived from research on the history of smallpox inoculation (variolation). I argued in this essay that Jenner's vaccines were derived from smallpox virus, and that early vaccination was a more attenuated form of inoculation.

I believed that traditional inoculation did not spread natural smallpox, but I have since modified my view on this subject. I revised my view on the basis of evidence on the use of Jenner's vaccine in the United States. One of Jenner's vaccine threads was sent in 1800 to Benjamin Waterhouse, who practised in the Boston area. This appears to have produced a severe reaction, with all the classical symptoms of a heavy case of inoculated smallpox. Subsequently, the person "vaccinated" appears to have communicated natural smallpox through respiratory infection to other people. Eventually, Jenner's vaccine became attenuated by taking virus from a previous site of injection through arm-to-arm transmission. A full treatment of this subject is dealt with in my book, ***Edward Jenner: The History Of A Medical Myth.***

Note by the Editor of *Medical History*, Dr F. N. L. Poynter

The provocative title of Mr Razzell's article will doubtless shock many readers, but it is the duty of the historian to take nothing for granted and to put to the question periodically the major assumptions of history, just as it is an editor's duty to give space to iconoclasts as well as to idolists.

[1] First published in *Medical History*, Vol. 10, No. 3, July 1965.

The following article is frankly controversial and the editor considered its implications so important, both for medical history and current practice, that he has invited Professor A. W. Downie, M.D., F.R.S., of Liverpool University, an acknowledged authority in this field, to comment on Mr Razzell's arguments. The latter has claimed the right to reply to Professor Downie's criticisms and both comment and reply will be found at the end of the article. Discussion is now open to readers and any further discussion, by Professor Downie or others, will be published in forthcoming issues of *Medical History*. The editor confines himself to remarking that the October issue will contain an interesting account of smallpox in Ethiopia which may be read as an implicit refutation of Mr Razzell's case. Despite the long-continued use of inoculation in this close community, epidemics of smallpox raged until Jennerian vaccination was introduced in the nineteenth century. If Mr Razzell's article and the ensuing debate prove nothing else we are given a lively demonstration that medical history is by no means a dead subject but is concerned with issues which are very much alive.

F.N.L.P

The main aim of this paper is to argue that early vaccination was a more attenuated form of the eighteenth-century practice of inoculation.[2] In a paper on eighteenth-century population change,[3] I have argued that inoculation was effective in gradually eliminating natural smallpox, well before the advent of vaccination at the beginning of the nineteenth century. It is not possible to present the full evidence for this conclusion here, but selected statistics will serve to illustrate the nature of the hypothesis.[4]

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[2] Throughout this paper inoculation is used to mean variolation (except where stated), as this was the term used by eighteenth-century contemporaries, some of whose writings we shall be considering.

[3] See Chapter 1.

[4] None of the figures in this paper ought to be taken too literally, as there are many problems with regard to the classification of disease. However, smallpox is a sufficiently distinctive disease to enable us to use these figures as indications of trends.

Table 1. Smallpox Mortality from Epidemics in Boston, Mass., USA, in the Eighteenth Century[5]

	1677–8	*1702*	*1721*	*1730*	*1752*	*1764*	*1776*	*1788*	*1792*
Population	4,000	6,750	10,700	13,500	15,684	15,500	—	—	19,300
Natural Smallpox									
Cases			5,759	3,600	5,545	699	304	122	232
Smallpox Deaths	700	213	842	500	539	124	29	40	69
Deaths per 1,000 cases			146	139	97	177	95	328	298
Inoculated Smallpox									
Cases			287	400	2,124	4,977	4,988	2,121	9,152
Deaths			6	12	30	46	28	19	179
Deaths per 1,000 cases			21	30	14	9	6	9	20
Total Smallpox Deaths	700	213	848	512	569	170	57	59	284
Smallpox Deaths per 1,000 population	175	32	79	37	36	11	—	—	15
Left the town					1,843	1,537			262
Escaped disease in town					174	519			221
Had smallpox before					5,998	8,200			10,300

Three important conclusions are to be derived from this table. First, that the smallpox death rate was reduced from 175 smallpox deaths per 1,000 living in 1677–8 to 15 per 1,000 by 1792. Second, this was achieved in spite of an increase in the virulence of the disease. Third, the reduced mortality may be directly attributed to inoculation, which protected the vast majority of the vulnerable population by the end of the eighteenth

[5] J. Blake, *Public Health In The Town Of Boston (Mass.), 1630–1822* (1959), p. 244. *Royal Commission On Vaccination, 6th Report* (Parl. Papers 1896/47), p. 762. H. R. Viets (ed.), *A Brief Rule To Guide The Common People Of New England* (1937), p. 35. The figures in this table do not balance, as some people inoculated were not inhabitants of the town, and were therefore not included in the total population.

century. An example of the effects of inoculation on smallpox mortality in England is to be found in eighteenth-century Maidstone.

Table 2. Smallpox Mortality at Maidstone, Kent, 1752–1801[6]

Period	*Smallpox burials*	*All burials*
1752–1761	252	1,703
1762–1771	76	1,426
1772–1781	60	1,549
1782–1791	91	1,676
1792–1801	2	2,068

A mass inoculation was conducted by Daniel Sutton in 1766 and its effects were described by Howlett in 1782:

> Upon casting an eye over the annual list of burials we see that, before the modern improved method of inoculation was introduced, every five or six years the average number was almost doubled; and it was found upon enquiry, that at such intervals nearby the smallpox used to repeat its dreadful periodical visits ... in the short space of thirty years it deprived the town of between 500 and 600 of its inhabitants; whereas in the fifteen or sixteen years that have elapsed since that general inoculation it has occasioned the deaths of only about sixty.[7]

The main reason why most historians thought that inoculation had been ineffective against smallpox was the set of smallpox mortality statistics for London. These were faulty in several ways,[8] but must be reinterpreted in the light of the fact that inoculation was utilized on a large scale much later in London than in the rest of the country, especially outside large towns.[9] Howlett stated this quite explicitly in 1781:

> It may be thought, at first sight, that the healthiness of London is more increased than that of country towns. ... But it must be remembered that the diminished mortality in the latter appears to be chiefly owing to the salutary practice of inoculation, whereas in the

[6] Figures compiled from the Maidstone Parish Register.

[7] J. Howlett, Observations On The Increased Population ... Of Maidstone, (1782), p. 8.

[8] For example, no account is taken of the increased number of births.

[9] See Chapter 1.

> former, for want of universality, it has hitherto been of little advantage. ... In provincial towns and villages, so soon as this disease [smallpox] makes its appearance, inoculation takes place amongst all ranks of people; the rich and the poor, from either choice or necessity, almost instantly have recourse to it; and where 200 or 300 used to be carried to their graves in the course of a few months, there are now perhaps not above twenty or thirty.[10]

It is in the light of these findings that we must re-examine the relationship between inoculation and vaccination. One aspect of the conventional medical view of the relationship is that inoculation differs from vaccination inasmuch as it gives rise to pustular eruptions other than at the site of injection and is consequently a source of infection to an unprotected population.[11] There is contemporary eighteenth-century evidence to suggest, however, that this was not always the case. None of the hundreds of incumbents making returns in the *Statistical Account Of Scotland* at the end of the eighteenth century, specifies a case of inoculation spreading smallpox, in spite of the partial inoculations of the gentry and farmers in some parishes.[12] According to a letter sent from the Council of Geneva in 1791:

> An epidemic of smallpox is of almost regular occurrence every five years, and between the epidemics it frequently happens that we have no natural smallpox whatever, little in the city or its vicinity. Inoculation began to be practised here in 1751, since which date we have inoculated a very large number of children annually, and with such marked success that the deaths have not exceeded 1 in 300. Although we have often had to inoculate with pus brought from a distance at times when there was no smallpox to be found in the city, and although children so inoculated have gone freely into the streets,

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[10] J. Howlett, *An Examination Of Dr Price's Essay On The Population Of England And Wales* (1781), p. 94.

[11] The traditional medical view of the relationship is that inoculation uses smallpox virus, whereas vaccination uses cowpox virus.

[12] See Sir J. Sinclair (ed.), *The Statistical Account Of Scotland*, 21 Vols. (1791–99). (1994): Since I wrote the above in 1965, I have modified my view: I now believe that in very severe cases of inoculated smallpox, inoculation did on occasions spread the respiratory form of the disease.

> walks, and other public places, before, during, and after the eruption, we have never observed that they were sources of contagion, nor that they produced any intermediate epidemics, nor that they accelerated the return of the periodical epidemic.[13]

An almost identical description was sent from the Hague:

> The 200 persons who were inoculated at the Hague, about the end of the year 1768, without much regard to themselves or others, frequented all places of public resort; notwithstanding which no epidemic was produced, nor in the whole year did more than eight persons die of the smallpox, and of these three died in the spring, one by inoculation, and two by the natural disease, which they had caught at some other place and carried with them to the Hague, and the remaining five died towards the end of the year.[14]

There were similar experiences noted at Chester[15] and at Ware, Herts.,[16] and many inoculators were well aware that their patients were not a source of contagion. The most convincing evidence of the relative non-contagiousness of inoculation is provided by a series of experiments conducted during the late eighteenth century by Dr O'Ryan, Professor of Medicine at the College of Lyons, France, part of which he described as follows:

> I placed a person in the eruptive fever of the smallpox by inoculation at the distance of about half a yard from four children properly prepared; each exposure continued one hour, and was repeated daily for a fortnight, reckoning from the commencement of the fever till the pustules were become perfectly dry: not one of the four received the infection. Two months afterwards, I inoculated three of these children; they had the distemper in a very mild manner and recovered without difficulty.[17]

O'Ryan concluded from his experiments

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[13] J. Haygarth, *A Sketch Of A Plan To Exterminate The Casual Smallpox* (1793), pp. 472–5.

[14] *Gentleman's Magazine*, Vol. 47 (1777), p. 224.

[15] J. Haygarth, *An Inquiry How To Prevent The Smallpox* (1785), p. 588.

[16] J. C. Lettsom, *A Letter ... Upon General Inoculation* (1785), p. 11.

[17] J. Haygarth, *A Sketch Of A Plan ...*, pp. 82, 83.

> that there is no risk of contracting it [smallpox], provided the person who is liable to the infection, keeps himself at a very little distance from patients in the smallpox, or from things which they have touched.[18]

Although we now know this view to be erroneous, we must still explain the results of his experiments. A clue to the answer to our problem is to be found in Dixon's recent text on smallpox. In discussing the infectivity of scab virus he writes: "... in practice scab virus seems to lack epidemic potential. I have suggested (Dixon 1948) that the virus extruded through the skin, perhaps modified by its passage, is in some way different from the virus from the respiratory tract."[19] Logically, the opposite also applies, that is to say, the virus injected through the skin is also modified in some fundamental way. Therefore an inoculated person, would be less infectious, as all the smallpox viruses in his body would have derived from a stock of modified virus extruded through the skin of another person's body (the person from whom the virus was originally taken) and then passed through his own skin. As the degree of infectivity of smallpox is probably connected with the degree of severity of the disease,[20] we would expect the transmission of the virus through the skin to produce milder forms of smallpox. This is in fact what happened, as all the inoculators well knew. Mowbray, Gatti and the Suttons all produced much milder and safer results from inoculation by arm-to-arm transmission. Gatti ran into difficulty over his inoculation in 1765 of the Duchess de Bouffle, who had no pustular eruption except at the site of inoculation and suffered an attack of natural smallpox two-and-a-half years later,[21] a problem which would occupy the vaccinators forty years later. Gatti appears to have achieved these very mild results by taking the smallpox virus for his inoculations from the site of a previous inoculation, rather than from one of the pustular eruptions around the body.[22]

Fortunately, we have some experimental evidence on the degree to which smallpox virus can be attenuated. In 1777 John Mudge, a Plymouth surgeon, reported the following experiment:

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[18] *Ibid*, pp. 78, 79.

[19] C. W. Dixon, *Smallpox* (1962), p. 298.

[20] *Ibid*, p. 298.

[21] C. Creighton, *The History Of Epidemics* (1894), Vol. 2, pp. 495, 496.

[22] A. Gatti, *New Observations On Inoculation* (1768).

> Messrs. Longworthy and Arscott, surgeons, in the spring of 1776, inoculated at Plymton, a neighbouring town, forty patients; of which number, thirty were injected with crude matter from the arm of a young woman [from the site inoculation before the eruption of pustules], five days after she herself had been inoculated with concocted matter [from a pustule around the body], which eventually did produce in her a pretty smart fever, and a sufficient number of eruptions. The other ten were inoculated with matter of another kind, which I procured, in a concocted state, from a pustule of the natural smallpox. The arm of all the forty patients took the injection; and the latter ten, after the eruptive fever, had the smallpox in the usual way. Of the other thirty, though the injection took place on their arms, so as to inflame them considerably, and to produce a very large prominent pustule, with matter on it, on each of them, yet not one of them had any eruptive fever, or a single subsequent eruption, on any part of the body. ... It is to be remarked too that the matter which was in those pustules having been used to inoculate others produced on them exactly the same appearances, unattended also with either fever or smallpox.[23]

In other words it was possible to attenuate the smallpox virus to such an extent that only a single pustule was produced at the site of inoculation and this was achieved by taking the virus from the site of a previous inoculation. Adams repeated the same experiment at the beginning of the nineteenth century and was able to produce a whole series of cases in which there was only an eruption at the site of inoculation. He compared the latter with typical vaccine vesicles and claimed that they were identical.[24] This was a conclusion confirmed by Guillou, who was also able to produce a typical vaccine vesicle at the site of inoculation.[25] Dr John Walker, Director of the Royal Jennerian Society, wrote to Lettsom in 1813:

> I have, from the first introduction of vaccination, after having observed its symptoms and progress, entertained an opinion respecting its native difference from those who suppose it a substitute only for

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[23] J. Mudge, *A Dissertation On The Inoculated Smallpox,* (1777), pp. 20–22.

[24] See *Royal Commission On Vaccination, 4th Report* (Parl. Papers, 1890–91/44), p. 52.

[25] *Ibid,* p.53.

> the [inoculated] smallpox.... Now I have from an early part of my practice, been in the habit of diluting the smallpox virus with water, previous to its introduction into the system, and in every instance I have then always found the disease mild, and the fever slight: this led me to the conclusion above hinted at.... I believe the variola and vaccine (so called) to be, at bottom, the same disease, and could wish that the term variola mitior were employed instead.[26]

Walker was using smallpox virus as the source of his 'vaccine' and as Creighton observed, "the very Director of the Jennerian Institute was among the prophets of the old inoculation."[27] However, from our present point of view what is significant is that Walker was able to produce the single local vesicle typical of vaccination, through a process of attenuating smallpox virus.

It is in the light of these neglected facts that we must reinterpret the history of vaccination itself. After a few initial experiments with cowpox in 1796 and 1798, Jenner's original vaccine lymph was lost, and it was not until the end of January 1799, when cowpox was discovered in Gray's Inn Lane by Woodville, that experiments were resumed. Woodville immediately sent Jenner some lymph to check its suitability. With this lymph, Jenner operated on twenty persons and reported to Woodville: "Berkely, February 1799. The rise, progress, and termination of the pustules created by the virus were exactly that of the true cowpox".[28] Woodville was completely confused about the relationship between vaccination and inoculation, and later wrote: "The virus which Dr Jenner declared to be perfectly pure and genuine was taken from the arm of a [smallpox] hospital patient who had 310 pustules, all of which suppurated".[29] Woodville, who was a doctor at the London Smallpox Hospital, had found that a majority of 500 people vaccinated by him had pustular eruptions similar to those that took place during inoculation. The conventional medical explanation of this is that repeated recently by Dixon:

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[26] T. J. Pettigrew, *Memoirs Of The ... Late John Coakley Lettsom* (1817), Vol. 3, pp. 350, 351.

[27] Creighton, *op. cit.*, p. 590.

[28] Whilliam White, *The Story Of The Great Delusion* (1885), p. 147.

[29] *Ibid*, p. 149.

> Unfortunately Woodville vaccinated his cases at the Smallpox Hospital, and at least two-thirds of them showed some general eruptions. It is almost certain, that under these circumstances the patients were either inoculated [injected] with a mixture of vaccine and variola virus from contaminated lancets, were vaccinated and naturally infected with smallpox at the same time, or, in some cases, were vaccinated and then variolated from three to five days later, when they again had a double infection.[30]

This interpretation neglects a considerable body of evidence to the contrary, particularly that supplied by Jenner himself. At the beginning of 1800 he wrote a letter to Lord Egremont, one of his patrons, who had complained that some of the vaccine sent from London had produced pustular eruptions when used on his family at Petworth. Jenner wrote:

> In many places where the [vaccine] threads were sent, a disease like a mild smallpox frequently appeared; yet, curious to relate, the matter, after it had been used six or seven months, gave up the variolous character entirely, and assumed the vaccine; the pustules declined more and more, and at length became extinct. I made some experiments myself with this matter, and saw a few pustules on my first patients; but in my subsequent inoculations [vaccinations] there were none.[31]

It is quite clear from this letter that the conventional medical explanation (e.g. Dixon's) of the pustular eruptions in Woodville's cases of vaccination is incorrect, for pustular eruptions occurred outside the London Smallpox Hospital where contaminated lancets, mixed injections or natural smallpox cannot be invoked as explanations (this is particularly true of Jenner's own cases). These pustular eruptions gradually disappeared as the new vaccine was transmitted from arm-to-arm, using the site of a previous inoculation. Thus Jenner's vaccine was probably smallpox virus, which was attenuated in a manner already familiar to some of the inoculators. The vaccinators were producing results similar to those produced by Arscott and Longworthy, Gatti, Adams and Walker, through taking smallpox virus from sites of previous inoculations and transmitting

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[30] Dixon, *op. cit.*, pp. 119, 120.

[31] J. Baron, *Life Of Dr Edward Jenner* (1827), vol. 1, pp. 314, 342.

it from arm-to-arm; the only difference being that they thought that they had discovered a new process. Arscott, Longworthy and Mudge had rejected this attenuated technique which produced only a local pustule, as they felt it would give insufficient protection against future attacks of smallpox (in this they were right) and it was left to the vaccinators to utilize unknowingly the same technique twenty-four years later. However, we must still try to explain what the relationship is between the cowpox and smallpox viruses. Unfortunately, the virologists do not seem to be in a position to settle this problem and it is not even agreed whether the one virus is autonomous of the other.[32] According to one authority:

> At the present day the general opinion agrees with that held by Jenner, that cowpox is simply smallpox much modified by passage through the cow. It might be supposed that this fact would be one easy of demonstration, and cows have by many observers, e.g. Woodville in 1799, by Ceely, by Badcock, and by Thiele of Kazan in 1838, been experimentally inoculated with smallpox but in most cases the disease, when thus artificially produced in cows, appears to retain a considerable degree of virulence, and to produce general though slight symptoms when again communicated to human beings, instead of the purely local symptoms of ordinary vaccinia.[33]

Copeman attempted to explore the relationship between smallpox and cowpox experimentally:

> He first inoculated a monkey with smallpox virus and then inoculated a calf from such an infected monkey. This resulted in typical vaccine, from which good strains of vaccine lymph were obtained. On the basis of this experience Copeman suggested that cowpox may actually have originated in the eighteenth century from inoculated smallpox, as the local sore produced by the inoculated incision frequently was very itchy, and milkers who scratched their arms may easily have conveyed infectious matter to the cow's udder.[34]

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[32] Dixon, *op. cit.*, pp. 119, 120, 163.

[33] W. A. R. Thomson, *Black's Medical Dictionary,* 1963, p. 942.

[34] G. Miller, *The Adoption Of Inoculation ... In England and France,* (1957), pp. 19, 20.

From our point of view the transmission of the smallpox virus through a cow or any other non-human animal, is an irrelevance, inasmuch as smallpox inoculation can be attenuated into vaccination merely by arm-to-arm transmission of the virus, using the previous sites of inoculation. This hypothesis is the only one to explain the manifold contradictions contained in all the evidence. This includes the phenomena of generalized vaccinia which on the present hypothesis is nothing other than what eighteenth-century contemporaries would have considered a typical inoculation. It would also explain why "although vaccinia and cowpox have common features of wide host range, serologically variola is more closely related to vaccinia".[35]

Does this conclusion mean that the reputation of Jenner is undeserved? He, who had been inoculated in the old method as a boy during the mass inoculation at Wootton-under-Edge in 1756, was an inoculator using the Suttonian method before he claimed to have discovered vaccination.[36] The only advantage the latter had over the more traditional methods of inoculation was that it appeared to cause fewer direct deaths. The problem in evaluating this claim is that many deaths were attributed to inoculation, which were probably due to the fact that many people had caught smallpox before being inoculated. Thus for example, in Boston, Mass., inoculation was forbidden by law and was only allowed when the presence of an epidemic created such panic as to make it inevitable. As several thousand people were inoculated, some of them would have caught smallpox before being inoculated, and their subsequent deaths would be incorrectly attributed to the inoculation. In more controlled conditions the death rate from the mild Suttonian method of inoculation was virtually nil. The Sutton's claimed in 1768 "that about 55,000 had been inoculated by them since the year 1760, of which number, six only had died".[37] Among the 5,694 people inoculated at the London Smallpox Hospital during the years 1797–9 there were only nine deaths. By the beginning of the nineteenth century the inoculators had attenuated their viruses sufficiently to be able to eliminate the risk of death altogether; for example Dr Forbes, a supporter of vaccination and an opponent of inoculation, had to report that of the 2,500 people

[35] Dixon, *op. cit.*, p. 163.

[36] J. J. Abraham, *Lettsom* (1933), p. 192.

[37] R. Houlton, *Indisputable Facts, Relative To The Suttonian Art Of Inoculation* (1768), p. 10.

inoculated in the Chichester area in 1821 not one died.[38] Inoculation had the advantage over the more attenuated vaccination of conferring a much longer period of immunity against future attacks of smallpox, and this was of course because of the larger numbers of antibodies produced. This much greater period of immunity was no mean advantage at a time when smallpox was such a constant threat.

Generally we must conclude that Edward Jenner's contribution to the history of medical innovation has been greatly over-estimated, and at most he was one of many innovators in the technique of inoculation against smallpox.

Comment by Professor A. W. Downie, M.D., F.R.S.

I have read through this paper carefully and it appears to me that the author has been very selective in quoting sources to uphold his thesis.

In his general proposition that the reduction in the smallpox death rate between 1677 and 1792 was due to smallpox inoculation, he has ignored the importance of other factors. It is true that the figures from Boston (Table 1) would appear to lend some support to his thesis, but he ignores the fact that in Boston very strict quarantine regulations were enforced to prevent the introduction of smallpox into that City. Isolation of cases when they occurred was strictly enforced. This and the quarantine regulations introduced to prevent the importation of smallpox into the town, were probably more important measures than inoculation in determining the diminution in incidence of the disease over the period covered in Table 1.

The author appears to believe that by the end of the eighteenth century inoculation of smallpox was very widely and generally applied. This would seem very far from being the case. (Up to 1764 only 5,554 persons in the whole of Scotland had been inoculated with smallpox according to Alexander Monro Senior.) It is obvious from Haygarth's correspondence published in his *Sketch* (1793) and in the letter to Percival of Manchester, that after the first few years of the introduction of inoculation against smallpox in Chester, the poor people in the town would not avail themselves of this measure. Indeed, he regrets that no-one had come forward

[38] Dr J. Forbes, "Some account of the smallpox lately prevalent in Chichester and its vicinity", *London Medical Repository*, (1822), pp. 211–15.

at all for inoculation and that the poor preferred to acquire the disease in the natural way. With reference to the diminution of smallpox as a result of inoculation towards the end of the eighteenth century, he states that in 1774 only $\frac{1}{14}$th of the population of Chester had not suffered from the disease. This was at the time when inoculation of the smallpox was not available to the poorer people in the town. Similar observations were made in Leeds and Newcastle. So much for the author's suggestions that inoculation had greatly lessened the ravages of smallpox by the end of the eighteenth century! It seems much more likely that the diminished mortality from the disease at this time was due to the recognition of the infectious nature of the disease and measures of isolation being introduced to prevent its spread, such as the provision of isolation wards in hospitals and isolation of patients at home, together with improved housing and nutrition of the poor.

The author also quotes reports apparently showing that the inoculated disease was not infectious. This, however, is not supported by evidence from other sources. Maitland recorded in 1722 that the little girl, Mary Batt aged two years, who was inoculated by himself, infected six domestic servants with typical smallpox from which one of them died. It is also apparent from Haygarth's *Sketch* (1793) that the disease was frequently spread from inoculated to susceptible persons. Indeed, it was an essential part of Haygarth's plan that those inoculated with smallpox must isolate themselves at home to avoid spreading infection. He quotes several instances where such spread did in fact occur. The author of the manuscript has made selections from the letters published in Vol. 2 of the *Sketch*, choosing those purporting to show that the inoculated disease was not infectious. He has ignored other letters in the same volume which provide evidence of spread of infection from inoculated persons. He also ignores the fact that even the casual smallpox is not as highly infectious as many people think – a point also stressed in all Haygarth's writings.

There is evidence from the observations of the Suttons, Dimsdale and others, that the introduction of the Suttonian technique, of taking material from the site of inoculation of the smallpox after four or five days for further inoculations, produced a milder type of inoculation smallpox than had previous practice. When this technique was followed the mortality from the inoculated disease became much less than in the earlier years (1721–30), but even at the end of the eighteenth century most authorities agreed that the mortality from inoculated smallpox was still of the order of $\frac{1}{200}$ to $\frac{1}{500}$.

The author's main contention that the vaccinia virus now employed has been derived from smallpox virus attenuated by repeated passage through arm vaccination, may be true, but proof of this is not available at the present time. The strains at present in use for vaccination have been so long passed in laboratory animals that the history of their origin is uncertain. It has, however, I think been established from Jenner's experiments and those carried out with fresh stocks of cowpox in 1799, that cowpox infection did protect against smallpox. It is true, as Dixon has maintained, that Woodville's experiments were unreliable in that his inoculations of cowpox were carried out in a smallpox hospital and many of the subjects were subsequently tested by variolation a few days later. These two facts made it very difficult to be sure that Woodville's observations had much bearing on the value of cowpox virus as an immunizing agent. It is, however, also clear from the observations of Ceely, published from 1839 onwards, that inoculation of genuine cowpox virus would protect against smallpox. Ceely gave very clear descriptions of the effects of inoculating cowpox virus on humans and, indeed, isolated fresh stocks of virus from the natural disease in cows or persons infected from them. In my opinion, Dixon's comments on Woodville's work are quite justified.

The author mentions the adaptation of smallpox virus to propagation in the cow. Many such observations of this kind were recorded in the nineteenth century but they are all of doubtful value because cowpox was sometimes inoculated on the same animals and the later experiments were carried out (e.g. Copeman's) with variola virus in institutes where strains of vaccinia were also in use. French workers showed many years ago that vaccinia virus spread very readily amongst cows and suggested that many of the reported successful inoculations with variola in cows were, in fact, cross infection of the animals with strains of vaccinia in use in the same establishments. All recent attempts (in the last twenty-five years) to infect cows with smallpox virus and to pass the virus to successive animals, have failed, even when the monkey has been used as an intermediate host. (Our own attempts to convert variola to vaccinia by inoculation of animals have been completely unsuccessful.) (See also Herrlich *et al. Arch. ges. Virusforschung,* 1963, 12, 579.)

We have no doubt that cowpox is a natural disease of cattle and is not derived from variola. We have isolated at least a dozen strains of cowpox virus from the natural disease of cattle or from lesions on the hands of those working with infected animals. All these strains of virus are quite

different from strains of variola virus and also from current strains of vaccinia virus. However, these strains of cowpox virus are immunologically practically identical with vaccinia virus and with variola virus. Immunisation of animals with cowpox virus produces antibody which is apparently effective against variola and vaccinia viruses. Our cowpox strains have the same host range as vaccinia strains and can be readily passed on to a variety of laboratory animals. This feature is not shown by a number of strains of variola virus which we have tested in this way.

I apologize for writing to you at such length, but I cannot agree with much of the argument in the enclosed manuscript. The author has selected to support his thesis only such evidence as would suit his purposes and has neglected many other works which would appear to refute his arguments.

Mr Razzell's reply

I will deal with Professor Downie's points in the order that they were raised. He writes: "This [the isolation of smallpox cases] and the quarantine regulations taken to prevent the importation of smallpox into the town, were probably more important measures than inoculation in determining the diminution in incidence of the disease over the period covered by Table 1". Yet if you look at Table 1 you will see that the numbers escaping the disease in and out of town amount to only 483 people out of a total population of 19,300 in 1792. Table 1 unequivocally demonstrates that the diminution in the number of smallpox deaths may be directly attributed to the effects of inoculation.

The second point raised concerns evidence for the hypothesis that inoculation did spread natural smallpox to an unprotected population. Maitland's example of an inoculated two-year-old girl spreading the disease to six domestic servants is cited. This incident occurred in 1722 when the English inoculators invariably used natural cases of smallpox as the source of their virus. As Dixon has written:

> In spite of the warnings in the earlier writings of the desirability of sending someone else to collect the smallpox matter so as to avoid infecting the inoculated person simultaneously with the natural disease (from respiratory virus on clothing, or in other ways from an infectious patient), it seems clear that Armyand as well as Maitland did not realize the effect of inoculating simultaneously with, or after

> contact with, natural smallpox in confusing the statistics of inoculation.[39]

The standard practice of later inoculators was to take smallpox matter from previously inoculated cases or to carry it with them dried on threads, thus avoiding the problem of transmitting the infection from natural cases. Even if we reject Dixon's point, Maitland's example of the danger of inoculation is very suspect, because many cases of natural smallpox were to be found in London every week of every year during this period (see the London Bills of Mortality) – therefore it is quite possible for the domestic servants to have caught the disease naturally from another source. A much better type of evidence is that referring to a situation where a partial inoculation takes place in an isolated rural area in response to the threat of an epidemic. In the twenty-one volumes of the *Statistical Account Of Scotland* many of the incumbents described the recent history of diseases in their parishes – of the 234 incumbents who mentioned that inoculation has taken place in their parishes not one specified an instance of it spreading the natural disease to vulnerable members of the population. An even more convincing example of this point is supplied by Dr John Forbes (a supporter of vaccination and an opponent of inoculation), who in his description of the smallpox epidemic of 1821 in the Chichester area had to admit that

> during the winter months he [a local inoculator] inoculated upwards of 1,000 persons [around the country area outside Chichester] ... not more than 130 or 140 cases of natural smallpox were witnessed by all the surgeons during the course of the epidemic. Of these, by far the greater number occurred in Chichester, owing to the continued resistance of the surgeons to inoculate.[40]

Professor Downie goes on to point out Haygarth's belief in the contagiousness of inoculation. All contemporaries believed that inoculation spread smallpox, inasmuch as they believed it to be itself a mild form of natural smallpox. However, when it came to a question of empirical evidence rather than theoretical belief, there is no doubt about the

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[39] C. W. Dixon, *Smallpox* (1962), p. 232.
[40] Forbes, *op. cit.*, pp. 213, 215.

conclusion to be drawn. Haygarth himself concluded in 1781 from his experience in Chester that

> Inoculation did not, as some might apprehend, spread the contagion, but appeared to produce a quite contrary effect. For in the districts where most patients were inoculated, there remained the fewest in the natural smallpox, and, in the districts where the smallest number were inoculated, the distemper was afterwards most general.[41]

The most conclusive evidence for the relative non-contagiousness of inoculation is the series of experiments by O'Ryan which were quoted in the text of my paper. Early 'vaccines' were directly derived from smallpox virus without transmission through a cow (e.g. Walker's 'vaccine'), and it has never been suggested that such 'vaccines' spread natural smallpox.

According to Professor Downie the Suttonian technique consisted "of taking material from the site of inoculation of the smallpox after four or five days for further inoculation". This was not the case – the Suttons took their material from any pustule around the body and not just from the site of inoculation; they also took their material from pustules at every stage of development.[42]

The essence of their technique was the use of a lancet, making the lightest of scratches and inserting the minimal amount of material. As for the mortality from inoculation, it is very difficult to assess independently of mortality due to natural smallpox before inoculation had time to take effect. As I have already indicated, in the controlled conditions of the London Smallpox Hospital its mortality was negligible, particularly in the later period – e.g. of the 431 in-patients inoculated between 1808 and 1813 not one died.[43] Pearson, one of Jenner's chief supporters, conceded that the mortality from inoculation was negligible and quoted two examples:

> Dr William Heberden informs me, that at Hungerford, a few years ago, in the month of October, 800 poor persons were inoculated for the smallpox, without a single case of death. No exclusion was made on account of age, health, or any other circumstances, but pregnancy;

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[41] Haygarth, *An Inquiry ...*, p. 188.

[42] R. Houlton, *Indisputable Facts Relative to the Suttonian Art of Inoculation* (1768) p. 40.

[43] R. Hutchinson, "A historical note on the prevention of smallpox in England", *Health Annual Report* (Ministry of Health, 1945) Vol. 46. Appendix A, p. 122.

> one patient was eighty years of age; and many were at the breast, and in the state of toothing. Dr Woodville acquaints me, that in the current year (1798), from January to August inclusive, out of 1,700 patients inoculated at the Inoculation Hospital, including the in and out patients, only two died, both of whom were of the latter description.[44]

It should also be remembered that Walker's 'vaccine' which was the one most widely used in early nineteenth-century England, was in fact diluted and attenuated smallpox virus – and it gave rise to a negligible rate of mortality.

I have not disputed the power of cowpox to protect against smallpox, but have argued that vaccinia was directly derived from smallpox. Professor Downie counters this point by stating that it has been impossible during the past twenty-five years to infect cows with smallpox virus, i.e. produce cowpox from smallpox. He suggests that the very many previous successes in doing this were due to "cross infection of the animals with strains of vaccinia in use in the same establishments." This argument is implausible given that the purpose of trying to infect cows with smallpox was not experimental, but was an attempt to produce vaccinia which was otherwise not available. Vaccines were difficult to maintain and acquire, hence the attempts to produce them 'artificially'.[45] This being so it is highly unlikely that vaccinia was present in these establishments. If "cowpox is a natural disease of cattle" why is it not to be found in New Zealand where there is little or no smallpox and vaccination, and why do not cases of human cowpox arise in slaughterhouse workers ? As Dixon has said: "This would suggest that cowpox is not a natural disease of bovines".[46] Cowpox appears to have increased considerably with the advent of inoculation in the eighteenth century and declined during the nineteenth and twentieth centuries when inoculation disappeared and the amount of vaccination diminished. This would suggest that Copeman was right in thinking that for smallpox to be suitable for adaptation to the cow it must be taken from an inoculated rather than a natural case (it should be noted that there were several mass inoculations in Gloucestershire at about the time that Jenner

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[44] G. Pearson, *An Inquiry Concerning The History Of The Cow Pox* (1798), p. 79.
[45] See for example J. Jones, *Vaccination* (1884), pp. 401–3.
[46] Dixon, *op. cit.*, p. 162.

discovered his first cases of cowpox, for example in 1795 at Berkeley and at Dursley in 1797 when over 1,100 people were inoculated). However, for the purposes of my paper it is not necessary to demonstrate that cowpox derives from smallpox, but merely to show that the early 'vaccines' were directly derived from smallpox without using an intermediary host such as the cow.

As for Professor Downie's last point about inoculation not being very widespread at the end of the eighteenth century, I have dealt very fully with this question in the paper to be published in the *Economic History Review*.[47] In fact the best evidence is to be found in the writings of Jenner and his early supporters, e.g. Jenner wrote: "... the common people were rarely inoculated for the small-pox, till that practice was rendered general by the improved method introduced by the Suttons ..."[48] These early writings are full of references to mass inoculations, and most of Jenner's cases of people with natural cowpox had been inoculated at some time during their lives. Professor Downie takes the experience of the towns as typical for the country as a whole, but only a small minority of the total population lived in such towns. In a country village or market town epidemics of smallpox were very infrequent, sometimes occurring only every twenty or thirty years. When such an epidemic did occur it struck such a large proportion of the total population (children and adults) and was so virulent (lack of a pool of antibodies) that the resulting panic drove everyone to be inoculated, for example, when an epidemic broke out in Blandford, Dorset, in 1766 "a perfect rage for inoculation seized the whole town".[49] In a place like Chester only a fourteenth of the population (all infants) had not suffered from smallpox, because it was virtually endemic, that is to say, in the town nearly every year. This bred a fatalistic attitude amongst the parents of poor children, particularly as the piecemeal nature of smallpox mortality did not lead to a spectacular demonstration of the effects of inoculation as it did in the country areas. Inoculation was virtually universal in such areas after about 1770, and was also making rapid headway in the large towns by the end of the eighteenth century.

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[47] See the previous chapter.

[48] *The Medical Repository*, Vol. 5 (1802), p. 239.

[49] C. Creighton, *The History Of Epidemics*, Vol. 2, (1894), p. 513.

Chapter 3

Population Growth and Economic Change in Eighteenth- and Early Nineteenth-Century England and Ireland[1]

Much more has been written about the history of inoculation in England than Ireland. Other than the additional material in my book, ***The Conquest Of Smallpox,*** *I am not aware of any further discussion of Irish inoculation history. The evaluation of inoculation in Ireland suffers from an absence of reliable demographic evidence for the eighteenth-century period.*

I did not give sufficient weight in this essay to the colonial relationship between England and Ireland, involving the transfers of wealth from the latter to the former. However, this is implicit in my discussion of Irish social structure, and the analysis of the effects of population growth could be broadened to take account of this omission.

In papers on this important and controversial subject Professor David Chambers has eloquently argued that although population growth and economic change were linked in eighteenth-century England, the increase in population cannot be explained directly in economic terms.[2] Traditional

[1] First published in E. L. Jones and G. E. Mingay (eds.), *Land, Labour And Population In The Industrial Revolution: Essays Dedicated To David Chambers* (1967).

[2] See particularly J. D. Chambers, "The Vale of Trent 1670–1800", *Economic History Review*, Supplement 3. He concluded from this study that population "was vulnerable to disease, but not as a result of famine. Epidemics could do their own

'medical' explanations of falling mortality have been discredited by medical historians, which has led them to an assume that economic growth must have preceded and brought about population expansion. In this essay I try to deal with some of the important problems raised by Professor Chambers, and will argue that the increase in population during the eighteenth and early nineteenth centuries was not primarily brought about by economic factors, but on the contrary, itself shaped the pattern of economic change.

I

The neo-Malthusian view of eighteenth-century population growth is that it was the consequence of increased fertility, resulting from earlier and more frequent marriage due to expanding employment opportunities and a rise in the general standard of living. However, there is evidence to suggest that both the age at marriage and the marriage rate were roughly constant throughout the eighteenth century.[3] Professor Chambers himself has published statistics for agricultural villages which suggest that both the birth and marriage rates may have declined between 1743 and 1801 in the Vale of Trent region.[4] In 1751 Thomas Short published statistics of population, baptisms, marriages, and burials during 1724–36 for seven market towns and fifty-four rural parishes.[5] According to his figures, the baptism rate was 33.8 per 1,000 and the burial rate 29.4 per 1,000.[6] If we

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work without its aid, nor it would seem, did they require the assistance of gin ... For reasons which are far from clear, [disease] severity was mitigated from the middle of the [eighteenth] century in this region, especially in regard to the lower age groups ..."

[3] The figures for the age at marriage are derived from marriage licences which are not entirely satisfactory. However, figures from parish registers suggest a similar conclusion. See C. C. Morell, "Tudor marriages and infantile mortality", *Journal Of State Medicine*, XLIII (1935), p. 179. [1994] For a further discussion of these issues, see Chapter 7.

[4] Chambers, *op. cit.* p. 55.

[5] T. Short, *New Observations On Bills of Mortality* (1751), p. 133.

[6] Undoubtedly some births and deaths were not registered owing to the presence of Dissenters, particularly in the market towns. This, of course, would raise both the 'true' birth and death rates.

compare these rates with those computed from civil registration returns in the 1840s, it appears that the long-term birth rate was more or less constant, while there was a sharp fall in the death rate. This overall conclusion is confirmed by the figures for agricultural villages published by Chambers.[7]

One of the weakest points in the neo-Malthusian argument is that the fairly reliable figures of the 1840s indicate no particular association between the distribution of industry and high fertility rates. The counties with the highest age-specific birth and marriage rates and the lowest age at marriage during the early 1840s were Cambridge, Bedford, Huntingdon, and Northamptonshire, all largely agricultural counties. Although Lancashire had a high crude birth rate, its age-specific birth rate and age at marriage appear to have been about average.[8] Furthermore, the age at marriage of spinsters appears to have varied little between different social strata during the eighteenth century, suggesting that economic considerations were not paramount in determining the age at marriage at least for women.[9]

It is difficult to reach reliable conclusions from the statistics derived from the Anglican parish registers. The figures for burials are probably more accurate than those for baptisms, as few Nonconformists were buried outside the Anglican Church,[10] and the main reason for the under-registration of deaths was probably the existence of private burial grounds in the

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[7] Chambers, *op. cit.*, p. 55. The reverse of these trends applied, however, in the town of Nottingham.

[8] The age at marriage in Lancashire in the 1840s was about the same as for the country as a whole. The ranking of age-specific birth-rates varies considerably according to which age group of women is considered; if the age group 20–30 is taken the age-specific birth-rate was below average, for the age group 15–45 it was above average. See the *Registrar-General's 4th Report*, (1842), p. 9; *R.-G.'s 8th Report* (1847), pp. 37, 187.

[9] The mean ages at marriage of spinsters calculated from the Nottinghamshire marriage bonds and allegations for the period 1701–70 were as follows (number in sample is given in brackets): Farmers and yeomen: 24 (285); Husbandmen: 24.5 (235); Labourers and servants: 25 (390); Artisans and tradesmen: 23.5 (290); Gentlemen: 24 (210).

[10] There were four baptism birth registers to one burial register kept by religious nonconformists before 1810. Few Methodists buried outside of the Anglican Church before 1810. See *Report On Non-Parochial Registers*, (Parl. Pap. 1837–38, XXVIII).

large cities[11] If we exclude urban industrial counties from the analysis, it is clear that there was a substantial fall in the death rate during the eighteenth century. According to Deane and Cole, the death-rate in eighteen southern counties fell from 30.6/1,000 in 1701–50 to 20.6/1,000 in 1801–30.[12] Although these figures should not be taken too literally, the long-run trend is probably fairly accurately described by them.

In addition to this evidence, several studies of the aristocracy and gentry indicate that there was a sharp drop in mortality during the middle of the eighteenth century.[13] Hollingsworth's study of the aristocracy yielded the following increase of expectation of life at birth for females during the eighteenth century:

Table 1. Expectation of Life (Years) at Birth for Aristocratic Women[14]

Period	*1700–24*	*1723–49*	*1750–74*	*1775–99*	*1800–24*
	36.3	36.7	45.7	49.0	51.71

Most of the increase in life-expectancy was due to the saving of life amongst younger age groups. These statistics are derived from sources sufficiently reliable for us to be sure that they describe a genuine decline in mortality. Although it is not justifiable to generalize about the total population from such a finding, we must attempt to explain it in terms which might be relevant for the whole population. Obviously an explanation in terms

[11] This was reflected in the death/burial ratios for different counties, e.g. the 1839–40 ratio for Lancashire was 1.61, as against the national average of 1.18. P. Deane and W. Cole, *British Economic Growth 1688–1959* (1962), pp. 108, 109. [1994]: However, although the non-registration of burials in non-conformist and private burial grounds was important in the nineteenth century, I now believe that burial registration was highly deficient in the eighteenth century and earlier, and that this was primarily due to clerical negligence. See Chapters 7 and 8 of the present book.

[12] *Ibid,* p. 127.

[13] See Chapter 1; T. H. Hollingsworth, "A demographic study of the British ducal families", *Population Studies,* XI (1957); T. H. Hollingsworth "The demography of the British peerage", *Supp., Population Studies.* Vol. 18, No. 2 (1964). New evidence presented in Chapter 7 of this book indicates that this fall in mortality began at the beginning of the eighteenth century.

[14] Hollingsworth, *op. cit.* (1964), p. 57.

of the quantity of food supply is irrelevant to groups such as the gentry and aristocracy. Mortality diminished so rapidly during 1750–74 that one must seek an explanation more radical than those usually given.

The elimination of smallpox amongst the aristocracy could explain much of the rise in the expectation of life for that group[15] and indeed for the whole of the increase in population during the late eighteenth and early nineteenth centuries. For the population as a whole inoculation only became popular after about 1765, when the Suttons perfected their much safer technique. Jenner himself recognized this. He wrote "that the common people were rarely inoculated for the smallpox, till that practice was rendered general by the improved method of the Suttons ..."[16] Howlett in 1782 collected statistics from 225 parishes for the two approximate periods 1734–53 and 1754–73; the balance of baptisms over burials in the first period was negligible, and was only slightly greater in the second, suggesting that the great increase in population occurred after 1770,[17] which fits in very well with the chronology of the spread of inoculation.

Other medical and environmental 'improvements' were largely associated with towns, yet in 1801 only about a fifth of the total population lived in towns with a population greater than 10,000.[18] Even as late as the 1840s

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[15] That inoculation was responsible for the elimination of smallpox, rather than vaccination, is supported by the negligible rise in life-expectancy for the aristocracy between 1800 and 1824.

[16] *The Medical Repository*, Vol. 5 (1803), p. 239. Chambers draws attention to payment by a Nottinghamshire parish to one of the Suttons for inoculating some poor children in 1767, *op. cit.*, p. 32, n. 4. He also notes a relatively slight smallpox epidemic occurring in Nottingham in 1801, which is not incompatible with the slow spread of inoculation in towns. The same is to some extent true of Boston, Lincs. (mentioned by Chambers), where the decline of registered smallpox deaths was from 14.1 smallpox burials per 100 baptisms during 1749–75 to 5.25 per 100 during 1776–1802.

[17] The exact figures are: 1734–53 – baptisms 109,478, burials 104,750, marriages 34,110; 1754–73 – baptisms 123,715, burials 109,758, marriages 40,285. See J. Howlett, *Observations On The Increased Population, Healthiness ... Of Maidstone* (1782), p. 14. This pamphlet was published anonymously and a copy of it is to be found in Maidstone Museum. [1994]: New work on parish register reliability suggests that any conclusions based on parish registers should be treated with caution.

[18] See B. R. Mitchell and P. Deane, *Abstract Of British Historical Statistics* (1962), pp. 8, 24–27.

mortality in the large towns was very high: for example, about 48.5 per cent of all males born in the Liverpool district died before the age of 5 during 1838–44.[19] Any improvements in the large towns would have been more than outweighed by the consequence of a smaller proportion of the total population living in the relatively healthy rural areas. Furthermore, the medical historians T. McKeown and R. G. Brown have pointed out that most of the medical 'improvements' during the eighteenth and nineteenth centuries, for example, fever hospitals and midwifery services, were probably ineffective.[20] Even if they were effective it is doubtful whether they affected more than a very small minority of the total population.[21]

In the country as a whole smallpox was the most significant epidemic disease so far as mortality was concerned in the eighteenth century. For example, Charles Deering the historian of Nottingham, wrote in 1751 that "there mostly happens once in five Years some Distemperature in the Air, which either brings along with it some Epidemical Fever, (tho' seldom very Mortal) or renders the Small-Pox more dangerous than at other Times; of this last, the Year 1736, was a fatal Instance ... the Burials exceeded that Year the Births by above 380 ..."[22] Deering's description of five-year cycles for smallpox echoes that known from bills of mortality and parish registers in other towns such as Northampton and Maidstone. His belief that the outbreak of smallpox in 1736 was the most severe epidemic since the plague, is borne out by the evidence. Smallpox was increasing in virulence throughout the seventeenth and eighteenth centuries, an increase which was particularly marked during the 1720s and afterwards. For example, the total number of smallpox deaths in Godalming, Surrey, was

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[19] *Registrar-General's 8th Report*, 1846, p. 206.

[20] T. McKeown and R. G. Brown, "Medical evidence related to English population changes in the eighteenth century", *Population Studies*, Vol. 9 (1955–6).

[21] With reference to improvements in midwifery, the figures produced by Dr Eversley for the Worcestershire area do not suggest any significant fall in infant mortality during the eighteenth and early nineteenth centuries; this finding is compatible with the high infant mortality rate (about 15 per cent) for England and Wales at the beginning of civil registration. See D. E. C. Eversley, "A survey of population in an area of Worcestershire from 1660–1850 on the basis of parish records", *Population Studies*, Vol. 10 (1956–7), pp. 269–71. [1994]: But this conclusion is now open to question, because of new evidence on burial register unreliability.

[22] C. Deering, *Nottinghamshire Vetus Et Nova* (1751), p. 82.

as follows: 1686, 50; 1701, 24; 1710–11, 39; 1722–3, 94.[23] The increasing virulence of smallpox probably explains the check to population which occurred in the 1720s.

Creighton, the medical historian, mentions influenza as an important disease during this period, but it rarely appears in bills of mortality and parish registers as accounting for large numbers of deaths. Also, Creighton could be misleading in his reading of evidence. For example, he reported a rumour that the high mortality in Exeter in 1729 was due to influenza, yet he overlooked the testimony of a local diarist, who recorded that not only was smallpox in the town that year, but was a particularly virulent variety.[24]

An improved standard of life may have diminished mortality amongst the general population, but such an explanation does not fit easily with the known chronology of population growth and per capita incomes. Any rise in real incomes of the labouring population probably took place during the first half of the eighteenth century, rather than the second[25], yet population increased much more rapidly at the end of the century. Also, growing real incomes cannot explain the sharp fall in mortality amongst the gentry and aristocracy. And finally, there was surprisingly little variation in adult male mortality between different occupational groups, due to income differentials, during the middle of the nineteenth century,[26] suggesting that income factors were not important in determining rates of mortality.

II

The most recent comprehensive work on the history of Irish population during the eighteenth and early nineteenth centuries is that by Professor K. H. Connell, who concluded that the great acceleration in population growth at the end of the eighteenth century was due "very likely to the increase of fertility that followed earlier marriage."[27] Connell argued that

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[23] *Surrey Archaeological Collections*, Vol. XXVII, pp. 16–20.

[24] "The Small Pox was very fatall to some. Mr. Vivian lost all his children, being four sons." See R. Pickard, *Population And Epidemics Of Exeter* (1947), pp 65, 66.

[25] For example, see Deane and Cole, *op. cit.*, pp. 19, 91.

[26] See the *Registrar General's 14th Report*, (1851), pp xviii, xxii.

[27] K. H. Connell, *The Population Of Ireland, 1750–1845* (1950), p. 248.

before the Great Famine of 1846, the age at marriage had been low, and that the famine itself had been responsible for dramatically increasing the average age at marriage. Connell's work was criticized by Dr Michael Drake, on the ground that the statistics for the 1830s do not, in fact, indicate a low age at marriage.[28] This can be seen in the Irish Census returns for 1841:

Table 2. Proportion Unmarried of 100 of the Population of the Respective Ages, Ireland, 1841[29]

		Under 17	*17–25*	*26–35*	*36–45*	*46–55*	*55+*
Males	Rural	100	93	44	16	16	8
	Civic	100	87	36	17	12	10
Females	Rural	100	81	28	15	12	12
	Civic	100	79	33	20	15	15

The distribution of the unmarried amongst various age groups was very similar to that in England at about the same time:[30] if allowance is made for the overstatement of early marriages in the statistics for the 1830s (as outlined by Drake), the mean age of marriage of spinsters and bachelors was nearly the same for both Ireland and England, i.e. about 24.5 for spinsters and 25.5 for bachelors.[31] Both the crude birth rate and age-specific fertility were similar for the two countries for the period around 1840.[32] It might be argued, of course, that the relatively late age of marriage in Ireland was not typical of the period before 1841. Drake has examined the statistics for the 1830s and has concluded that there was no trend towards a lower age at marriage in the 1830s.[33] Possibly at an even earlier period marriage

[28] M. Drake, "Marriage and population growth in Ireland, 1750–1845", *Economic History Review*, Vol. XVI (1963–4).

[29] *Population Census Ireland, 1841*, (Parl. Pap., 1843, XXIV), pp. 41, 42. Indeed Ireland appears to have had one of the highest mean ages at marriage and lowest marriage rates in Europe in the pre-Famine period.

[30] See Mitchell and Deane, *op. cit.*, pp. 15, 16.

[31] For English ages at marriage during 1839–41 see the *Registrar General's Fourth Annual Report* (1842) p. 10.

[32] The proportion of women between 15 and 44 as a percentage of the total female population and the crude birth-rate were about the same for both countries during this period. See Connell, *op. cit.*, pp. 30, 37.

[33] Drake, *loc. cit.*, p. 311.

took place at a lower age, but then the age at marriage would be rising throughout the early nineteenth century when population was increasing very rapidly.

The only evidence for early marriage is literary rather than statistical, but if the evidence for the 1830s is typical we are unable to rely upon the estimates of casual observers. Connell has written that "according to an official summary of the immense mass of evidence presented to the Poor Inquiry Commission of 1836, men in Galway usually married when they were between 14 and 21; in Leitrim between 16 and 22; in Mayo and Sligo usually under 20, and in King's County between 17 and 20",[34] yet according to the 1841 Irish Census there were only fifty-three married men and 480 married women under the age of 17 in the whole of Ireland.[35] It is probable that the Commission's informants had a vested interest in castigating the moral 'laxity' of agricultural labourers and small cultivators: they had to find an explanation for the poverty of the majority of the population, and what more convenient explanation than the Malthusian one?

Drake has argued that alternative explanations exist for the rapid expansion of the Irish population: (i) "that a highly nutritious and regular diet of potatoes so improved the health of Irish women that their fecundity increased markedly";[36] and (ii) "that the universal acceptance of the potato as the staple food would lead to a once-and-for-all drop in the general level of mortality".[37] There are two major difficulties with this interpretation. First, that population increased rapidly only after 1772, whereas potatoes had been used widely in Ireland since at least the beginning of the eighteenth century. Second, that diets in the earlier period were probably much more nutritious than the exclusive reliance on potatoes at a later date. Petty wrote in about 1671–2 that "The Diet of these people [the Irish] is Milk, sweet and sower, thick and thin, which is also their Drink in Summertime, in Winter, Small-Beer or Water. ... Their Food is Bread or Cakes, whereof a Penny serves a Week for each; Potatoes from August till May, Mussels, Cockles and Oysters, near the Sea; Eggs and Butter, made

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[34] K. H. Connell,"Peasant marriage in Ireland: its structure and development since the Famine", *Economic History Review*, Vol. XIV (1961–2), p. 520.

[35] *Population Census Ireland , 1841* (Parl. Pap., 1843, XXIV), p. 439.

[36] Drake, *op. cit.*, p. 311.

[37] *Ibid*, p. 312.

very rancid, by keeping in Bags. As for Flesh ... tis easier for them to have a Hen or Rabbit, than a piece of Beef of equal substance".[38] Several contemporaries thought that the Irish poor could no longer afford milk and other 'extras' during the late eighteenth and early nineteenth centuries.[39] It seems inconceivable that the slightly more luxurious earlier diet was less nutritious than potatoes by alone. And if potatoes were associated with higher fecundity, why were not Irish women – with their more exclusive reliance on potatoes – more fertile than English women?

If increasing fertility cannot explain rising population, what is the evidence that falling mortality was responsible for growth? Mortality in Ireland appears to have been lower during the 1830s than it was in England. According to the retrospective statistics collected for the Irish census of 1841, the crude death rate was 16.8 per 1,000 for the late 1830s,[40] whereas in England and Wales for the same period it was 22.2 per 1,000.[41] That this finding is not an artefact of the method of collecting statistics or due to differences in the age composition of the two populations is demonstrated by comparing age-specific death rates for the year 1840/41.[42] Below the age of about 35 the Irish mortality rates were all lower than the English, with the greatest disparity occurred amongst young children: Ireland suffered about 40 deaths per 1,000 children living under the age of five, whereas the equivalent English rate was about 67 per 1,000.[43] The explanation for this marked difference in child-mortality rates is probably that a much higher proportion of the Irish population lived in rural areas. Within Ireland, the urban civic districts had a child mortality rate (about

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[38] Other writers during the late seventeenth century emphasize potatoes and milk in the diets of the Irish poor. See G. O'Brien, *The Economic History Of Ireland In The 17th Century* (1919), pp. 137–42.

[39] G. O'Brien, *The Economic History of Ireland From The Union To The Famine* (1921), p. 21.

[40] *Ibid*, p. 189.

[41] *The Registrar-General's Fifth Annual Report* (1843), p. 379.

[42] As the number of deaths in 1840 was ascertained from a house to house survey made in the following year (1841), the figures likely to be reliable, especially for young children's deaths.

[43] For the Irish age-specific mortality rates see Connell, *Population Of Ireland* p. 193; for English mortality rates for roughly similar age groups, see Mitchell and Deane, *op. cit.*, pp. 38, 40; for the exact figures under the age of 5, *The Registrar-General's Fourth Annual Report* (1842), p. 128.

78 per 1,000) well over twice that in the rural districts (about 35 per 1,000). The conclusion to be drawn from these comparisons is that like the age at marriage, and the age-specific birth and marriage rates, the age-specific death rate in Ireland was similar in about 1841 to that in England and Wales when allowance is made for distribution effects of population in urban and rural areas. This essentially implies that demographic factors were independent of economic differences, a conclusion similar to that reached from a study of the age at marriage and age-specific birth and marriage rates within England during the eighteenth and nineteenth centuries.

If the increase in Irish population before the pre-1841 period is not to explained in terms of a high birth rate associated with a low age at marriage, but in terms of a falling death rate, what possible cause or series of causes could explain any reduction in mortality during the late eighteenth and early nineteenth centuries? We have already rejected the hypothesis that there was an improvement in the Irish diet during the eighteenth century. Professor Connell, after reviewing possible causes for a reduction in mortality, concluded that his "examination of the social habits and the housing of the Irish, the dissemination of hospitals and dispensaries, the spread of vaccination and the incidence of fever does not support the proposition that in Ireland, as is said to have been the case in England, greater cleanliness and medical advances led to a substantial lowering of mortality".[44] Professor Connell also reviewed the history of smallpox and inoculation, but unfortunately did not treat the subject at any length; here it is only possible to elucidate some hypotheses and briefly illustrate them with relevant statistics.

Smallpox appears to have been present in Ireland at least from the Middle Ages onwards and had become endemic before the eighteenth century.[45] The disease seems to have occurred almost every year in Dublin during the period 1661–1746, when bills of mortality were kept.[46] Accord-

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[44] Connell, *op. cit.*, p. 239.

[45] As Rogers wrote in 1743: "though of foreign Growth, and by Transplantation brought in amongst us, it is now become a Weed of our own Soil, and a Native of our Country." Joseph Rogers, *Essay On Epidemic Diseases* (1734), p. 82.

[46] For a description of the content of the bills and relevant statistics, see J. Fleetwood, *History Of Medicine In Ireland* (1951), p. 65, and Dr J. Rutty, *A Chronological History Of The Prevailing Diseases In Dublin* (Dublin, 1770).

ing to statistics derived from these bills, smallpox accounted for about 20 per cent of the total deaths during the two periods 1661–90 and 1715–46.[47] Smallpox killed about 33 per cent of all children born during 1715–46, according to the Dublin bills of mortality. No other reliable statistics of smallpox mortality are available for Ireland before the 1830s.

Several observers described smallpox epidemics the 1760s. Dr. James Sims recorded a smallpox epidemic in 1766–7: smallpox broke out with unheard

> havock, desolated the close of this year [1766], and the succeeding spring of 1767. They had appeared above a year before along the eastern coast of the kingdom, and proceeded slowly westward with so even a pace, that a curious person might with ease have computed the rate of their progress. ... As they had not visited the country for some years, numerous subjects were grown up for them to exercise their fury upon, and many blooming infants were just opening to the sun, in vain, since they were so soon to be cropt by this unfeeling spoiler. Of thousands who caught the infection in this [Tyrone] and the neighbouring counties, scarcely one-half escaped, and even of these, some with the loss of one or both eyes, and several with faces so altered, as to be known with difficulty by their most intimate acquaintances.[48]

A later epidemic in 1770 was less mortal but this was attributed to "the want of subjects for them to exercise their fury upon, the preceding disorder having left few who had not undergone the malady, than to any abatement in their malignancy".[49] These descriptions of smallpox epidemics in the countryside are very similar to those found in England before the advent of inoculation, and smallpox was always more virulent in isolated country areas owing to a lack of a pool of antibodies.

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[47] The actual figures are as follows: Dublin, 1661–90: smallpox deaths (annual average) – 472, total deaths (annual average) – 2,236; 1715–46 (excluding 1739): smallpox deaths – 13,759, total deaths – 74,585; total births – 42,566.

[48] J. Sims, *Observations On Epidemic Disorders* (1773), pp. 36–38.

[49] *Ibid*, pp. 134–5.

III

The Rev. H. Townsend wrote in 1810 that the increase of Irish population was partly due to "the universal custom of inoculating children for the smallpox, a disorder, which was once a little less injurious in its ravages than the plague".[50] In this essay I will briefly examine the evidence for this claim.

Inoculation was introduced into Ireland in 1725, and it seems to have spread very slowly amongst the general population. The watershed of the practice of inoculation in Ireland, like that in England, was probably the perfection of a safe technique by the Suttons during the 1760s. The Suttons appointed several partners in Ireland: "Messrs. Houlton, Blake and Sparrow in Dublin; John Hailey, MD. in Cork; John Morgan, MD. in Straborne, Tyrone; and Messrs. Vachell, Ward, Shields & Arnold soon [1768] to be appointed to particular districts in Ireland".[51] Inoculation does not appear to have been used much during the 1766 epidemic as described by Sims, although he refers to the existence of 'inoculators' at that time.[52] Houlton observed in 1768 that several itinerant inoculators were claiming that they practised the safe Suttonian technique,[53] and as this was probably the beginning of popular inoculation in Ireland.

In 1769 "a special infirmary was set apart in the Foundling Hospital of Dublin, for Experimenting with inoculation upon the inmates".[54] In April 1777 "agreeable to the humane resolutions of the King's County Infirmary, 461 persons were, in the course of last month, inoculated".[55]

By the beginning of the nineteenth century inoculation was practised almost universally. The Dublin College of Physicians, when asked their opinion in 1807 of vaccination, replied that "Variolous Inoculation had

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[50] Rev. H. Townsend, *Statistical Survey Of The County Of Cork* (1810), p. 90.

[51] R. Houlton, *Indisputable Facts Relative To The Suttonian Art Of Inoculation* (1768), p. 10.

[52] Sims, *op. cit.*, p. 42.

[53] Houlton, *op. cit.*, p. 25: "Some, I am informed since my arrival in Ireland, are now travelling over several parts of the kingdom ..."

[54] *Population Census 1851* (Parl. Pap., 1856, XXIX), p. 146.

[55] *Ibid*, p. 422.

been long, almost exclusively in the hands of a particular branch of the profession ('irregular practitioners') ... being the usual medical attendants in families, and especially employed in the diseases of children. ... Smallpox is rendered a much less formidable disease in Ireland by the frequency of inoculation for it ... hence parents, not unnaturally, objected to the introduction of a new disease [vaccination] rather than not recur to that with the mildness and safety of which they are well acquainted".[56] The difficulty of tracing the history of inoculation in Ireland is that most of it was carried out by "individuals [who] proceed from village to village several times during the year for the purpose of inoculating the infantile population",[57] a practice made necessary through the lack of doctors in Ireland. The activities of the itinerant inoculators were noted in Derry in 1812[58] and in Co. Waterford, Cork, Kerry, and Clare at later dates.[59] Sir William Wilde noticed the activities of the inoculators as late as 1851.[60]

Professor Connell seems to have accepted that inoculation was practised very extensively, but accepted the conventional medical view about the dangers of inoculation. According to Sir William Wilde, vaccination was practised in Irish towns much more than in country areas, as a result of a preference for inoculation amongst country people.[61] Smallpox mortality was much lower in the country areas than in the towns:

Table 3. Irish Smallpox Mortality in Town and Country[62]

Period	*Population 1841*	*Smallpox Deaths 1831–40*	*Annual Average Smallpox Deaths Per Million Living*
Civic Districts	1,135,465	12,418	1,093
Rural Districts	7,039,659	45,459	647

This difference cannot be explained by the different age structures of the

[56] *Report Of The Royal College Of Physicians Of London On Vaccination* (1807).
[57] *Population Census Ireland 1841,* (Parl. Pap. 1843, XXIV), p. xii.
[58] W. S. Mason, *Statistical Account, A Parochial Survey Ireland,* I (1814), p. 313.
[59] *First Report Of The General Board Of Health In The City Of Dublin,* pp. 94–97.
[60] *Population Census Ireland, 1851* (Parl. Pap. 1856, XXIX), p. 422.
[61] *The Epidemiological Society Report* (1852–53), p. 29.
[62] *Royal Commission On Vaccination, 1st Report* (1889).

town and countryside population (they were approximately similar), or by the greater extent of smallpox in the towns: everywhere in Ireland during the 1830s smallpox was a young child's disease: 49,000 of the 58,000 total smallpox deaths during 1831–40 were of children under 5 years of age. Most children caught it (unless they were inoculated or vaccinated) by their fifth birthday. In such a situation inoculation could not spread smallpox, as it was already a universal disease. Smallpox mortality was higher in urban areas because inoculation and vaccination were less practised there. Rural areas had lower smallpox mortality rates because of the protection given by inoculation. The total smallpox mortality rate of Ireland was about 710 annual deaths per million living. Although this figure may appear at first sight to be high, it is, in fact, remarkably low if compared with earlier mortality rates. In Dublin during 1661–90, for instance, the smallpox mortality rate had been about 8,600 per million.[63] Expressed as a proportion of total deaths, smallpox had accounted for about 20 per cent of deaths in the 1661–1745 period in Dublin, whereas in that city during 1831–40 it accounted for under 3 per cent of them.[64]

The rate of 710 per million is also low by what might be expected if neither inoculation nor vaccination had been utilized on a wide scale. The case fatality rate of natural smallpox amongst infants was about 40 deaths per 100 cases during the 1830s.[65] Had all children under the age of 5 caught smallpox without inoculation or vaccination, the smallpox mortality rate would have been 400,000 deaths per 1,000,000 living rather than the 39,300 per 1,000,000 which was the actual rate for children under 5,[66] i.e. it would have been about ten times the actual rate. The point of these

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[63] This is using Petty's population figure of 55,000 for Dublin; undoubtedly this is an underestimate, but so many deaths were not registered that the two underestimations appear to cancel each other out. The overall crude death-rate using Petty's population figure is about 40 per 1,000, a not unreasonable figure for a city the size of Dublin during this period.

[64] *Report ... By The ... Vaccination Committee 1853*, (Parl. Pap. 1852–53, CI), p. 80. The smallpox mortality statistics in this essay should be taken only as approximations, as smallpox deaths were probably under-registered.

[65] See the *Royal Commission On Vaccination, 1st Report* (1889), pp. 74, 215; *Ibid, 6th Report*, pp. 717–20; E. G. Edwards, *A Concise History Of Smallpox And Vaccination* (1902), p. 55.

[66] Connell, *The Population Of Ireland*, p. 219

hypothetical comparisons is to indicate the scale of saving of life by inoculation and vaccination.

Because of a lack of reliable statistical data for Ireland in the eighteenth century, it is not possible to trace the precise effects of inoculation. However, informed observers attest to a significant decline in smallpox mortality in the last quarter of the eighteenth century. Sir William Wilde's survey of smallpox epidemics in Ireland shows that the half-century between 1776 and 1827 had no major outbreaks of the disease.[67] The apparent success of inoculation and vaccination could account for a substantial part of the increase in population after about 1770.[68]

IV

I have suggested that much of the population growth in both England and Ireland during the latter half of the eighteenth and first half of the nineteenth centuries may be explained by the gradual elimination of smallpox, and therefore may be considered independent of contemporary economic changes. But since it appears that the demographic experience of the two countries was similar, why was it that economic effects were so different? Any answer to this question would be complex, involving consideration of a wide range of economic, social, political and other factors. Here, I will suggest some points of possible relevance.

The cloth industry was England's chief commercial manufacture during the eighteenth century, but it accounted only for about 5 per cent of the total national income,[69] and its domestic market appears to have changed

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[67] *Population Census Ireland, 1851* (Parl. Pap. 1856, XXIX), p. 422.

[68] Certainly if the 1767 epidemic was typical of pre-inoculation experience, the disappearance of smallpox in Ireland would explain a significant increase in population. Generally, smallpox mortality appears to have been heavier in Ireland than in England; nevertheless population expansion in Ireland before 1770 was probably due to earlier long-term changes such as the disappearance of the plague. In this sense, the gradual elimination of smallpox would only explain the great acceleration of Irish population after 1770.

[69] Deane "The output of the British woollen industry in the eighteenth century", *Journal Of Economic History,* Vol XVII (1957), p. 221.

little between 1695 and 1772.[70] As most of the expansion in the cloth industry before 1772 can be explained as a consequence of increasing exports, we must ask how much other economic growth during this period was due to domestic expansion. Deane and Cole have argued that a general economic expansion took place from the 1740s onwards. This conclusion is based, however, on an index of real output which is virtually an index of estimated population growth, as agriculture (43%) and rent and services (20%) are both based on questionable estimates of the size of population.

An analysis of the production series which are available throws considerable doubt on the 1740s as a turning-point. As one writer has pointed out: "Of the dozen or so commodities for which output figures are available there are several in which the levels reached in 1741–45 and 1746–50 were lower than those achieved earlier in the century. This is true of strong beer, starch, hides and skins, coal imports, raw silk and thrown silk. Indeed, for some of these commodities the 1740s is a low point. In other commodities, such as printed goods and soap, the acceleration of output was clearly later in the century".[71] This criticism appears valid, since, if one takes Deane and Cole's own home industries index (beer, leather, candles, and soap), the uninterrupted and main increase in production certainly occurs after 1770.[72]

One hypothesis which would explain differences in the chronology of increased consumption of different commodities is that the consumption of quality goods increased much sooner and in greater quantities than that of cheaper goods. The output of tallow candles, used by poorer people, doubled between 1715 and the end of the century, whereas that of wax candles, used by the wealthier classes, increased nearly tenfold.[73] The

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[70] According to Deane's estimate, domestic consumption of manufactured cloth was about £3 million in 1695. If one accepts the proportion of Yorkshire woollens and worsteds exported in 1772 as being typical of the country as a whole (at this time Yorkshire output accounted for about 60 per cent of the total) domestic consumption of manufactured woollen cloth was also about £3 million in 1772. See Deane, *op. cit.*, pp. 220, 221.

[71] D. Whitehead, "History to scale? The British economy in the eighteenth century", *Business Archives And History, Vol. 4*, No. 1 (1964), p. 83.

[72] The index numbers were as follows (beginning at 1700 and continuing at every tenth year until 1800): 100, 98, 108, 105, 105, 107, 114, 114, 123, 137, 152. Deane and Cole, *op. cit.*, p. 78.

[73] T. S. Ashton, *An Economic History Of England: The Eighteenth Century* (1955) p. 60.

production of high-quality white glass nearly quadrupled between 1747 and 1801, whereas that of common bottles only began to increase during the 1790s.[74] The best comparison between the output of quality and cheap goods is between silk and beer. The consumption of silk increased rapidly after 1755, whereas that of beer only really began to increase after 1775.

Table 4. Output of Quality and Cheap Goods, 1695–1804[75]

	Imports of Silk (1695–1704 = 100)	*Strong Beer Production (1695–1704 = 100)*
1695–1704	100	100
1705–1714	92	99
1715–1724	110	112
1725–1734	130	104
1735–1744	107	102
1745–1754	116	108
1755–1764	153	113
1765–1774	182	112
1775–1784	203	123
1785–1794	225	136
1795–1804	217	163

It may be suggested that the earlier expansion of the market for quality products was a result of the rapidly increasing population of the aristocracy, gentry, and other wealthy groups. During the eighteenth century about a quarter of the national income went to 3.5 per cent of all families, that is to say, the aristocracy, gentry, and merchant class.[76] Due to decreased mortality their numbers probably quadrupled between 1750 and 1850,[77] and they were the social classes most able to translate their increased numbers into effective demand. This could have occurred in several ways: by a switch from savings to consumption; by increased borrowing, including mortgaging of land; improvements of their assets, through the enclosure of land and a more intensive use of their capital in

[74] Mitchell and Deane, *op. cit.*, p. 267.

[75] Deane & Cole, *op. cit.*, p. 51. The index figures are only approximations.

[76] Mathias, "The social structure in the eighteenth century: a calculation by Joseph Massie", *Economic History Review*, Vol. X (1957–8), pp. 42–45.

[77] See T. H. Hollingsworth, "A demographic study of the British ducal families", *Population Studies*, Vol. XI (1957).

business.[78] The scale of possible profit from enclosures was estimated by Gregory King in 1685: only about half of the total land surface of England was farmed, of which three-fifths was cultivated under the common-field system;[79] and by a general exploitation of patronage through increased participation in Parliament, particularly with reference to finding places in the very rapidly expanding army.[80] The main problem would have been to find positions and capital for their now surviving younger sons and provide their daughters with portions; possibly this was one of the reasons for the frequent failure of many of the poorer gentry and yeomanry during this period.

Beginning probably during the 1770s, there was a considerable expansion of the home market for cheap woollens and cottons, due almost certainly to an increase in population rather than a growth in per capita incomes. It would appear that the domestic consumption of woollen cloth increased rapidly from about 1772: after this date the total output of woollen cloth rose, while the proportion exported fell from about 70 per cent in 1772 to 35 per cent in 1805, and 20 per cent after the 1820s.[81]

It is not necessary to describe the effects of the great upsurge in population after 1770 which affected every branch of economic and social life – the growth of canals, the improvement of roads, enclosure of land, development of the factory system – in short, the Industrial Revolution. Although increasing exports and the raised demand of the wealthy led to a expanded production, they were not the foundation of the fundamental change in the economy.[82]

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[78] Both the number of patents taken out and the number of bankruptcies increased sharply from the 1760s onwards: Mitchell and Deane, *op. cit.*, p. 268; Ashton, *op. cit.*, p. 254.

[79] See J. L. and S. Hammond, *The Village Labourer* (1919), p. 26 n. 1.

[80] The proportion of the old aristocracy in the House of Commons rose significantly during the eighteenth century, and younger sons of the aristocracy increased their numbers in the church, navy, and 'civil service', as well as in the army. The colonial army and mercantile 'administration' provided outlets particularly for younger sons of the gentry.

[81] Although this reduction in woollen exports was partly due to the substitution of cottons for woollens in the export market, only about 30 per cent of all cottons were exported during the second half of the eighteenth century. See Deane and Cole, *op. cit.*, pp. 185, 196.

[82] The growth in the export market depended partly upon emigration, and thus on population increase at home; inoculation was also widely used in America and the West Indies, and so was contributory to population growth in these markets.

They helped to maintain the real incomes of the mass of the population, and therefore helped to translate increased needs (from an enlarged population) into effective demand, which raised prices and stimulated economic growth.[83] Only a radical expansion of mass markets could provide the sufficient condition necessary for the fundamental transformation of the economy, that is to say, the growth of the new factory capitalism. It is no accident that this capitalism did emerge ultimately in Lancashire, after its earlier forms had developed elsewhere. Lancashire had been the centre of production of the very cheapest cloth in the early eighteenth century, and untrammelled by traditional constraints it was the natural place for the emergence of the factory system producing for a mass market.

V

In Ireland the result of the population explosion was the growth of a deepening subsistence economy rather than an industrial revolution. Although the Irish census of 1841 recorded that about 30 per cent of the total occupied population as employed in industry, two-thirds of these were women, most of whom worked at home in domestic industry, providing goods for local consumption.[84] The only province with a sizeable male population employed in industry was Ulster, the centre of the linen manufacture.[85] This industry had been encouraged since the beginning of the eighteenth century as a compensation for the destruction of the Irish woollen industry in 1699. Although the manufacture of woollen cloth was very small in Ireland at the end of the seventeenth century, it was growing very rapidly during the last decade. It was suppressed at the instigation of English clothiers, who were afraid it might eventually provide overwhelming competition.[86] The export of linen cloth and yarn trebled between

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[83] According to figures computed by Arthur Young, the price of wheat began to rise in about 1764; the price of wheat (statute measure) at the Windsor Market was as follows: 1714–38 – £1 15s. 5d. per qtr.; 1739–63 – £1 14s. 2d.; 1764–88 – £2 6s. 6d. See A. Young, *Annals Of Agriculture*, Vol. XIV (1790), pp. 228–30.

[84] T. W. Freeman, *Pre-Famine Ireland* (1957), pp. 76–77.

[85] *Ibid.*

[86] See G. O'Brien, *The Economic History of Ireland In The Seventeenth Century* (1919), pp. 227–9.

1718–47 and 1748–77, about 90 per cent finding its way into the English market.[87] In 1771 it was estimated that the manufacture of linen was worth £2,200,106, 70 per cent of the export output.[88] Linen was estimated to be worth about half the total value of all exports during 1771–7,[89] but its export importance declined during the late eighteenth and early nineteenth centuries, while home consumption appears to have expanded sharply during the same period.[90] Cotton, however, began to displace linen, for, as one observer noted in 1840:

> men cannot live for what they get for [linen] weaving now. There is a great difference in respect of the appearance of weavers who come to market now and formerly; they are not so well dressed, nor near so comfortable looking: the fine sturdy young men, who once came to the market, have now gone out of the trade, and many have emigrated to America. I remember when it was the best trade in Ireland; now it has gone to nothing. The cotton trade has ruined the linen; formerly everybody wore linen, and now everybody wears cotton.[91]

The change was probably due to the abandonment of protection of Irish industry in 1825, as even the domestic cotton industry began to wilt under the competition from England.[92] The first cotton mill driven by water power in Ireland was established near Belfast in 1784.[93] By the 1830s and 1840s "the deserted factory with its silent water wheel was already a familiar aspect of the Irish scene".[94] One of the main reasons for the eclipse of Irish industry was the lack of indigenous coal, although presumably the cheapness of labour might have more than offset the cost of importing coal from England.

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[87] A. W. Hutton, *Young's Tour Of Ireland*, Vol. 2 (1892), pp. 200, 202.

[88] *Ibid*, p. 201.

[89] *Ibid*, p. 255.

[90] The following are contemporary estimates: linen manufacture 1771: export – £1,541,200; home consumption – £658,906; value of linen manufacture 1817 – £3,151,752; exports of linen 1822 – £861,944. See Hutton, *op. cit.*, p. 201, and O'Brien, *op. cit.* (1921), p. 302.

[91] Freeman, *op. cit.*, p. 85.

[92] See O'Brien, *Economic History Of Ireland* ... (1921), p. 311.

[93] Freeman, *op. cit.*, p. 85.

[94] *Ibid*, p. 6.

The failure of industry in Ireland was probably rooted in the nature of the country's social structure. In 1779 Arthur Young had noted:

> [the] only considerable manufacture in Ireland, which carries in all its parts the appearance of industry, is the linen; and it ought never to be forgotten that this is solely confined to the Protestant parts of the kingdom; yet we may see from the example of France and other countries that there is nothing in the Roman Catholic religion itself that is incompatible with manufacturing industry. The poor Catholics in the south of Ireland spin wool very generally, but the purchasers of their labour, and the whole worsted trade, is in the hands of the Quakers of Clonmell, Carrick, Bandon, etc. The fact is, the professors of that religion are under such discouragements that they cannot engage in any trade which requires both industry and capital. If they succeed and make a fortune, what are they to do with it? They can neither buy land, nor take a mortgage, nor even fine down the rent of a lease. Where is there a people in the world to be found industrious under such a circumstance?[95]

Young was undoubtedly correct in emphasizing the lack of financial incentives for Catholics to engage in industry, and another factor probably as important was their lack of capital. Very little land was owned by Catholics, and as early as the late seventeenth century most of the Irish population were peasants relying on subsistence farming. According to one observer writing in 1691, "their food is mostly milk and potatoes, their cloathing coarse bandrel cloth and linen, both of their own make; a pot of gruel; a griddle whereon to bake their bread, a little salt, snuff, and iron for their ploughs being almost all they troubled their shopkeeper or merchant for. A little hut or cabin to live in is all that the poverty of this sort hope or have ambition for".[96] Petty had estimated that out of a total of 200,000 houses, 160,000 were without any chimney, suggesting that they "live in a brutish nasty condition as in cabins with neither chimney, door, stair nor window".[97] With this degree of poverty it must have been impossible for Catholic peasants to acquire capital sufficient to establish manufacturing industry, quite apart from the lack of a home market

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[95] Hutton, *op. cit.*, p. 65.
[96] O'Brien, *op. cit.* (1919), p. 141.
[97] *Ibid*, pp. 137–8.

suitable for the absorption of such manufactures. Any capital available was owned by the Protestant landlords, many of whom were absentees; and as the population grew it became increasingly lucrative for them to invest their money in land, from which it was possible to obtain very high rents.[98] The derivation of these rents was described by Arthur Young:

> The poverty, common among the small occupying tenantry, may be pretty well ascertained from their general conduct in hiring a farm ... they provide labour, which in England is so considerable an article by assigning portions of land to cottars for their potato gardens, and keeping one or two cows for each of them, and by means of living themselves in the very poorest manner, and converting every pig, fowl, and even eggs into cash, they will make up their rent ...[99]

In 1841 Ireland had a subsistence economy based on small peasant cultivation, widely scattered throughout the whole country: only about 20 per cent of the population lived in villages and towns, the rest in isolated cabins.[100] Pressure of population drove cultivation of potatoes "towards the summits of the hills"[101] and meant that "every possible spot of land is laboured".[102] Subdivision of land and an almost exclusive potato diet enabled population to grow, inasmuch as the survivors of diminished mortality did not starve – until the subsistence economy collapsed in 1846 and there occurred the great famine. The causes and consequences can best be seen in the following table:

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[98] O'Brien, *op. cit.* (1921), pp. 12, 89, 97, 98.
[99] Young, *op. cit.*, pp. 31, 32.
[100] Freeman, *op. cit.*, p. 27.
[101] Connell. *op. cit.*, p. 96.
[102] *Ibid,* p. 118.

Table 5: Size of Land Holdings in Ireland, 1841 and 1851[103]

Size of holdings	*Number of holdings*	
	1841	*1851*
Not exceeding 1 acre	134,314	37,728
Exceeding 1 but not exceeding 5 acres	310,436	88,083
Exceeding 5 but not exceeding 15 acres	252,799	191,854
Exceeding 15 but not exceeding 30 acres	79,342	141,311
Exceeding 30 acres	48,625	149,090

The very poor peasants and casual labourers were virtually eliminated within a decade: these were the inhabitants of the 'growth class' housing – one-room cabins – which declined in number by 355,689 between 1841 and 1851, a decline of about 70 per cent.[104] The majority of the people leaving these cabins probably emigrated, although their reliance on potato subsistence meant that many of them starved to death or died from fevers associated with the famine.

VI

Unable to industrialize, and with a rapidly expanding population increasingly dependent on the potato, famine in Ireland was inevitable. In England, on the other hand, all the conditions for industrial growth had been present before the population explosion: a relatively high standard of living and a social structure encouraging enterprise and providing a potential mass market; a thriving textiles industry; the existence of provincial capital markets and a great and growing commercial centre in London; relative political stability; a progressive agriculture; sufficient technical innovation; abundant market outlets and sources of supply in overseas markets – including colonial Ireland – to mention only the best known of the much-discussed influences on growth. Although in both countries population increased rapidly during the second half of the eighteenth century through the use of inoculation against smallpox, England was fortunate in being able to industralize and thus avoid the mass starvation that was the disastrous fate of Ireland.

[103] O'Brien, *op. cit.* (1921), p. 59.
[104] *Ibid*, p. 59.

Chapter 4

The Evaluation of Baptism as a form of Birth Registration through Cross-matching Census and Parish Register Data: a Study in Methodology[1]

This essay was written to address the problem of parish register accuracy. The paper discusses research involving the comparison of census and parish register data, and covers a number of topics relevant to the debate about eighteenth-century population growth. The arguments are technical and detailed and perhaps it is useful to summarise the main conclusions of the work. First, about a third of all births in the 45-parish sample were not registered through baptism, and this did vary greatly over time. Second, the quality of baptism registration varied greatly from parish to parish. Third, the 1851 Census appears to have been very accurate, both with respect to statements about birthplace and age.

The accuracy of age statements is an important issue as it is fundamental to estimates of population and demographic changes calculated in the Cambridge Group's back projection programme.[2] Recent research by Audrey Perkins involving the checking of the 1851–1881 censuses with the parish registers of six Kent parishes, confirms the findings of the present study and the next essay of this book: census birthplace and age statements were of a high order of reliability.[3]

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[1] Originally published in *Population Studies*, Vol. 26, No. 1 (1972). The research on which this paper was based, was conducted with the aid of a grant from the Social Science Research Council.

[2] See Chapter 7.

[3] Audrey Perkins, "Age checkability and accuracy in the censuses of six Kentish parishes 1851–81", *Local Population Studies*, No. 50, Spring 1993.

In England all new work on the historical demography of the pre-civil registration period has used information provided by parish registers. Most of the available registers end in 1837 with the beginning of civil registration. For a period of 300 years prior to that date, parish registers provide the only systematically collected data available for demographic analysis. The reliability of register information is crucial to English historical demography. Yet the only comprehensive scholarly study of parish registration accuracy – the work of J. T. Krause – has cast serious doubts upon its reliability.[4] Krause concluded that there were very significant changes in the accuracy of registration over time:

> ... it seems that parochial registration was relatively accurate in the early eighteenth century, became somewhat less so in the 1780s, virtually collapsed between roughly 1795 and 1820, and then improved somewhat between 1821 and 1837.[5]

Krause reached this conclusion from a general survey of all the factors which might have affected registration accuracy, not from evidence based on direct statistical measurement. The only such evidence is for London and Southampton at the end of the seventeenth century, compiled by David Glass by comparing parish register returns of baptisms and burials against information on births and deaths derived from tax returns.[6] Glass's work is, however, only the beginning of an attempt to measure directly the completeness of registration, and the major aim of this paper is to outline a new method of measurement developed through substantive application to a limited number of parishes.

One of the fortunate accidents of English demographic history is that the core unit of registration of baptisms, burials and marriages on the one hand, and nineteenth-century census returns on the other, has been the

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[4] J. T. Krause, "The changing adequacy of English registration, 1690–1837", in D. V. Glass and D. E. C. Eversley (eds.), *Population In History* (1965).

[5] *Ibid*, p. 393.

[6] D. V. Glass, "Notes on the demography of London at the end of the seventeenth century", *Daedalus* (1968). Information on Southampton privately communicated by Professor Glass.

parish, the boundaries of which have remained unchanged for centuries. It is thus possible to compare census information with that in the parish register. The logic behind this method may be summarized in highly simplified form, as follows: If the 1851 Census (the earliest with the relevant information) was perfectly accurate with respect to information on name, age and birthplace, it would be possible to check the completeness of Anglican baptism for people surviving to the census by comparing the census entries with those in the baptism register.[7] For example, the 1851 Census for Horringer, Suffolk, records that Peter Day was aged 44 years and was born in that parish; if baptism were a perfect form of birth registration, Peter Day would appear in the register in about 1807. In practice, of course, it cannot be assumed that the 1851 Census is perfectly accurate, but is it generally agreed by historical demographers that it was very much more reliable than the parish registers.[8]

It is possible to test the accuracy of the 1851 Census by comparing it in detail with that for 1861, on the assumption that a comparison of independent censuses will yield relevant information on errors (this will be discussed below). Anderson has undertaken such a comparison for a part of Preston, and found that for 475 people traced in both the 1851 and the 1861 censuses there was agreement on stated age to within two years in about 96% of cases and agreement on birthplace in 87.7%.[9] Allowing for the compounding of errors between the two censuses, these figures suggest that the 1851 Census was of a high degree of accuracy. The 1851 Census need not be perfectly accurate for the census/parish register comparison method to be of value; it is only necessary that the relative levels of census accuracy did not vary significantly between different age groups and parishes, which are key variables for checking variations in register reliability over time and between different parishes.

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[7] Assuming that baptism was performed in the parish of birth. This is one of the assumptions examined in the next chapter.

[8] See A. J. Taylor, "The taking of the census, 1801–1951", *British Medical Journal* (1951), pp. 715–720; P. M. Tillott, "Inaccuracies of census statistics resulting from the method of collection in 1851 and 1861", in E. A. Wrigley (ed.), *Nineteenth-Century Society: Essays In The Use Of Quantitative Methods For The Study Of Social Data* (1972).

[9] M. Anderson, "The Structure of the Family", in E. A. Wrigley (ed.), *op. cit.*

There is one other major problem. It cannot be assumed that the individuals enumerated in 1851 were typical of all those born before this date, as the characteristics of those dying before the census may have been different from those of the survivors. There is no direct way of dealing with this problem. It is only possible to mention the factors which might make the census population unrepresentative and to attempt to measure their effect on the reliability of baptism registration. Two factors might influence the representativeness of the census population: (i) variations in the delay between birth and baptism and effects of changes in infant mortality on the proportion of babies baptized; (ii) a correlation between social class and pre-census mortality and registration reliability. The first factor arises as a result of children dying before baptism: changes in birth/baptism delays and infant mortality rates would affect the proportion of births unregistered through baptism. The second factor is more-or-less self-explanatory – variations in class mortality could affect the representativeness of individuals who survived to the census.

The delay between birth and baptism can be measured where information on both dates is given in the register. Variations in infant mortality cannot be precisely measured because of the absence of exact information on the deaths of such infants, although it might be possible to use all the information in those unusual parish registers which list deaths as well as burials. The effects of social class on census representativeness may be directly assessed by measuring the relation between occupation/social status and registration accuracy; if no significant correlation exists, the distorting influence of social class can be ruled out.[10]

As an initial way of evaluating the accuracy of the 1851 Census, the following parishes were selected: Maidstone, Kent, Bethnal Green, Middlesex, Bramfield, Suffolk, Wylye, Wiltshire and Bretforton, Worcestershire. Individuals were then traced in both the 1851 and 1861 censuses wherever possible. The criteria for establishing identity between censuses were as follows:

(1) The same name. Name is such an important criterion for establishing the correct identity of an individual that it is basically impossible to

[10] Even if there were a significant correlation, it would only be important if there were variations in the relationship between social class and pre-census mortality, for we are primarily interested in relative registration accuracy, over time and between different parishes.

check variations in name spelling from one census to another or from census to parish register. There were occasions, however, when it was obvious that there was a genuine variation in spelling with phonetic equivalence; such variations were discovered through searching the baptismal registers, although the indexes to these registers usually provide relevant information on surname spelling variations. However, if there was any doubt about the name being the same, it was always assumed that the names referred to different individuals.[11]

(2) Residence in the same household of at least one other person of the same name in 1851 and 1861; this might be a relative, apprentice, friend, or servant. In many cases the individual was found to be living in the same house or street, particularly in Maidstone and Bethnal Green where it was difficult to locate individuals in both censuses except through a common address.

1,282 cases were located in both the 1851 and 1861 censuses for the five parishes. In 127 – 9.9% – there was disagreement about stated birthplace, and in 108 – 8.4% – a difference of three years or more in stated age. These figures conceal some important differences between the individual parishes, which may be initially summarized by grouping the three village parishes of Bramfield, Wylye and Bretforton together and comparing them with the urban areas, Maidstone and Bethnal Green, individually. The results are given in Table 1.

[11] That this is not a major problem has been confirmed by work done by Richard Wall of the Cambridge Group. He has kindly checked a proportion of Colyton census cases which could not be found in the baptism register (N.I.R. cases) against his own records – and there are no disagreements on the identification of individuals by name.

Table 1. 1851 and 1861 Census Comparison

	Maidstone	*Bethnal Green*	*Bramfield, Wylye and Bretforton*
Birthplace Statement Disagreements (B.S.D.s)	15 (5.9%)	75 (17.0%)	37 (6.3%)
Age statements differing by three years or more	15 (5.9%)	41 (9.3%)	52 (8.9%)
Total Cases	255	440	587

The figure which stands out in Table 1 is the relatively high proportion of birthplace statement disagreements in Bethnal Green, the other variations being relatively insignificant. Inaccuracies in birthplace statements are much more important for the comparison method than are discrepancies in age. The latter can be allowed for in the register itself to a large extent, by searching through a time period around the expected date of baptism, whereas the former cannot be checked through the register. The relatively large proportion of birthplace statement errors can probably be explained by the fact that Bethnal Green was a large urban parish with indistinct boundaries; many discrepancies in birthplace statement attributed the birth to contiguous parishes. Bethnal Green may also have had a larger proportion of non-natives who were likely to have been less precise about their parish of birth than natives. In order to test this hypothesis, the consistency of birthplace statements was measured for those stated in the 1851 Census to be born in the parish, as against non-natives.

Table 2. 1851 and 1861 Census Comparison of Birthplace Statements
(B.S.D. = Birthplace Statement Disagreement)

	Natives in 1851 Census			
	Maidstone	*Bethnal Green*	*Bramfield, Wylye and Bretforton*	*Total*
B.S.D.s	6	31	4	41
Total Cases	150	273	349	772
% B.S.D.s	4.0	11.4	1.1	5.3

Non-Natives in 1851 Census				
	Maidstone	*Bethnal Green*	*Bramfield, Wylye and Bretforton*	*Total*
B.S.D.s	9	44	33	86
Total Cases	105	168	238	511
% B.S.D.s	8.6	26.2	13.9	16.8

Table 2 suggests that it was not only in Bethnal Green that migration was important in causing birthplace statement errors. The most striking influence of migration and nativity was found in the three rural parishes, where there were nearly thirteen times as many birthplace statement disagreements amongst non-natives in the 1851 Census as amongst natives. The overall difference accords with what would be expected on commonsense grounds and the remarkably low number of birthplace statement disagreements for 1851 natives in the rural and small town parishes suggests that birthplace information for them was highly accurate. Those who had lived in such parishes all their lives are likely to have been quite certain about their birthplace; the probable source of error is the census itself, for example the faulty copying of information by the enumerator.[12]

The main conclusion to be drawn from this evidence is that the key information on birthplace is significantly more reliable for those stated as being native to a particular parish in 1851 than for those stated to be non-native. Since the census comparison method duplicates errors – an error in either of the 1851 and 1861 census will give rise to a birthplace statement disagreement – it could be argued that even the census data for 1851 non-natives were of high quality. It is, however, much easier to use the data for natives in the census/parish register comparison method, because of the concentration of cases in the one (native) register – non-natives would have to be traced in a large number of other registers. It is for this reason, and because of the more accurate census information for natives, that the census/register comparison has been restricted to natives only, with one exception which will be discussed below.

Individuals were included in the sample provided; (a) they were stated as native to the parish in the 1851 Census; and (b) aged 17 or over in the 1851 Census – persons under 17 would be too young to be found in the

[12] This has been discussed at some length by Tillott, whose figures suggest that this was very rare indeed in the rural and small town parishes. See Tillot, *op. cit.*

registers which ended in 1837.[13] Married women were excluded because of the extra work required in tracing their maiden names.

Some difficulty was anticipated at the outset in relation to the allowance to be made for age errors in the 1851 Census when deciding whether an individual was to be identified with an entry in the baptism register or not. The comparison of censuses suggested that ages in the census were likely, in most cases, to be accurate to within two years. The appropriate allowance for age error was decided on the basis of additional information collected to check the validity of names, birthplaces and ages used to identify an individual in a register. This additional information consisted of the names of the parents of those individuals enumerated with their parents in 1851, checked against the names of the parents listed in the parish register at baptism.[14] The relation between correct identity and discrepancies in age between the census and the baptismal registers is set out in Table 3.

Table 3. Correctness of Identity Established Through Parental Names

		Identity Correct	*Identity Incorrect*	*% Incorrect*
Discrepancies in Age Between Census Statement and Baptismal Date in Register. (45 parishes)	2 years	2,391	67	2.7
	3 years	54	6	10.0
	4 years	18	6	25.0
	5 years	7	4	36.4
	6–15 years	7	11	61.1
	Total	2,477	94	3.8

A number of conclusions follow from this table. The vast majority of cases located in the baptismal register were correct identifications, as independently measured by information on parents' names: only 3.8% of the

[13] The age of 17 was originally selected in order to allow for an error of three years for those born in 1834; eventually the appropriate age error decided upon was five years, but this change does not materially affect the results as these errors were of insignificant proportions.

[14] Discrepancies in the mother's name compatible with the father having re-married were accepted, but the minimum criteria for establishing identity were simply that at least one of the parents' names was known to be correct, and that neither of their names was incorrect.

total cases were discrepant. The proportion of discrepant cases, however, rises dramatically with increase in age discrepancies between census and register, and, although the number of cases in most of these groups is too small for precise generalization, the evidence does suggest that the majority of cases with an age discrepancy of five years or less are correct identifications; greater discrepancies yield a majority of incorrect cases. Table 3 also demonstrates the overall accuracy of birthplace statements in the 1851 Census, for these acted as criteria and pointers to the 96.2% of correctly identified cases.

It was therefore decided to include all individuals found in the baptismal register within five years on either side of the expected date of baptism (derived from stated age in the 1851 Census); cases with age differences of 6–15 years were excluded as not being in the register (N.I.R. = Not In The Register).[15]

Table 4. Distribution of Discrepancies in Age Between Census Statement and Baptism Date in Parish Register (45 parishes)

Years	*0*	*1*	*2*	*3*	*4*	*5*	*6–15*	*Total*
Number	1,792	1,226	406	195	93	56	85	3,853
Per Cent	46.5	31.8	10.5	5.1	2.4	1.5	2.2	100

By excluding the group with differences in ages between 6–15 years, we are only excluding 2.2% of all 'matched' cases – the vast majority of these cases are either perfect fits or show discrepancies of two years or less. But there are some special categories in the sample which are over-represented in the group with discrepancies between 6–15 years. The two main factors to be considered are variations in the proportions in N.I.R.s over time and between different parishes. Changes in the proportion of N.I.R.s over time may be measured by arranging the figures by age group as classified in the

[15] It was assumed that age errors greater than 15 years could not occur in the census, and so names outside this range were excluded. It should be noted, however, that baptisms of "persons of riper years" constituted between 2 and 3 per cent of all baptisms in England during the period 1885 to 1958 (see *Facts And Figures About The Church of England*, Church Information Office, 1962, p. 57), and so N.I.R. proportions are likely to overstate birth omissions from baptisms, although not to the full extent of this proportion as some children aged under 15 were included in these figures.

1851 Census, assuming that variations in birthdates are exact reflections of stated age, so that, for example, a person aged 51 in 1851 would have been born in approximately 1800. There was no significant correlation between census/register age discrepancies and different age groups, with one exception. The very small group of those aged between 81 and 90 in the sample of matched cases (51 cases) had a significantly higher proportion of age discrepancies.

Table 5. Distribution of Discrepancies in Age Between Census Statement and Baptism Date in Parish Register for 81–90 Age Group (45 Parishes)

Years	*0*	*1*	*2*	*3*	*4*	*5*	*6–15*	*Total*
Number	20	11	7	3	3	2	5	51
Per Cent	39.2	21.6	13.7	5.9	5.9	3.9	9.8	100

If Table 5 is compared with Table 4, it will be seen that the proportion showing differences of four or five years among the 81–90-year-olds is about two and a half times the figure in the total sample, and that with differences of six to fifteen years about four and a half times as great. The marked difference for the 6–15 years discrepancy suggests that some of the 81–90-year-olds were stating their ages in the 1851 Census inaccurately, although this must not be exaggerated – 90.2 per cent were correct to within 5 years.

Variations in age discrepancies between different parishes are generally insignificant; only 1.0% of the total sample (including matches and N.I.R.s) had age discrepancies of six years or more, and most parishes had discrepancies of this magnitude. Only seven parishes had 2% or more of cases with discrepancies of 6–15 years, which were as follows: Acomb 2 0%; Horton 2.6%; Hemyock 2.3%; Halberton 3.6%; Wheathampstead 2.3%; Hartland 4.6%; Speldhurst 2.1%. The only significant proportions are those for Hartland and Halberton, which also have higher proportions of age discrepancies of 4–5 years than the total sample. The reasons for the higher proportions in these two parishes are unclear.[16] But these age

[16] The proportion for Hartland with discrepancies of two years (9.9%) was significantly higher than average. Although a search for all cases within three years of expected date was made in all registers (in addition to a search of the index) a double search was only made in a limited number of parishes (including Hartland and

discrepancies should not worry us too greatly, for we will see that variations in proportions of N.I.R.s between different parishes are not greatly affected by variations in age discrepancies.[17]

We are now in a position to present the main findings of the census/parish register comparison. The sample of parishes for comparison was selected primarily on the basis of the availability of parish registers in the library of the Society of Genealogists. Parish registers were first selected on account of being typed or printed (and therefore available on loan) and indexed for the period covering at least the years 1760–1837, although there were one or two special exceptions to this. From among these, registers were selected haphazardly depending on their availability. The final sample consisted of 45 parishes, a full list of which is given in Table 6. This Table shows the variation between parishes of the proportion of census cases not found in the baptism register.

Table 6. Distribution of N.I.R.s by Parish, Total Sample (N.I.R. = Not In Register)

Parish	*Population*	*Total Cases*	*N.I.R. Cases*	*% N.I.R.s*
Bramfield, Suffolk	343	116	28	24.2
Aston Abbotts, Bucks.	340	91	10	11.0
Hunsdon, Herts.	481	32	7	21.9
Burtonwood, Lancs.	831	118	33	28.0
Cocking, Sussex	482	68	12	17.6
Acomb, Yorks.	979	101	6	6.0
Lapford, Devon	766	78	12	15.4

Halberton) due to shortage of time. Of nine parishes intensively searched within a two-year period, 66 alternative cases were found out of 1,126 (5.9%). This percentage does not differ greatly from that found through one search in all parishes: 4.0% for the three-year period. These low proportions confirm the general reliability of the census/register comparison method inasmuch as there is no significant problem in selecting a case (and therefore establishing a correct identity) from a number of alternative cases. Most of the evidence – in particular the name of parents – suggests that the vast majority of alternative cases were not related to each other in any way.

[17] Even if all cases in the seven parishes with discrepancies of 6–15 years were defined as being in the register and subtracted from the relevant percentages of N.I.R.s, the relative position of these seven parishes would not be materially affected.

Parish	*Population*	*Total Cases*	*N.I.R. Cases*	*% N.I.R.s*
Chipstable, Somerset	395	60	9	15.0
Wylye, Wilts.	510	89	9	10.1
Bretforton, Worcs.	575	91	5	5.5
Preston-Bisset, Bucks.	554	123	16	13.0
Medmenham, Bucks.	401	62	19	4.8
Horton, Bucks.	—	38	15	39.4
Chetwode, Bucks.	217	33	5	15.2
Barton-Hartshorn, Bucks.	137	29	4	13.8
Rushton, Northants.	429	88	13	14.9
Black-Torrington, Devon	1,115	198	87	44.0
Hemyock, Devon	1,185	222	26	11.8
Chardstock, Dorset	1,387	302	112	37.1
Great Clacton, Essex	1,281	231	43	18.6
Purleigh, Essex	1,184	127	29	22.8
Danbury, Essex	1,221	197	42	21.3
Chiddingstone, Kent	1,260	212	55	25.9
Tooting-Graveney, Surrey	1,089	152	51	33.6
Muker, Yorks.	1,321	362	111	28.9
Hambledon, Bucks.	1,840	177	38	21.5
Iver, Bucks.	1,985	214	53	24.8
Langley-Marish, Bucks.	1,874	222	81	36.5
Fowey, Cornwall	1,606	208	47	22.6
Halberton, Devon	1,745	252	71	28.2
Chigwell, Essex	1,965	168	72	42.9
Wheathampstead, Herts.	1,908	349	138	39.5
Benenden, Kent	1,608	391	130	33.3
Old Malton, Yorks.	1,505	151	38	25.1
Lanercost, Cumb.	1,574	251	68	27.1
Eton, Bucks.	3,666	354	85	24.0
Colyton, Devon	2,504	460	95	20.6
Hartland, Devon	2,183	575	115	20.1
Fordingbridge, Hants.	3,096	729	209	28.7
Ringwood, Hants.	3,928	834	362	43.4
Hadlow, Kent	2,395	430	99	22.8
Speldhurst, Kent	2,839	326	108	32.1
Hackney, Middlesex	30,372	595	345	58.0
Kingston, Surrey	10,622	952	475	49.9
Putney, Surrey	2,845	96	15	15.6

The most important conclusion to emerge from Table 6 is that for some parishes baptism was a rather unreliable method of birth registration – for example, those with over 40% of N.I.R. cases: Black-Torrington, Chigwell, Ringwood, Hackney and Kingston; whereas for others it is strikingly good – for example, those with under 10% of N.I.R.s: Acomb, Bretforton and Medmenham. Most parishes cluster around the overall average of 31.0%. However, there are some significant differences between parishes of different sizes, as will be seen in Table 7.

Table 7. Distribution of N.I.R.s by Size of Parish (45 parishes). Population Sizes of Parishes, 1851

	Under 500 (9 parishes)	*500–999 (7 parishes)*	*1,000–1,499 (9 parishes)*	*1,500–1,999 (10 parishes)*	*2,000 + (10 parishes)*	*Total Sample*
N.I.R. Cases	107	96	556	736	1,908	3,403
Total Cases	579	638	2,003	2,383	5,351	10,954
% N.I.R.s	18.5	15.0	27.7	30.9	35.7	31.0

The larger the parish, the larger the proportion of N.I.R.cases – this is no great surprise and would be expected on commonsense grounds. It is possible that the association between larger parishes and religious nonconformity accounts for part of the correlation between parish size and poor baptism registration. To check the influence of religious dissent on the adequacy of Anglican baptism as a form of birth registration, it is possible to analyse the list of non-parochial registers which were collected by the government in 1838 and 1857.[18]

Nine of the 45 parishes in the main comparison sample were also in the list of parishes possessing non-parochial registers: Hackney, Fordingbridge, Hambledon, Fowey, Colyton, Hartland, Chigwell, Ringwood and Kingston. The proportion of N.I.R.s in these nine parishes is 35.3% compared to the 27.6% of N.I.R.s in the rest of the sample for which no non-parochial registers have survived. The relative similarity of

[18] An attempt was made by the government in 1838 to collect all surviving non-parochial registers for legal purposes and a second attempt in 1857 added virtually no additional registers, which suggests that most of the surviving registers had been collected in 1838. See *Report On Non-Parochial Registers* (Parliamentary Papers, 1838, XXVIII); *Report On Non-Parochial Registers*, (Parl. Pap., 1857, XXIII).

N.I.R. proportions in the two types of parish, suggests that religious nonconformity played a minor role in the non-registration of births through baptism.[19] We will see later, that the registration process itself was probably responsible for most under-registration, and this was probably linked to size of parish.

The most interesting variation in registration accuracy is that occurring over time, as this will throw light on the difficult question of changes in the adequacy of baptism as a form of birth registration.. Table 8 summarizes changes in proportions of N.I.R.s in time periods calculated from age in the 1851 Census.

Table 8. Distribution of N.I.R.s Over Time (45 parishes)

	1761–70	*1771–80*	*1781–90*	*1791-1800*	*1801-10*	*1811–20*	*1821–30*	*1831–34*
N.I.R.s	22	97	208	379	486	656	936	619
Total Cases	68	347	637	1,053	1,517	1,989	3,092	2,251
% N.I.R.s	32.4	27.9	32.6	36.0	32.0	33.0	30.0	27.4

Leaving aside the doubtful figure for the period 1761–70,[20] the overall trend would appear to be one of deteriorating accuracy between 1771 and 1800 and a trend towards improving reliability from 1801 onwards. The changes in the proportion of N.I.R.s from any one decade to another are not great and at no point suggest a collapse in registration reliability as suggested by Krause for the period 1795 to 1820. The trend of deterioration followed by improvement is in line with his analysis, although the degree of change is certainly not of the order suggested by him.[21] This relative lack of deterioration may partly be a function of the rural nature of the sample of 45 parishes. In order to explore this further, a special analysis of the larger parishes in the sample was carried out.

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[19] A comparison between degree of under-registration estimated on proportions of N.I.R.s and the number of entries in the non-parochial registers for Hambledon, Fowey and Chigwell suggests that only 11.5% of all unregistered cases (estimated from N.I.R. figures) were due to non-parochial registration.

[20] The earlier discussion of the accuracy of the census statements of age of this group would suggest that the correct figure for the percentage of N.I.R.s is about 25%.

[21] See Krause, *op. cit.*

Table 9. Distribution of N.I.R.s Over Time

		1761–80	*1781–90*	*1791-1800*	*1801-10*	*18II–20*	*1821–30*	*1831–34*
	N.I.R.s	8	12	11	9	13	28	14
Colyton	*Total Cases*	25	35	41	51	75	141	92
	% N.I.R.s	32.0	34.3	26.8	17.6	17.3	19.9	15.2
	N.I.R.s	5	10	15	14	9	30	32
Hartland	*Total Cases*	35	39	54	87	84	154	122
	% N.I.R.s	14.3	25.6	27.8	16.1	10.7	19.5	26.2
	N.I.R.s	13	18	24	25	33	44	52
Fordingbridge	*Total Cases*	39	47	66	96	129	187	165
	% N.I.R.s	33.3	38.3	36.4	26.0	25.6	23.5	31.5
	N.I.R.s	12	27	42	38	60	115	68
Ringwood	*Total Cases*	36	59	80	110	135	245	169
	% N.I.R.s	33.3	45.8	52.5	34.5	44.4	46.9	40.2
	N.I.R.s	13	31	76	92	133	—	—
Hackney	*Total Cases*	17	44	106	183	245	—	—
	% N.I.R.s	76.5	70.5	71.7	50.3	54.3	—	—
	N.I.R.s	8	22	44	44	67	189	101
Kingston	*Total Cases*	18	41	73	97	130	359	234
	% N.I.R.s	44.4	53.7	60.3	45.4	51.5	52.6	43.2
Colyton, Hartland, Fordingbridge, Ringwood, Hackney, Kingston	*N.I.R.s*	59	120	212	222	315	406	267
	Total Cases	170	265	420	624	798	1,086	782
	% N.I.R.s	34.7	45.5	50.5	35.7	39.5	37.4	34.3
16 Parishes With Population Less Than 1,000	*N.I.R.s*	5	11	14	32	46	55	24
	Total cases	34	74	103	151	238	346	262
	% N.I.R.s	11.7	14.9	13.6	21.2	19.3	15.9	9.2

Table 9 does not suggest that the pattern for larger or for very small parishes was very different from that for the overall sample, and the relatively unchanging reliability of the baptismal registers raises a number of important problems. Krause assumed that the rapid growth of religious nonconformity at the end of the eighteenth century resulted in a major deterioration in the value of Anglican parish records as a source of registration. An examination of the non-parochial registers lodged in the

Public Record Office suggests, however, that only a minority of nonconformist congregations registered their own births and baptisms – by the end of the period 1839 non-parochial entries represented only about 9% of the equivalent Anglican baptism entries.[22] There are several other factors which may have influenced the reliability of Anglican registration, but these need not concern us here; the important point is that there is no known reason to doubt the findings about variations in registration accuracy based on the figures for N.I.R. proportions. In fact, as we shall see later, some independent evidence exists to suggest that the N.I.R. proportions are good indicators even of the absolute level of under-registration.

The above tentative conclusion about the relatively unchanging reliability of parish registers as a source of demographic information applies, of course, only to the aggregate figures for groups of parishes together. For the historical demographer interested in a particular parish it is more important to know whether the register for that parish is reliable as a form of registration over time. Only parishes with relatively large populations are suitable for this kind of detailed analysis and Table 9 summarizes the relevant information for all such parishes. There is a considerable amount of variation between the parishes levels of registration accuracy over time. Colyton and Hackney show a more or less progressive and consistent improvement, Fordingbridge a tendency to improvement, and Hartland, Ringwood and Kingston a fluctuating pattern not unlike that of the total sample of 45 parishes. Both Colyton and Hartland were included in the sample because they were the first two parish registers to be used for the family reconstitution method by E. A. Wrigley.

In Table 9, the number of cases in some of the decennial groupings is too small for firm generalization, although an increase in numbers could probability be achieved in a special study of a particular parish by including married women in the sample. The figures do, however, hold serious implications for family reconstitution work; for example, the significant increase in fertility which Wrigley believed he had found in Colyton at the end of the eighteenth and the beginning of the nineteenth centuries could

[22] See P. E. Razzell, *The Role Of Smallpox Inoculation In The Growth Of Population In Eighteenth-Century Britain* (Oxford University D.Phil. Thesis, 1965), pp. 11–15, for a general discussion of the non-parochial evidence and the basis of the estimated 9% figure. There is evidence to suggest that some nonconformist baptisms were not registered in any way.

be an artefact of the improvement in registration during this period in the parish. [(1994): The same-name technique (considered in Chapter 7), suggests a very similar pattern of changing parish registration accuracy in Colyton. The proportion of N.I.R.s in Colyton was 31.0% in 1761–1800, falling to 17.9% in 1801–34; the proportion of burials unregistered according to the same-name technique was 36.2% in 1751–1800 and 16.5% in 1801–1837.]

The improvement in registration would have less effect on the accuracy of the results of family reconstitution if all unregistered cases tended to occur in particular families; these families could then be excluded, in extreme cases, from the reconstitution sample, and thus the final results would not be materially affected. Evidence from the census/parish register comparison suggests, however, that N.I.R.s were distributed more or less randomly amongst all families: of a sample of families of which at least one member was a N.I.R. (a total of 3,600 cases) 27.8% were not found in the baptismal register,[23] a proportion of N.I.R.s which is lower than that for the total sample, 31.0%.

Table 9 also indicates that a London parish register such as that for Hackney for example, would be quite unsuitable for family reconstitution, or, indeed, for any demographic work. The same might be said of Kingston-on-Thames and Ringwood, since there were decades during which the majority of cases were not being registered through baptism. The findings for Hackney are of particular interest for some of the historical conclusions which have been reached on the basis of parish register data about London's demographic experience during the eighteenth century. Dorothy George, in her work on the eighteenth-century demographic history of London, quotes extensive statistics derived from parish register returns, perhaps the best-known example being that "for the twenty years from 1730 to 1749 the burials of children under five were ... 74.5% of all the children christened".[24] This, of course, was the famous 'gin-period', but the evidence from the census/ parish register comparison for Hackney (which was a part of the London Bills of Mortality area) suggests that such extreme

[23] This sample of families was selected about one-third way through the project and included 30 parishes; only families with at least two members qualifying for the census/register comparison were included in the sample.

[24] M. Dorothy George, *London Life In The Eighteenth Century* (1966), pp. 39, 399. See also her article in *The Economic Journal*, Vol. 32 (1922), pp. 325–352.

levels of child mortality were a function of under-registration of births – the proportion of N.I.R.s for the eighteenth-century period was over 70%. The mortality ratio quoted by George depends on the assumption that baptisms and burials were an equally reliable method of registering births and deaths respectively, for the ratio is simply the number of baptisms at a particular time divided by the number of burials during the same period. If baptisms and burials were equally defective as forms of registration, errors would cancel out, but it is unlikely that some 70% of all deaths were not registered through burial statistics.[25] It would be very premature to regard Hackney as typical of all London, but the census/register comparison for this parish does raise fundamental questions about statistics which have been widely quoted by historians and others as indicating the demographic and social conditions of the period.

Enough has been said to illustrate the value of the census/parish register comparison method in assessing the reliability of register information for particular places; we must now return to the weaknesses and problems associated with the method. It was anticipated that the main problem would lie in the accuracy of the 1851 Census for the larger parishes, and so an 1851/1861 census comparison check was made for individuals in the census/register sample who were living in parishes with populations over 1,000.[26] Table 10 gives the proportion of birthplace statement disagreements for sample individuals in the larger parishes, as well as the N.I.R.s for purposes of comparison.

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[25] A special survey of unregistered burials in London was carried out by the Collector of the Tax on Burials in the last six months of 1794; this survey showed that 3,148 persons were interred without being registered by the Anglican Church. As there were 20,537 Anglican burials in the year 1794, unregistered burials formed about one-quarter of the total for that year. See the *1811 Parish Register Abstract*, p. 200, and George, *op. cit.*, p. 398.

[26] Parishes with smaller populations were excluded because of lack of time.

Table 10. 1851/1861 Census Comparisons
(B.S.D. = Birthplace Statement Disagreement; N.I.R. = Not In Register.)

	B.S.D.s	*Total Cases*	*Percentage Of Birthplace Statement Disagreements*	*% N.I.R.s*
Black Torrington	0	43	0.0	44.0
Hemyock	1	31	3.2	11.8
Chardstock	4	52	7.7	37.1
Great Clacton	0	48	0.0	18.6
Purleigh	2	16	12.5	22.8
Danbury	0	50	0.0	21.3
Chiddingstone	1	36	2.8	25.9
Tooting-Graveney	0	22	0.0	33.6
Muker	0	89	0.0	28 9
Hambledon	1	37	2.7	21.5
Iver	3	42	7.1	24.8
Langley-Marish	3	46	6.5	36.5
Fowey	1	41	2.4	22.6
Halberton	3	44	6.8	28.2
Chigwell	2	39	5.1	42.9
Wheathampstead	2	80	2.5	39 5
Benenden	4	69	5.8	33.3
Old Malton	0	31	0.0	25.1
Lanercost	0	52	0.0	27.1
Eton	1	37	2.7	24.0
Colyton	0	63	0.0	20.6
Hartland	0	71	0.0	20.1
Fordingbridge	1	133	0.8	28.7
Ringwood	4	157	2.5	43.4
Hadlow	2	92	2.2	22.8
Speldhurst	4	55	7.3	32.1
Hackney	8	26	30.8	58.0
Kingston	1	71	1.4	49.9
Putney	0	12	0.0	15.6
Total	48	1,585	3.0	

The table shows that there is little correlation between the proportions of N.I.R.s and B.S.D.s – the 14 parishes with the highest percentages of N.I.Rs (28.2% and above) had an average of 3.9% of B.S.D.s compared with 3.0% B.S.D.s for the whole sample. This latter figure is even smaller

than the proportion of birthplace disagreements for natives in the initial sample of five parishes given in Table 2 (5.3%), and suggests that the 1851 Census was highly accurate in respect of birthplace statements for natives. The superiority of information for natives was also confirmed by analysing data on those individuals who acted as criterion cases for the main sample (women and children in the 'native' group, men being included in the sample). B.S.D.s amounted to 6.5% of 476 native criterion cases as against 14.3% of 357 non-native ones.

Although the high figure for Hackney (30.8% B.S.D.s) is based on the very small sample of 26 cases, it does suggest the possibility of serious census inaccuracy, and in this case, it was impossible to extend the sample in the time available. Census comparison work on large urban areas is extremely laborious. However, information on criterion cases and others proved amenable to analysis.[27] Of 46 of these extra 'native' cases, only 2 were B.S.D.s; of 31 non-native cases, 13 were B.S.D.s. There is little doubt that Hackney was rather like Bethnal Green in its high proportion of birthplace statement disagreements, and the percentage of cases in the latter parish, where the sample was much larger was 11.4% for 'natives.' This figure is very similar to the 13.9% of a total of 72 for all the 'natives' in Hackney who were checked through census comparison. Although this is a relatively high figure, it would only account for a fraction of the very high proportion of N.I.R.s in Hackney. It is likely that the Hackney N.I.R. ratios would be similar to those of Kingston if allowance were made for the higher level of B.S.D.s and the main conclusions drawn from these N.I.R. ratios would not have to be altered.

The fact that only 8.8% of all N.I.R. cases in the census comparison sample were B.S.D.s (36 out of a total of 409) suggests that the overall effect of census inaccuracies on proportions of N.I.R.s is small. Although this proportion is significantly higher than that for matched cases found in the baptism register – 1.0% (12 out of 1,176) – it is still a very small minority of all N.I.R. cases and therefore can only explain a fraction of them.

There is some evidence of a slight correlation between age and the proportion of B.S.D.s, which could distort the accuracy of the frequency

[27] Many individuals who could normally have been included in the main sample but for the fact that the Hackney parish register finished in 1820.

of N.I.R.s over time. However, as will be seen from Table 11, only the 17–20 age group is affected to any appreciable extent.

Table 11. Age as Given in the 1851 Census, Compared to Birthplace Statement Disagreements

	17–20	*21–30*	*31–40*	*41–50*	*51–60*	*61–90*	*Total*
B.S.D.s	11	13	17	9	6	2	48
Total Cases	128	420	400	338	214	90	1,585
% B.S.D.s	0.8	3.1	4.2	2.7	2.8	2.3	3.0

Table 11 should be read in conjunction with Table 8: the proportion of N.I.R.s for the 1831–34 group (this is the same group as the 17–20 age group in the table above) would be about 30% rather than the 27.4% in that table, if it were assumed that the whole of the lower-than-average B.S.D. percentage (2.2%) was due to inaccuracy of the 1851 Census. Other than this slight modification – which is based on an extreme assumption – Table 11 does not suggest any other significant changes to Table 8.

I have assumed that the census comparison method is an adequate check on the reliability of census information, on the general grounds that two independent censuses will yield information on errors in either of the two censuses. However, two fundamental objections may be made against this assumption: (i) that the enumerators were the same in the 1851 and 1861 censuses; (ii) that the individuals enumerated repeated erroneous information in the two censuses. It is possible to check the first factor directly by examining the relationship between enumerator continuity and the percentage of B.S.D.s in the different parishes, as well as by calculating the percentage of all enumerators who acted in both censuses. When this is done no correlation is found between enumerator continuity and the proportion of birthplace statement disagreements, a conclusion further borne out by the fact that only 23.1% of the total number of enumerators (113) acted in both censuses. The problem of erroneous information being given in both censuses is rather more difficult to deal with. The only way of evaluating the scale of this problem is by checking the 1851 Census/parish register age discrepancies against age statement disagreements of the 1851/61 census comparison. If the census comparison method is a reliable method for measuring census errors, it should be possible to trace errors for individuals to

discrepancies between the 1851 Census and the parish register. Table 12 summarizes the relevant data for this checking process.

Table 12. Distribution of 1851 Census/Baptism Register Age Discrepancies and 1851/61 Census Age Disagreements for Individuals in Parishes With Populations Greater Than 1,000

		1851/61 Census/Age Disagreements							
		0	*1*	*2*	*3*	*4*	*5*	*6–15*	*Total*
1851 Census/ Register Age Discrepancies	*0*	420	161	36	13	6	3	3	642
	1	165	137	40	9	5	4	9	369
	2	26	36	17	9	5	3	1	97
	3	12	11	2	9	2	3	1	40
	4	5	2	1	2	0	0	1	11
	5	2	2	1	1	1	1	1	9
	6–15	4	1	1	0	0	0	2	8
	Total	634	350	98	43	19	14	16	1,176

This table needs careful interpretation before it can yield the information we need. The number of cases with large age disagreements is very small, but it is possible to reach certain very tentative conclusions. We want to know from the table the proportion of genuine age errors which are picked up by the census comparison method. Not all the larger census/register age discrepancies can be assumed to be genuine errors, as most of the group with differences of 6–15 years, for example, are probably incorrectly identified individuals (see Table 3). The group with a discrepancy of three years or less between the census and the register provides the most useful test of the value of the census comparison in discovering age errors, as Table 3 indicates that only about 10% of these cases are incorrect identifications. Only 12 of the 40 cases in this group were perfect fits in the census comparison (30.0%) as against 420 of the 642 cases in the census/register fit category (65.4%). Some of the perfect fits in this group may be due to a possible delay of about three years between birth and baptism, so that in these cases the census fit would still be correct in spite of the census /register discrepancy. It is impossible to determine the exact proportion of age discrepancies that can be accounted for by the census comparison method; however, Table 12 does indicate that the method is capable of detecting a very significant proportion.

At the beginning of this paper it was suggested that two main factors might influence the representativeness of the census population. First, variations in the delay between birth and baptism,[28] and the effect of shifts in infant mortality on the proportion of babies baptized. Second, possible correlations between social class and both pre-census mortality and registration reliability. Reliable data on infant mortality in the pre-civil registration period does not exist,[29] but infant mortality would only affect variations in registration accuracy if it accompanied variations in the delay between birth and baptism (the effect of infant mortality on absolute levels of registration is discussed below). A number of registers used in the present study do give some information on dates of both birth and baptism, allowing a tentative analysis in changes in the interval between birth and baptism over time.

Table 13. Interval Between Birth and Baptism Over Time (45 parishes)

	Birth Date (From Age Statements in 1851 Census)						
	1761–80	*1781–90*	*1791–1800*	*1801–10*	*1811–20*	*1821–30*	*1831–34*
Total Cases	38	82	148	268	215	307	234
Approximate Medians (in weeks)	3.5	3	4	4	3	6	6
Means (in months)	6.4	2.3	3.8	3.9	2.4	2.6	3.9

These figures show no discernable trend, although the median interval between birth and baptism shows a tendency to lengthen slightly at the end of the period. These figures do not suggest that we should seriously question the representativeness of the census on the basis of these data.

The other major factor which might disturb the representativeness of the census – social class – can also be assessed. The relationship between social status (as measured by the presence of servants in the household) and the proportion of N.I.R.s can be examined. Of the servant-keeping

[28] The information in Table 13 indicates that there was no significant overall change in interval between birth and baptism during 1761–1834. This conclusion is confirmed for 'median' parishes in the period 1771–1812 by Berry and Schofield. Their evidence suggests, however, significant variations between individual parishes. See B. Midi Berry and R. Schofield, "Age at baptism in pre-industrial England", *Population Studies*, Vol. 25, (1971), pp. 453–464.

[29] (1994): See a discussion of this issue in Chapter 7

class, 32% were N.I.R.s (382 out of 1,194), as against 30.8% of the rest of the sample without servants (3,004 out of 9,760). This absence of a significant correlation between social status and registration reliability is confirmed by the evidence for occupational class and proportions of N.I.R.s. Table 14 indicates that there was some significant variation between the different occupational groups and registration accuracy, but there is no linear relationship between the social status of the occupational group and the proportion of N.I.R.s.

Table 14. Distribution of N.I.R.s by Occupational Group (35 parishes)[30]

	Occupational Group						
	Professional	*Farmers*	*Shopkeepers*	*Pensioners*	*Servants*	*Labourers*	*Paupers*
N.I.R.s	49	194	789	16	88	1,282	25
Total Cases	118	724	2,454	67	244	4,207	61
% N.I.R.s	42.2	26.8	32.2	23.9	36.1	30.5	41.0

There is no obvious pattern in this table: a high-status group like the professionals has the highest N.I.R. ratio, but other high-status groups like farmers and shopkeepers have relatively low ratios. The lack of a correlation between occupational status and N.I.R. ratios suggests that social class did not distort the census/parish register comparison.

For much demographic work it is necessary to estimate the absolute levels of under-registration. Because of the exclusion of young infants who died before baptism, N.I.R. proportions are not a complete measure of non-registered births. It is possible to estimate the scale of this problem by examining the interval between birth and baptism in the light of the Registrar-General's evidence about infant mortality during the early period of civil registration. Table 15 gives the distribution of intervals between birth and baptism for all 1,292 cases in the sample for which the relevant information was available.

[30] The parishes of Bramfield, Cocking, Acomb, Lapford, Chipstable, Wylye and Bretforton are not included in this sample since the appropriate information was not collected at the early stage of the research. The names of the occupational groups indicate the occupations included in each group (where there was some indication of a pauper's previous occupation he was classified under that heading).

Table 15: Distribution of Intervals Between Birth and Baptism for the Total Census/Baptism Register Sample

Delay	*0–6 days*	*7–13 days*	*14–20 days*	*21–27 days*	*28 days to 2 months*	*2–3 months*	*3–6 months*	*6 months to 1 year*	*1 year +*
Number of Cases	112	54	159	313	341	95	99	52	67

The table shows that the median interval between birth and baptism was about one month and about 75% of the sample had been baptized within two months. According to the Registrar-General's figures for 1839–1844, just after the introduction of civil registration, approximately 4.3% of all children born in England and Wales died within the first month of life, which was the median interval between birth and baptism for the whole census/register sample.[31] It cannot be assumed that this percentage was typical of similar infant mortality during the whole period 1760–1834, and there is some evidence that a figure of about 5% would be a low estimate of infant mortality for the median interval of one month.[32]

It cannot be assumed that all infants dying in the first month after birth were not registered through baptism, since the Church of England made special provision in cases of "great cause and necessity" for the private baptism of sickly infants. However, private baptisms were often not entered in the baptismal register;[33] although Rickman claimed that the

[31] For the numbers of infants dying at various times within the first year of life, see the *Registrar-General's Eighth Annual Report* (1847/48), p. 282. For the number of births during the period 1839–44, see B. R. Mitchell and P. Deane, *Abstract Of British Historical Statistics* (1962), p. 29. I have inflated Mitchell and Deane's number of births by 8%, which is the approximate inflation ratio suggested by Glass on the basis of comparing census and civil registration data. See D. V. Glass, "A note on the under-registration of births in Britain in the nineteenth century", *Population Studies*, Vol. 5, (1951), pp. 70–88.

[32] (1994): See the discussion of infant mortality in Chapter 7.

[33] For example, in the period before 1811, see *B.M. Add. MSS 6896*, folios 7, 8, 11, 29 and 80. Rickman writing in the report on the 1801 Census observed that those "who are privately baptised are not always registered. The practice of the Clergy is not uniform on this point ..." ("Observations on the results of the Population Act, 41 Geo. 3", *1801 Census, Parish Registers*). Examples of registered private baptisms are to be found in the Kelsale, Suffolk register for the period 1801–12, and the Wylye, Wilts., register for 1813–20. See Krause, in Glass and Eversley, *op. cit.*, p. 391.

1812 Parish Register Act had virtually ensured the registration of all private baptisms by making this compulsory by law.[34] This might mean that there was a relative increase in registration accuracy after 1812, not measured by the N.I.R. statistics. Some infant private baptisms are known to have been registered before 1812 and not all neo-natal deaths would have been registered through private baptism after 1812. Given that the N.I.R. figures suggest that about one-third of all births were not registered through baptism during 1760–1834, the unmeasured element of neo-natal mortality would not greatly affect the N.I.R. figures as indices of absolute levels of registration accuracy, even before 1812 (one-third of the 5% would in any case be accounted for by 'normal' under-registration).

The validity of the N.I.R. figures as an index of absolute registration inaccuracy in the country as a whole can be checked roughly for the 1830s, since alternative means of estimating registration accuracy became available. Glass has estimated that at least 24.8% of all births in England and Wales during 1831–37 went unregistered through Anglican baptism. He arrived at this estimate by comparing national baptism and civil registration data with census/civil registration information.[35] This independent estimate of 24.8% is close to the N.I.R. proportion of 27.4% for the 1831–34 age group in the census/registration sample. The fact that the N.I.R. sample used in this paper consists of only 45 parishes selected according to the availability of registers means that generalizations based on this sample must be treated as hypotheses to be tested by an application of the comparison method to a larger and more randomly selected sample. Nevertheless, the similarity between Glass's estimate of registration inadequacy and that derived from the N.I.R. proportions does suggest that the latter can be used tentatively as an index of absolute registration inaccuracy in the country as a whole.

One obvious drawback of the census/register comparison procedure is that because it takes its starting point from the 1851 Census, it is only applicable to people born in the 1760s at the earliest. Much of the controversy about population change during the period of the industrial revolution concerns changes which occurred from the early eighteenth century onwards. It is, however, possible to apply the method outlined in

[34] See "Preliminary Observations", *1821 Census*, p. xxvii, and *Parish Register Abstract, 1831 Census*, p. xiv.

[35] Glass and Eversley, *op. cit.*, p. 234.

this paper to data for this earlier period. The marriage licence information for the Archdeaconry of Chichester area of Sussex contains detailed information on the period of residence in the parish of those marrying after 1760,[36] and I have carried out an exploratory study of a number of cases where the parties were stated as having resided in a particular parish 'all their lifetime'.

Thirteen parishes with an average population of about 1,200 in 1851 were selected for analysis,[37] and 60 people marrying in the decade 1758–1768 were compared with 60 others marrying in the period 1795–1800. The mean age at marriage of the first sample was about 25 years and that of the second 24 years, indicating from the stated age of marriage on the marriage licence, that the first group of births centred around 1738 and the second around the years 1773–74. In both groups it was possible to locate 48 of the 60 cases in the appropriate baptism register within five years either side of the expected birth date (this taken from the stated age of marriage on the marriage licence). Thus, exactly 20% in both groups may be considered as being the equivalent of N.I.R.s, a figure which, in spite of the small numbers and the different source of information about name, age and birthplace, is very similar to that of comparable groups in the census/register sample.The most important point about the similar proportions of married people being found in the baptism registers, is that it does not suggest any significant change in registration accuracy for Sussex parishes from the 1730s to the 1770s. It is evidently necessary to extend this type of work before any firm conclusions can be reached. Information similar to that found in these Sussex marriage licences may be available for other areas and possibly for even earlier periods.

Why were there so many missing baptisms in the period covering the 1730s to the 1830s? One of the main factors was the separation of the acts of baptism and burial from the actual registration process itself. The law stipulated that "the parson, vicar, or curat ... shall every Sonday take furthe [the register], and in the presence of the said [church] wardens, or one of

[36] See D. Macleod (ed.), *Calendar Of Sussex Marriage Licences* (Sussex Record Society, XXXII, 1926, and XXXV, 1929).

[37] The parishes with their 1851 populations are as follows: Petworth, 3,439; Pulborough, 1,825; Yapton, 609; Boxgrove, 755; Mid-Lavant, 284; East Dean, 419; Midhurst, 1,481; Eastbourne, 1,076; Funtington, 1,079; Bosham, 1,126; Westbourne, 2,178; Walberton, 578; Aldingbourn, 744.

them, write and recorde in the same all the weddings, christenyns, and burialles, made the hole weeke before ..."[38] Although Rose's Act of 1812 modified this provision it did so only to the extent of requiring registration to take place within seven days of the event itself. Needless to say, the law was frequently ignored. The week-to-week business of registration tended to be left in the hands of the parish clerks, some of whom kept their own records which were copied by the incumbent into the parish register sometimes as infrequently as once a year.[39] The effect of handing over registration to parish clerks was discussed by Burn in his study of parish registers, first published in 1829:

> The custody of Parish Registers having been frequently committed to ignorant parish clerks, who had no idea of their utility beyond their being occasionally the means of putting a shilling into their pockets for furnishing extracts, and at other times being under the superintendence of an incumbent, either forgetful, careless or negligent, the result has necessarily been, that many Registers are miserably defective, some having the appearance of being kept from month to month, and year to year, yet being deficient of a great many entries ...[40]

A correspondent of the *Gentleman's Magazine* in 1805 had reached the same conclusion:

> Our Parochial Registers are in many instances now kept by Parish-Clerks, and as these Record-keepers derive no profit from the employment, except a casual shilling now and then for a search, it may be imagined what sort of Record is kept, where ignorance and negligence are united.[41]

Burn quoted numerous examples of the negligence of parish clerks, the one most illuminating for our present purposes being the

> parish clerk [who] said it was usual for him, and not the clergyman, to take an account of Burials, and he entered them in a little sixpenny memorandum book, thus: 'A.B. 1s.' If the fee was paid at the time,

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[38] J. S. Burn, *The History Of Parish Registers In England* (1862), p. 18.
[39] *Ibid*, p. 261.
[40] *Ibid*, p.40.
[41] *The Gentleman's Magazine*, Vol. 75, No. 22 (1805), p. 1117.

> no name would be put into his book, he only booked what was due to him and the clergyman entered in the Register at the end of the year from his Memorandum Book.[42]

Much under-registration was clearly directly due to negligence by the incumbent. The *Gentleman's Magazine* remarked in 1811 that "the clergyman (in many country places) has entered the names at his leisure, whenever he had nothing better to do, and perhaps has never entered them at all."[43] Examples of such negligence can be found from the beginning of parish registration: in 1567 the incumbent of Tunstall, Kent, appeared to have tired of registering the Pottman family because of its concentration in the parish and simply stated in the register: "From henceforwd I omitt the Pottmans".[44] That this kind of negligence continued thereafter is indicated by the fact that in 1702–03 "a committee of Convocation drew up a list of ecclesiastical offences notoriously requiring remedy, in which irregularity in keeping registers is prominent in the list of gravamina".[45] Rickman gave one reason for under-registration of both births and deaths: "negligence may be supposed to cause some omissions in the Registers, especially in those of small Benefices, where the Officiating Minister is not resident".[46] Perhaps an extreme example of such irregularity in the period of greatest relevance to this paper is provided by the attempt by Charles Reynolds to 'prove' that William Fogden was baptized in the parish of Appledram, Sussex, for marriage licence purposes. At the end of 1822 Reynolds stated:

> that he hath this Day applied to the Clerk of the Parish of Appledram in the County of Sussex in which Parish this Deponent was informed the said William Fogden was born for the Register of Baptisms within that Parish when the Clerk said he did not know where it was that he then went to the House in the City of Chichester where the Reverend Mr Broadwood the Minister of the said Parish of Appledram resided to ask him for the said Register where he was informed

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[42] Burn, *op. cit.*, p. 44. This paragraph is quoted at greater length in Chapter 8 of the present book.

[43] *Ibid*, p. 42

[44] *Ibid*, p. 41.

[45] W. E. Tate, *The Parish Chest* (1969) p. 49.

[46] *Preliminary Observations, Population Returns 1811* (Parl. Pap. 1812, XI), p. xx.

the said Mr Broadwood was absent from Chichester And this Deponent saith that he hath been informed that the Churchwarden of Appledram, if there is any, is gone away from the said Parish.[47]

It was not only negligence that lead to under-registration. One Northamptonshire incumbent indirectly indicated that the clergy were not above the practice of refusing to register cases for which fees were not paid: "The very true reason why this [Brington] register, is found as imperfect in some years as from 1669 to 1695 is because the parishioners could never be persuaded to take to see it done, nor the church-wardens as ye canon did require, and because they refuse to pay such dues to ye curate as they ought by custome to have payed."[48] Enough has been said on this subject to indicate to the reader probable reasons for the large-scale under-registration of both births and deaths.[49]

What are the implications of the tentative conclusions reached in this paper about the adequacy of Anglican registration ? If the statistics of national baptisms collected in connection with the 1801–1841 Censuses are inflated by the appropriate N.I.R. ratios, approximate birth rates may be computed for decennial periods between the first five population censuses (Table 16).

Table 16. Estimated Birth Rates for England and Wales, 1801–1841[50]

	1801–10	*1811–20*	*1821–30*	*1831–40*
Birth Rate Per 1,000 Population	41.4	42.0	40.1	35.9

From a high birth rate in the 1800s there appears to have been a fall to a rate in the 1830s which was very similar to that computed by Glass

[47] "Marriage licences and affidavits", *Document EP. 1 19* (West Sussex Record Office).

[48] J. C. Cox, *The Parish Registers Of England* (1910), pp. 20, 21.

[49] For a very general source of further evidence on this subject, see the evidence taken before the Select Committee in 1833 to he found in *Parl. Pap. 1833, XIV.*

[50] I have used the adjusted population figures computed by Krause from official returns, by making certain assumptions about early census under-enumeration. See J. T. Krause, "Changes in English fertility and mortality, 1781–1850", *Economic History Review,* Vol. 11 (1958–59), p. 60. For numbers of baptisms, see Mitchell and Deane, *op. cit.*, p. 28. The population figures for each period were arrived at by taking an average of the beginning and end of a decade.

independently for the same decade – and, according to further estimates by Glass, the birth rate remained at this level for a further three decades.[51]

Unfortunately there is no known similar method for analysing changes in the adequacy of Anglican burial as a form of death registration,[52] although it should be possible to extend Glass's work on registration accuracy in London at the end of the seventeenth century to many parts of the country. Where reliable population figures are available and there is insignificant net migration (inward – outwards) it is possible, however, to calculate burial/death ratios when firm estimates of the total number of births are at hand (where migration is zero, net population increase = births – deaths). Using the number of births calculated for Table 16, we arrive at the following figures:

Table 17. Estimated Proportions of Deaths Omitted From Anglican Burial Registers, England and Wales, 1801–1841[53]

	Percentage of Omitted Deaths	
Period	*On Official Population Returns*	*On Krause's Population Estimates*
1801–10	34.1	37.5
1811–20	38.8	42.4
1821–30	28.3	27.9
1831–40	15.7	15.2

Two sets of population figures have been used to prepare this Table: the uncorrected official figure derived directly from the early nineteenth-century censuses, and 'corrected' estimates made by Krause, which assume that the 1801 Census was deficient by 5%, the 1811 Census by 2% and the remaining three censuses (1821, 1831 and 1841) by 1%. In fact, these assumed corrections do not materially affect the overall trend of burial registration accuracy, which suggests a high level of

[51] Glass and Eversley, *op. cit.*, p. 240.

[52] (1994): But see the new work on checking the accuracy of burial registration discussed in Chapters 7 and 8.

[53] The evidence on net migration is scanty but does not suggest that it would significantly affect the figures in this table. See Krause, "Changes in English fertility ...", *op. cit.*, p. 60; and for numbers of burials, see Mitchell and Deane, *op. cit.*, p. 28.

omissions for 1801–1820, followed by a dramatic improvement in the 1820s and 1830s. It is possible to check the estimate for the 1830s by comparing it with that made by Glass – who compared Anglican burial returns against civil registered deaths for the late 1830s[54] – which was 18.7% as against 15.7% and 15.2% in Table 17.[55] The methods of estimating burial deficiency in Table 17 was entirely independent Glass's, using different techniques and sources of information. The similarity of the results for the 1830s is noticeable and certainly lends support to the value of the procedure that I used in preparing Table 17. The percentages for the first two decades of the nineteenth century are surprisingly high, although Krause has consistently argued that Anglican registration improved after 1821.[56] The difference between the earlier and later periods is partly due to the fact that the published returns of burials for the 1800s and 1810s do not appear to have included any significant number of non-parochial returns, whereas the 1820s included at least 126,000 non-parochial burials of religious dissenters and others, and the 1830s a minimum of 156,000.[57] These figures only represent, however, about 3% and 4.5% respectively of the estimated total number of deaths during these decades, and so would not alter significantly the trend of burial/death registration from the beginning of the century.

The high level of under-registration of deaths during the first two decades of the nineteenth century raises the problem of how such an objectively observable event as death, which by all accounts invariably resulted in Anglican burial, escaped registration through the parish burial registers. There is, of course, evidence of the existence of large-scale non-parochial burial grounds in the bigger towns, and Krause has drawn attention to some absurdly low official burial rates for the first two or three decades of the nineteenth century in these same places.[58]

Large towns, however, only contained a small minority of the total population in the early nineteenth century, and London, which was by far the largest urban area, does not appear to have had a burial/death omission

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[54] With a slight correction factor for deficient civil registration.

[55] For Glass's estimates see Glass and Eversley, *op. cit.*, p. 234.

[56] For example, Krause in Glass and Eversley, *op. cit.*, p. 392.

[57] *Ibid*, pp. 390 and 392. Krause estimates that there were only 1,300 non-parochial burials included in the figures for the 1810s.

[58] Krause, "Changes in English fertility ...", *op. cit.*, p. 56.

rate very different from that quoted from the first two decades in Table 18. The main reason the under-registration of burials is probably the same as that for the under-registration of baptisms – but the discussion of this is dealt with in Chapters 7 and 8.

Using the burial omission proportions outlined in Table 17 it is possible to calculate death rates for the period 1801–1841.

Table 18. Estimated Death Rates per 1,000 Population for England and Wales, 1801–1841[59]

Period	*On Official Population Returns*	*On Krause's Population Estimates*
1801–10	30.1	30.5
1811–20	27.7	28.9
1821–30	26.1	25.9
1831–40	23.1	22 8

Both sets of estimates suggest a similar pattern of a significant decline in mortality from 1800 onwards – they differ only in the chronology of this decline. Crude death rates, of course, are only rough indices of mortality as they can conceal genuine changes in mortality disguised by shifts in the age structure of the population. The comparison of the age structure of the population in 1821 and 1841 does not reveal any such significant shifts,[60] so that the figures in Table 18 suggest that there was a real fall in mortality.

The published statistics of age structure of the enumerated 1821 and 1841 populations provide us with an appropriate source of information which, with the first life table of 1838–44, based on civil registration, permits an independent check of the plausibility of the figures in Table 18. The expected number of survivors of a particular age group at a specific moment can be calculated by applying age-specific mortality rates to the estimated numbers of children born into that group. The likely number of

[59] Populations for each period are averages of the censuses at the beginning and end of each decade. For the populations used in computing these rates, see Krause, "Changes in English fertility ...", *op. cit.*, p. 56; for burials, see Mitchell and Deane, *op. cit.*, p. 28.

[60] Glass and Eversley, *op. cit.*, p. 215. The slight decrease in the proportion of young children revealed by this comparison is an indirect confirmation of the fall in fertility outlined in Table 16.

births in the period 1801–1841 has already been estimated above through the use of the N.I.R. ratios.

There is no direct evidence on age-specific mortality for this period, but there is good reason to believe that the 1838–44 life-table gives a good approximation of the relative rates of mortality between the different age groups, from about 1813 onwards. The returns of Anglican burials, during the period 1813–40, contain information on age at death, and a comparison of the data for 1813–30 and 1831–40 indicates that there was no significant change in the distribution of ages at death between the two periods. The age structure of the 1821 and 1841 populations was very similar, which suggests that relative age-specific mortality was approximately the same in both periods. The distribution of ages at burial and the equivalent age structure of the population (England and Wales) in younger age groups was as follows:

	Distribution of burial ages, 1813–40	*Distribution of burial ages, 1831–40*
	%	%
0–4	34.5	35.8
5–9	4.2	5.0
10–14	2.7	2.7
15–19	3.4	3.5
	Age structure of population, 1821	*Age structure of population, 1841*
	%	%
0–4	14.9	13.2
5–9	12.6	12.0
10–14	11.1	10.9
15–19	10.0	10.0

The conclusion that relative age-specific mortality was probably the same in both periods depends on the assumption that the relative omission of deaths from burial registration by age group did not vary over time. As only very large variations of this kind could affect relative age-specific mortality significantly, this assumption is not a particularly critical one.[61]

[61] For the evidence on burial ages, see *Parish Register Abstract 1831* (Parl. Pap. 1833, XXXVIII), p. 488, and *Parish Register Abstract 1841* (Parl. Pap. 1845, XXV), p. xxi. For comparative age structure see *Registrar-General's Sixth Annual Report* (1844), p.xxvi.

The period 1831–40 is sufficiently close in time to the years for which the first life table was constructed (1838–44), to permit the assumption that mortality was about the same in both periods – this assumption is, moreover confirmed by the fact that the estimated death rate for the 1830s in Table 18 (about 23 per 1,000) is almost identical to that during the years 1838–44 calculated from civil registration returns. Of course, the absolute levels of mortality at different ages cannot be assumed to have been the same during the whole period 1801–1840, and, in fact, the estimated death rates in Table 18 suggest otherwise. If we use these death rates as indices of absolute mortality levels, we can weight relative age-specific mortality taken from the first life table by multiplying by the proportionate excess of a decade's death rate over that for the 1830s.[62] This means that the estimated burial omission rate is under evaluation, as this was used to calculate the death rates during 1801–40. The final result of the relevant calculations is set out in Table 19.

Table 19. Expected and Actual Numbers of People Living at Various Ages, England and Wales (in thousands)[63]

Age Group	*Expected 1821 Census*	*Actual 1821 Census*	*Discrepancy (percentage)*
0–9	3,127	3,347	–6.3
10–19	2,539	2,552	+0.7
Age Group	*Expected 1841 Census*	*Actual 1841 Census*	*Discrepancy (percentage)*
0–9	3,931	3,997	–1.7
10–19	3,503	3,307	+5.9

These figures suggest that the estimates of under-registration of births (the

[62] I have calculated proportions of children dying by a particular age, according to the 1838–44 civil registration returns from the detailed age-specific mortality figures for males and females given in Mitchell and Deane, *op. cit.*, pp. 38, 40, and weighting them for sex differences in age structure with figures given in the *Registrar-General's Sixth Annual Report* (1844), p. xxvi. For the absolute indices of mortality I have used the death rates calculated from Krause's estimated population figures.

[63] For the actual numbers living in each age group, see Glass and Eversley, *op. cit.*, p. 230.

N.I.R. proportions) and deaths (through comparison of enumerated populations statistics) are of a high order of plausibility. The discrepancies for the 0–9 age group in 1821 and the 10–19 age group in 1841 are fairly significant, but they are small compared to the correction rates applied to the original raw figures (the expected figures of Table 19 depend mainly on the correction rate to baptisms and this falls within the range of 27.4% to 33.0%).[64]

CONCLUSION

The comparison of census and baptism register data for 45 parishes indicates that the level of registration adequacy did not change significantly during 1760–1834 . About one-third of all births were omitted from baptism registers – and there is some evidence to suggest that this level of accuracy was typical of both earlier and later periods. There were, however, some marked variations between individual parishes, and significant variations over time in individual parishes. A number of sources of error in the census/register comparison method emerged, although no substantial evidence has emerged from this study to question its basic validity. It is therefore suggested that all family reconstitution projects using parish registers include a comparison of the 1851 Census with the baptismal register so as to evaluate register reliability. To economize on time it would be sufficient to collect information on the census ages of individuals (in order to analyse changes over time) and to use the following criteria for establishing a person as being in the register: (i) name the same; (ii) age within five years of that expected from the 1851 Census. If this information were collected for a sufficient number of parishes it would be possible to make firm generalisations for the country as a whole, particularly if cities such as Nottingham which have complete sets of parish registers could be included (in these cases it would be a question of comparing city of birth with all the registers for that city). The method might also be capable of being extended to data for much earlier periods than that dealt with in this

[64] The actual figures in Table 19 would also probably require correction because of under-enumeration in the 0–4 age group; Glass has estimated this to be about 5% in 1841; Glass and Eversley, *op. cit.*, p. 234.

paper. The census/register method already points to certain tentative conclusions about the pattern of fertility and mortality during the period of the Industrial Revolution; further studies in conjunction with family reconstitution projects could help finally to answer the fundamental problem of the relationship between population and economic growth in the eighteenth and nineteenth centuries.

Chapter 5

Further Evaluation of Anglican Baptism as a Form of Birth Registration Through Cross-matching Census, Parish and Civil Register Data[1]

This paper was written in response to various criticisms of the census/baptism register comparison method outlined in the previous chapter. There were three main criticisms of the method: (i) the 1851 Census significantly mis-stated the birthplace of individuals enumerated; (ii) many children were baptised outside their parish of baptism; (iii) the 1851 Census occasionally mis-stated names and ages.

An evaluation of these criticisms was carried out by cross-matching census, baptism and civil register data. The conclusions from this research for the parishes studied are: (i) about 3.2 per cent of all birthplace statements were in error; (ii) approximately 4.0 per cent of children were baptised outside of their parish of birth in the period 1813–52; (iii) about 2.1 per cent of all census entries were in error on name and ages statements sufficient to create a false match. In total, it is estimated that 9.3 per cent of all census cases not found in the baptism register (N.I.R.s) were due to under-matching as a result of these three categories of error.

However, this is counter-balanced by the over-matching of cases due to: (i) over-strict criteria employed in ruling out correct matches; (ii) missed births resulting from infant death before baptism. These two sources of over-matching are estimated as forming about 10.2 per cent of all cases, in effect counter-balancing the 9.3 per cent of under-matched cases. It is

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[1] Not previously published. The research on which this paper is based was funded by the Social Science Research Council.

concluded that the census/baptism register comparison method is overall a simple and reliable method of estimating baptism register accuracy.

The bulk of historical work on demography of England before the introduction of civil registration is dependent on Anglican parish registers for its source material. The development of new techniques of analysis, such as family reconstitution, does not overcome the problem of unreliable raw data, and the application of these new techniques alongside the proliferation of work using older methods like aggregative analysis, makes it essential to evaluate the quality of Anglican register information as a form of demographic data. Previous work by the present author[2] involving the comparison of census and parish register information, suggested large deficiencies in the reliability of Anglican baptism as a form of birth registration, with marked variability in the quality of data between different parishes.

The cross-matching of census and baptism register data works on the assumption that the 1851 Census (the first to give full details of birthplace) accurately states name, age and the birthplace of individuals enumerated. It also assumes that a person's birthplace is the same as his or her parish of baptism, allowing a comparison of the expected parish of birth (from the census) and the actual parish of birth as indicated through baptism. The validity of this cross-matching methodology has been criticised on three grounds: (i) the 1851 census significantly mis-states the birthplace of individuals enumerated; (ii) many parents had their children baptised in neighbouring parishes to that in which they were born, exaggerating the short-fall between births and baptisms; and (iii) "to a lesser degree", the 1851 Census sometimes mis-states names and ages.[3]

Tony Wrigley and his colleagues at the Cambridge Group have developed the census/ baptism register comparison method by applying it in great detail to Colyton and a number of surrounding parishes, the results of which have been published in *Population Studies*.[4] Obviously a single

[2] See Chapter 4.

[3] E. A. Wrigley, "Baptism coverage in nineteenth century England: the Colyton area", *Population Studies*, Vol. XXIX, No. 1 (1975).

[4] *Ibid*, p. 178.

parish will not necessarily be representative of the population as a whole; in the original cross-matching study of census and baptism registers, Colyton had a N.I.R. (= Not Found in the Baptism Register) ratio of 20.6 per cent, which was significantly below that of the average of the whole sample of 45 parishes: 31.0 per cent. Wrigley has quoted the low N.I.R. ratio of 6.3 per cent for Bottesford[5] in support of his general conclusions, and he equally could have referred to the even lower ratios for Acomb, Yorks (6.0 per cent), Bretforton, Worcs. (5.5 per cent) and Medmenham, Bucks. (4.8 per cent) from my original study, but this would be misleading as several parishes had N.I.R. ratios in the 40–50 per cent range, and, as we have seen, the average was above 30 per cent. This latter conclusion must be borne in mind when we are assessing the significance of findings about baptism registration in the Colyton area.

The major method used by Wrigley and his colleagues in assessing the quality of the 1851 Colyton census is the comparison of its contents with data taken from the 1841 and 1861 censuses for the same parish. This is an extension of the method employed in my first paper,[6] the difference being that an analysis of all birthplace statements for individuals listed in the censuses has been undertaken, instead of confining it to people enumerated as Colyton-born in the 1851 Census. This has meant that not only "loss errors" (individuals listed in 1851 as being born in Colyton, but elsewhere in 1861) could be counted, but also "gain errors" (elsewhere in 1851, Colyton in 1861).The names that Wrigley has given to these inconsistencies in census birthplace statements, indicate the nature of his analysis. It is assumed that most of the inconsistencies in the first category are due to individuals having mistakenly claimed Colyton birth in the 1851 Census, and then corrected their birthplace statement in the subsequent one; similarly, the people in the second category, are defined as having "gained" an error by changing a correct birthplace statement in 1851 to an incorrect Colyton one in 1861. Some evidence is produced in support of these assumptions: of 16 "loss error" cases traced in a baptism register, 10 were baptised outside of Colyton, while 6 were baptised in the parish.[7] Similarly, of the 35 "gain errors", only one could be traced in the Colyton baptism register, although it is not known whether any of the remaining

[5] *Ibid*, p. 316, fn. 27.
[6] See Chapter 4.
[7] *Ibid*, p. 316.

34 cases were baptised in the birthplace mentioned in the 1851 Census, as no search of these registers has been carried out.[8] At first sight the evidence on the baptism patterns of the "loss errors" and "gains errors" appears to confirm the assumptions about the nature of the errors. In one respect this is crucial to the argument, because it is on the basis of these "errors" that the later estimates of the number of birthplace statement errors in the 1851 Colyton census are made.

The assumptions about "gain errors" were further tested by dividing the sample between those present in Colyton in 1841 and 1851, as against those only present in 1851. The argument is that if there is a tendency for people born outside of the parish to cumulatively claim it as their birthplace, more "gain errors" would occur among those who were in Colyton in 1841 and 1851, than among those enumerated for the first time in 1851. Unfortunately this evidence suffers, as does that above about the baptism patterns of those in the two "error" categories, from a central defect: 62.5 per cent of cases baptised in a neighbouring parish, had parents resident in Colyton at the time, indicating that nearly two-thirds of the cases baptised outside of Colyton were in fact born there, suggesting that most of the birthplace statements were correct.[9] This has a very basic significance for the discussion about the reliability of census birthplace statements which I will return to later, but for the moment it is sufficient to point out that it brings into question the small amount of direct evidence supporting the assumptions about loss and gain "errors".

The second criticism of the census/baptism cross-matching methodology is the unreliability of name and age statements in the census. In practice this criticism has been seen as very minor compared to the postulated deficiencies of the census with respect to birthplace statements. A great deal of evidence from my first paper showed how reliable age statements were according to both 1851/1861 census comparisons, and the checking of age statements with date of baptism. This is a conclusion that Wrigley is in agreement with: he notes that 10 cases of a match between census and baptism register out of a total of 1,133 (0.9 per cent) would have been missed by assuming that age was accurate to within five years,

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[8] *Ibid*, Private communication from Tony Wrigley.

[9] This finding applies to the 80 (of the 107) F cases with information on residence. See Table 11, *Ibid*, p. 313. For Wrigley's conclusions about the birthplace of these cases see pp. 313, 314.

and the vast majority of cases were accurate to within one year.[10] The problem of name inaccuracies has been found to occur on a similar scale: it is estimated that about 17 links out of a total of 1,471 (1.2 per cent) would have been missed by the census/baptism register comparison method on account of name recording inaccuracies.[11] Taken together, age and name inaccuracies would only account for 2.1 per cent of unmatched cases.

The third ground of criticism of the cross-matching methodology is that it assumes all children were baptised in their parish of birth. The Cambridge Group through its contact with local amateur historians has been able to arrange a search of 23 registers of parishes in the neighbouring area of Colyton, so as to check the parish of baptism of those claiming Colyton birth. Of a total of 1,433 people claiming birth in Colyton (in the 1851 Census), 107 (7.5 per cent) were found to be baptised in neighbouring parishes, and for the pre-civil registration group who were ten years and over, the proportion was even higher – 89 out of a total of 910 (9.8 per cent).[12] There are two possible reasons for these people being baptised outside of their claimed parish of birth: (i) the 1851 census was incorrect on birthplace statement, and these cases were actually born in their parish of baptism; (ii) the census was correct on birthplace, but parents were simply baptising their children in neighbouring parishes. There is enough additional evidence available to settle this question fairly definitively. Wrigley has analysed the relationship between the area within Colyton and the location of the neighbouring parish in terms of geographical proximity and found a very close correlation, that is to say people baptised outside Colyton tended to use the parish church adjoining the Colyton localities in which they were living in 1851. He points out that:

> the parish church of Colyton is far less accessible from the periphery of the parish than from any part of the town of Colyton itself. Many people lived nearer to other parish churches than to their own.[13]

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[10] *Ibid*, p. 303. As my original sample excluded married and widowed women, the proportion of missed matches on account of age would have been even smaller, i.e. 7 out of 1,000 (0.7 per cent), instead of 10 out of 1,133 (0.9 per cent).

[11] *Ibid*, pp. 302, 304.

[12] *Ibid*, p. 309.

[13] *Ibid*, pp. 311, 312.

Even more direct evidence comes from the study of the stated residence in the baptism registers of 80 of the 107 children baptised outside of Colyton (most of these are people who were under forty in 1851, because place of residence only appeared regularly in the baptism register after the introduction of Rose's Act in 1812). As previously noted, fifty of the eighty were resident in Colyton at the time of their baptism, and taken along with all the other evidence about geographical residence and baptism patterns, the conclusion is escapable:

> It is evident that Colyton residents frequently preferred to use the services of incumbents of neighbouring parishes for baptisms and that therefore many respondents were giving accurate information at census time in claiming Colyton birth even though they had been baptised elsewhere.[14]

The practice of baptising outside of the parish of birth seems to have taken place with some frequency in the Colyton area: for the total number of people studied, 4.7 per cent (50/80 x 7.5 per cent) were baptised outside Colyton even though they were born there.[15]

Wrigley has estimated that about 140 of the 1,4471 individuals who gave Colyton as their birthplace in 1851, were in actual fact born outside of the parish. This conclusion is reached in two ways: (i) the "gain error" ratio is applied to all those who claimed extra-Colyton birth in 1851 (1,032 cases) – and various complex estimated adjustments are made for "loss errors", migrations and deaths between census periods;[16] (ii) the number of people claiming Colyton birth yet baptised and residing elsewhere – estimated as numbering 41 people – is added to the 103 individuals claiming Colyton birth in 1851, who could not be found in either the Colyton baptism register or those of surrounding parishes.[17] We have already seen that the concepts of "gain" and "loss errors" are highly suspect, and the small amount of direct evidence that we have about them leads us to question their validity. This mode of calculating the number of census birthplace mis-statements is of a highly hypothetical character involving a number of speculative assumptions, leading to an inflation of

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[14] *Ibid*, p. 313.

[15] *Ibid*. See Table 8 on p. 309 and Table 11 on p. 313 for the source of these figures.

[16] *Ibid*, pp. 305, 306.

[17] *Ibid*, p. 314.

known inconsistencies from the order of 20 ("loss errors") and 35 ("gain errors") to a number four to seven times that magnitude (140).

The second method of calculating census errors is much more satisfactory than the first because it is based on direct empirical observation and involves a minimum of hypothetical assumption. This involves comparing census statements about birthplace with baptism register entry. There are two components to the estimate: the F cases (*F Case = Found in a baptism register outside Colyton*) and NF cases (*NF Case = Not found in any baptism register.*) I estimate that 41 F cases were census statement errors – a figure arrived at by multiplying the total number of F cases (107) by the proportion known to have been residing in Colyton, and subtracting this from the total. The figure can be checked by using civil registration data published by Wrigley at the end of his paper: 23 of 32 F cases of an age to be found in civil registers, were found to have been born in Colyton.[18] If we apply this ratio – 23/32 – to all the 107 F cases, we reach a total of only 30 born outside of Colyton. The conclusion must be that between 30 and 41 of these 107 cases were census birthplace statement errors.

The second component of the estimate is the NF cases – it was estimated by Wrigley that 103 of these cases were birthplace statement errors, but this figure is almost certainly incorrect. As a part of the civil registration cross-matching, the 103 NF cases were checked where appropriate in the local Colyton civil register; 57 of the 103 cases were under the age of nine in 1851, and 42 of them were found to have been born in Colyton itself.[19] In other words, 73.7 per cent (42 out of 57) of these NF cases were definitely born in Colyton, and as we shall see later, there is some reason to believe that even the remaining 26.3 per cent might not have been born outside Colyton. It is clearly invalid to add the 103 NF cases to the 30–41 F ones in arriving at an estimate of the number of 1851 Census birthplace mis-statements, as the majority of the former were almost certainly correct in claiming Colyton as their birthplace.

We may summarise the position on the actual birthplaces of the 1,471 people who claimed Colyton as their birthplace in the 1851 Census, as follows: (i) 79.1 per cent of them were found in the Colyton Anglican baptism register and can therefore be assumed to have also been born in

[18] *Ibid.*

[19] *Ibid.*

the parish;[20] (ii) 6.3 per cent were baptised in non-conformist Colyton registers and similarly can be assumed to have been Colyton-born; (iii) of the 7.5 per cent in the F category, between 4.7 per cent (calculated on a residence basis) and 5.4 per cent (calculated on a civil registration basis) were born in Colyton; (iv) the remaining 7.2 per cent not found in any baptism register can on conservative assumptions – taking the evidence from the civil register entries on the 57 NF cases in the 0–9 age category – be divided between 5.5 per cent (7.2 per cent × 42/57) born in Colyton and 1.7 per cent born elsewhere. If we total these figures, we find that between 95.6 and 96.3 per cent of people claiming Colyton birth in 1851, were actually born there. This is a conservative figure because it only counts someone as being Colyton-born where there is tangible evidence in support of an estimate; many of the NF cases not found in the Colyton civil register may have been born in Colyton, but have been excluded in calculating the appropriate percentage for this category.

It is possible to extend this mode of analysis by directly comparing census statements of birthplace with civil registration information for those born after the introduction of civil registration. Wrigley and his colleagues were unable to do this for Colyton in a complete and systematic fashion because of restricted access to civil registration returns. I have however been able to check the birth entries of 117 children listed in the 1851 Census as born in Colyton and aged twelve and under, the check being made in both the local area register for the district of Axminster and in the national index kept at St Catherine's House. Of these 117 children, 94 were traced in the civil register, 92 of them to the actual parish of Colyton, i.e. of those traced, 97.9 per cent (92 out of 94) were returned as being born in Colyton. At first sight it seems surprising that only 80.3 per cent (94 out of 117) could be traced to a civil register, but we will see later that there is large amount of evidence to suggest that civil registration was often deficient to this order during the early period of

[20] It is possible of course that some of these cases were born in neighbouring parishes and baptised in Colyton. This is unlikely given that they were returned in the 1851 Census as having been born in Colyton and were baptised in that parish. The assumption that they were born in Colyton could however be checked by analysing their parish of residence at the time of their baptism as stated in the baptism register. The same considerations apply to the 6.3 per cent of people found in the Colyton non-conformist registers.

civil registration.[21] Age is not an important factor in explaining the results (through, for example, people being born before the introduction of civil registration), indicated by the fact that the proportion traced for the 0–9 age category – 77.9 per cent (60 out of 77) – is even lower than that for the 10–12 age group, which was born according to the census nearer to the introduction of civil registration: 85.0 per cent (34 out of 40). It is possible of course that some matches were missed on account of name and age mis-statements in the census, but as we have already seen this is likely to have been only on a minor scale, not even allowing for the fact that a search of the Axminster district register was made with this in mind.

If these figures are at all representative of the Colyton population as a whole, birthplace mis-statements occurred only very rarely. There is some small discrepancy between the proportion of such errors calculated from Wrigley's data (3.7 – 4.4 per cent) and that from the direct census/civil register comparison (2.1 per cent). This can be explained by the inclusion of 1.7 per cent of the total population in the NF category in the born outside of Colyton group; evidence from work on civil registration would suggest that individuals are not always found in the civil register for the area in which they were born. Also the 117 cases in the census/ civil register cross match were all children and it is possible that older people may have had a higher percentage of birthplace errors.[22]

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[21] In the case of Colyton, an especially thorough search was made because of the importance of the parish in the debate about parish register reliability. The first 117 cases with information on parents and aged twelve and under were selected from available photocopies of the 1851 Census. These cases were then checked in the civil register for the Superintendent-Registrar's District of Axminster (which covers about half of the neighbouring parishes in which Wrigley and his colleagues found F cases), allowing for both name and age errors in the searching. Cases which could not be found were then checked in the national index kept at St Catherine's House, and traced back to original entries where appropriate.

[22] There is little to suggest this conclusion from Wrigley's paper: roughly the same proportion of people in the 0–9 and 10+ age groups were baptised in Colyton itself. See Table 8, *Ibid*, p. 309. However, there were more of such errors among the younger age group in the census/civil register sample, although the samples were rather small: there were 2 out of 32 (5.9 per cent) birthplace errors amongst the 10–12-year-olds, and none out of the 60 traced among the 0–9-year-olds. Also, see the later discussion in the text of this quotation of age and birthplace errors.

It is now possible to summarise the position on the criticisms of the original cross-matching methodology mentioned at the beginning of the paper. As far as Colyton is concerned, we have arrived at the following estimates of the proportion of errors in each category: (i) age and name errors would account for about 2.0 per cent of missed matched cases in the census/baptism register comparison; (ii) 4.7 per cent of the people claiming Colyton birth in 1851 were probably born in Colyton but baptised elsewhere; (iii) the percentage of census birthplace mis-statements ranges from 2.1 per cent through to 3.7–4.4 per cent, with the probability that the lower figure is more correct than the higher. The total proportion of errors from these three categories ranges from 8.9 per cent to 11.2 per cent, approximately a half of the percentage of N.I.R. cases found in Colyton. This obviously is a very significant proportion of the total. Two points however must be made about these conclusions on the quality of the Colyton data, before we consider evidence for a number of other parishes elsewhere: Colyton's N.I.R. ratio – 20.6 per cent – is significantly smaller than that of the average for the total 45 parishes in the original sample (31.0 per cent), and therefore a 10 per cent error ratio would only constitute about a third of the N.I.R. ratio for all parishes; the exaggeration of the number of N.I.R. cases through errors in the census must be counter balanced to some extent by N.I.R. cases missed on account of young infants dying before baptism. I estimated from data on the time interval between birth and baptism for 1,292 cases in the original census/baptism sample that about 5 per cent of young infants died before they had time to be baptised. Wrigley suggests an even higher figure and believes that it might be "necessary to inflate any baptism total by just over eight per cent"[23] on this account. This suggested correction would almost completely restore the validity of the N.I.R. ratio for Colyton on the lower estimate of error proportions (a total of 8.9 per cent), and only leave a gap of about 3 per cent on the higher one (11.2 per cent).

We can now turn to the results of the work done on cross-matching census, parish and civil registers for areas other than Colyton. This research was designed primarily to measure the extent of errors introduced into census/baptism register matching on account of: (i) census birthplace mis-statements; and (ii) the practice of baptising outside of the parish of

[23] *Ibid*, p. 315.

birth. Some work was done on census errors in age and name (particularly the latter), but this was viewed as secondary because of the existing evidence on the high reliability of this kind of information. Two sub-samples of parishes were selected from the list of 45 used in the census/baptism register work: the five parishes with the highest N.I.R. ratios; 19 parishes for which parish registers were available at the Genealogical Society's Library for the post-1837 period. The logic of the two stages of the research was as follows. If census mis-statement of birthplace was responsible for a large proportion of N.I.R. cases, those parishes with the highest proportions of N.I.R.s will provide the harshest test of census reliability with respect to birthplace; therefore the census documents of the five parishes with the greatest N.I.R. ratios (with an average of 47.6 per cent of cases not found in the baptism register) were cross-matched with civil register returns for children in the appropriate age range. Secondly in order to measure the extent of the practice of baptism outside of the parish of birth, as many parishes in the original sample with baptism registers comparable with civil register returns should be covered – the comparison yielding information on the child's parish of birth (from the civil register) and place of baptism (from the parish register). Obviously, results from this work on children and young people of an age to be found in civil registers, can not necessarily be generalised to other age groups without great care and further analysis. This and other problems with the census/parish/civil register match will be discussed in an appropriate context.

The five parishes with the highest N.I.R. ratios were Black-Torrington, Devon (44.0 per cent), Chigwell, Essex (42.9 per cent), Ringwood, Hants. (43.4 per cent), Hackney, Middlesex (58.0 per cent), and Kingston-on-Thames, Surrey (49.9 per cent). For each of these parishes, the first fifty cases were selected from both the 1851 and 1871 censuses, meeting the following criteria: (a) that they should be fully legible; (b) all should be stated as being born in the parish in question; (c) they should be of an age to be traceable in the civil registers – in the case of the 1851 Census from 0–8 years old, and the 1871 Census from 20–28 years old. The first criteria is self-explanatory. The second was adopted so that the information on birthplace used in the census/baptism register match (that on "native" parishes) was being directly evaluated. The third was designed to allow a search in the civil register within the range of at least plus or minus three years of the expected age from the census, the constraint being on the upper age limit so as to allow for three years after the introduction of civil

registration.[24] The 1871 Census was included along with that from 1851, so as to enable a check on birthplace statements of people in an older age range. It is accepted that findings from the 1871 Census cannot necessarily be generalised to the 1851 one, but it does provide an additional source of information with which to evaluate the latter. A search was made in both the civil register for the whole of the local Superintendent-Registrar's District area in which the parish was located, and for cases which could not be found there, in the national index at St Catherine's House.[25]

Table 1. The Cross-Matching of Census and Civil Register Information on Children Listed as Natives in the 1851 and 1871 Censuses

Parish	*Date of Census*	*Number in Sample*	*Number Traced in Civil Register*	*Percentage of Sample Traced*	*Number Born in Parish*	*Percentage Born in Parish*
Kingston	1851	50	38	76.0%	38	100%
Kingston	1871	50	42	84.0%	42	100%
Ringwood	1851	50	47	94.0%	47	100%
Ringwood	1871	50	46	92.0%	46	100%
Chigwell	1851	50	49	98.0%	49	100%
Chigwell	1871	50	45	90.0%	43	95.6%
Hackney	1851	50	40	80.0%	40	100%
Hackney	1871	50	42	84.0%	38	90.5%
Black-Torrington	1851	50	47	94.0%	47	100%
Black-Torrington	1871	50	42	84.0%	41	97.6%
Total		500	438	87.6%	431	98.4%

The most striking finding from Table 1 is that 98.4 per cent of cases traced in the civil registers were born in the parish stated to be their birthplace in the 1851 and 1871 censuses. In seven of the ten samples, the proportion born in

[24] Ideally, a search should have been made for an even earlier period, but this kind of cross-matching of data is extremely laborious and the evidence anyway is that about 98.0 per cent of all census statements were accurate to within plus or minus three years. (See Wrigley, *op. cit*, p. 303 and Razzell, *op. cit.*, p. 126).

[25] Wrigley found 58 of the 107 F cases baptised outside of Colyton in parishes within the Superintendent-Registrar's District of Axminster and a direct search of these local civil registers allows cases with name and other errors to be picked up, where they might be missed in the national register via an index.

the stated birthplace was 100 per cent and it was in only three of the samples – Chigwell 1871, Hackney 1871 and Black-Torrington 1871 – that there were any cases found in the civil register outside of their stated birthplace. In all, there were only seven cases in this category, and four of those were in the Hackney 1871 sample. If we compare the samples taken from the 1851 census with those taken from 1871, the overall proportions of cases found outside their expected birthplace are none for the former, and 3.2 per cent (7 out of 216) for the latter. It is possible that the 1871 Census was more deficient than the 1851 one, but there is no reason or evidence to suggest such a conclusion. More likely is the effect of age on accuracy of birthplace statement: the children selected from the 1851 Census were all aged eight or under, whereas the people chosen from the 1871 Census were aged between 20 and 28. Yet this effect of age on census accuracy appears to be limited; both Wrigley and I found that age had little effect on the accuracy of census age statements.[26]

This finding that census birthplace statements were of a high level of accuracy applies to people under the age of 28. What is the evidence that it also applies to older age groups? In my original paper, I gave evidence on the relationship between stated age in the 1851 Census and birthplace statement discrepancies between the 1851 and 1861 censuses. Although birthplace comparisons were only made for people listed as being born in "native" parishes in 1851, the results are surprisingly compatible with those derived from census/civil register matching.

Table 2. 1851 Census Age and Birthplace Statement Disagreements Between the 1851 and 1861 Census. (45 Parishes)

	Age in the 1851 Census						
	17–20	*21–30*	*31–40*	*41–50*	*51–60*	*61–90*	*Total*
Birthplace Statement Disagreements (B.S.D.s)	1	13	17	9	6	2	48
Total Cases	128	420	400	338	214	90	1,585
% B.S.D.s	0.8	3.1	4.2	2.7	2.8	2.2	3.0

The similarity between the findings in the above Table for the age groups under thirty and the results from the census/civil register comparison quoted in Table 1 is partly fortuitous, as the two Tables refer to different

[26] Wrigley, *op. cit.*, pp. 303, 304; Razzell, *op. cit.*, p. 12.

samples and the second Table compounds errors between the two censuses. However, the similarity in findings gives some confirmation to the validity of Table 2, and the results of that Table indicate no correlation between census age and mis-statement of birthplace.[27]

The overall findings of Table 1 reveal a very high level of census accuracy on birthplace statement. Only 1.6 per cent of all cases traced were born outside of expected birthplace, which can be contrasted with the 47.6 per cent of cases not found in the baptism registers of the five parishes in the sample. If we confine the comparison to the age groups involved in the census/civil register match we come to an identical conclusion: the proportion of 20–29 year olds enumerated in the 1871 Census born outside of expected birthplace was 3.2 per cent, which can be set against the 30.0 per cent of N.I.R. cases among the same age group enumerated in 1851.[28] If we compare individual parishes, the contrast becomes even starker: the N.I.R. proportions for Ringwood and Kingston in the 1851 20–29 age group (these were the only two parishes with large enough samples for analysis) were 46.9 per cent and 52.6 per cent; the equivalent proportion of people born outside the two parishes for the same age group checked from the 1871 Census to the civil birth register, were zero per cent in both cases. Of course these comparisons are making the assumption that the 1871 20–29 age group is equivalent to the same age group in 1851; although this assumption cannot be tested, there is no reason to believe that the two censuses were greatly different in their overall level of accuracy, and any such difference would have to be massive to affect the conclusions drawn from the above data, i.e. that the very high proportion of N.I.R. cases found for the original sample cannot be a function of census inaccuracy of birthplace statement.

It probably has not escaped the reader's attention that there is one feature of Table 1 which could undermine the conclusions drawn from it. Of the 500 cases selected from the 1851 and 1871 censuses, only 438 (87.6 per cent) could be traced in the civil register for the appropriate period.

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[27] Although there are difficulties with Wrigley's evidence on birthplace, the information that he does give on the characteristics of various age groups also supports this conclusion. See Wrigley, *op. cit.*, p. 309.

[28] There is the problem with phonetical name variations, but as a very careful search was made in the local civil register, as well as a one-year search in the national index, this is not likely to result in many missed cases.

There are a number of possible explanations of these missing cases, all of which can be evaluated through further work: (i) census birthplace statement inaccuracies could make a detailed search of relevant civil register difficult; (ii) problems of name variation could lead to missing cases; (iii) census age inaccuracies might lead to people being missed through an insufficient time search; (iv) there could be deficiencies in civil registration during the 1840s. I will discuss these four topics in the above sequence, and in the process of analysing the relevant data, will attempt to deal with substantive points which also have a bearing on the census/parish register comparison.

The procedure adopted for checking census birthplace statements in the civil register was as follows. An entry was initially searched for in the local Superintendent-Registrar District's register for a seven-year period, centring on the expected year of birth as listed in the census. An inspection of all entries in the civil register for this seven-year period was made, until a satisfactory match could be established. Most of the cases taken from the 1851 census had information on parents available, which facilitated the matching process and ensured that the quality of the match was very high: 96.8 per cent of the 217 cases traced had matching information on a least one parent. Even with the 1871 census, a majority of cases had parental information – 56.5 per cent (122 out of 216) – and the over-all figure for the whole sample was 76.7 per cent (332 out of 433). The minimal criteria for establishing a match was that a person of the correct name had to be found in the civil register for the seven-year period (plus or minus 3 years from expected year of birth).

In practice nearly all the cases found were traced in this way, as the Superintendent-Registrar's district included many of the neighbouring parishes where one would expect to find cases born outside of the parish. When a person could not be found in this local civil register, a very detailed search was then made in the National Index kept at St. Catherine's House, and this was undertaken for the 46 cases with parental information and confined to expected year of birth. The search was restricted in this way for three reasons: (i) it is impossible to make an adequate match for cases without parental information outside expected area of birth; (ii) a search of the national index is extremely laborious, as it contains many individuals of the same name, particularly where the person in question has a common surname; (iii) evidence to be discussed later in the paper indicates that 85.6 per cent of matched 1851 census cases, and 69.0 per cent of 1871 ones, were found in their expected year of birth. Given these restrictions on the searching process, it is obviously necessary to inflate those cases

found in the national index by two ratios in order to correct for the restrictions: (a) multiply the cases found by 63/43 in order to allow for only 43 of the 63 people not found in the local civil register having parental information; (b) and by 215/184 to allow for the restricted one-year search for the 1851 cases, and 216/149 for the 1871 ones. In fact, these complicated adjustments are more-or-less irrelevant as only one case out of a total sample of 500 was found outside of the local registration district in the national index, in their expected year of birth. Applying the above adjustments to this figure, gives approximately one case which should be added to the number of matches, i.e. a figure of 0.2 per cent. This proportion reduces the 12.4 per cent of 1851 and 1871 Census cases not found in the civil register to 12.2 per cent.

We have already discussed the problem of census name inaccuracies in the analysis of the Colyton data. The census/civil register comparison material does not lend itself to this kind of work, as necessarily cases with significant name variations might be excluded from the sample. We must anticipate research to be discussed later in the paper to find data relevant to this topic. In order to evaluate the extent of people baptising their children outside of the parish of birth, a sample of 2,042 entries in 19 baptism registers was compared with equivalent civil register entries in the period 1838–53. Three of these baptism registers give date of birth as well as date of baptism, which along with information on names of parents and father's occupation, make it possible to make links even in cases of extreme name variation. Four hundred and thirty-three cases were studied in these three parishes (Iver, Mednenham and Kingston-on-Thames) and the following is a list of name variations which probably would cause matching difficulties:

Baptism Register Entry	*Civil Register Entry*
John Ottoway	Charles Otway
George James Beard	George James Thompson
Charlotte Winterham	Charlotte Winterbourne
Joseph Grimsann	Joseph Gainshire
William Burryn	William Berry
Lydia Bance	Lydia Barnes
Phoebe Bance	Rhoda Barnes
Edward Moore	Edwin Moore
Richard Bance	Richard Barnes
Emily Bance	Emily Barnes

In all, there were ten name variations that probably would have led to a missed match, i.e. 2.3 per cent of the cases in the sample. Five of these

ten cases however, referred to one family (Bance/Barnes), and are almost certainly the result of a census copying error. Such an error which affects a whole family can be relatively easily spotted because of the repetition of relevant information. An example of this is to be found in the census/civil register comparison for the 1851 census Hackney sample: after searching the local civil register, it was discovered that the three members of the Hebbin family were in fact registered under the name of Stebbing. If we exclude the Bance/Barnes family from the list of name variations, we are left with five cases out of a total of 438 –1.2 per cent – exactly the same proportion as found by Wrigley in Colyton. There were quite a few additional cases of name variation in the sample, but they were all of the phonetical kind that are easily linkable, e.g. Truby/Trubee, Sherwell/Shurvell, Perce/Pearce and Caywood/Keywood.[29] Taking the figure of 1.2 per cent as the most accurate proportion of cases likely to be unmatched on account of name variation, we must reduce the percentage of census cases not found in the civil register from 12.2 to 11.0 per cent.

The question of accuracy of census age statements can be examined within certain limits by a direct analysis of census/civil register matching information. as all but one of the 436 cases found in the civil register were discovered through the seven-year (0 plus or minus 3) local research, the degree of age accuracy can be revealed within those limits.

Table 3. Distribution of Expected Age (Census), Minus Actual Age (Civil Registration), 5 Parishes

	Years							
	–3	*–2*	*–1*	*0*	*+1*	*+2*	*+3*	*Total*
1851 Census	1	1	3	188	24	2	0	219
	0.5%	0.5%	1.4%	85.8%	11.0%	0.9%	0%	100.1%
1871 Census	6	7	26	150	24	3	1	217
	2.8%	3.2%	12.0%	69.0%	11.1%	1.4%	0.5%	100%
Total (1851 and	7	8	29	338	48	5	1	436
1871 Censuses)	1.6%	1.8%	6.7%	77.5%	11.0%	1.1%	0.2%	99.9%

The sample of 0–8 year-olds selected from the 1851 Census matched

[29] The only problem with phonetical name variations is when the search is made by index rather than by direct register examination. Every attempt was made to allow for phonetical variation during the one-year search of the national index.

extremely well with respect to their stated age and date of birth. The only substantial error category was the overstatement of age by one year and this appears to have been a function of parents rounding up an age of a child whose next birthday was soon after the date of the census. The sample of 20–28 year-olds taken from the 1871 census was less satisfactory, although even there the majority (92.1%) of cases was to be found in the narrow band of plus or minus one year of expected birth. The question arises as to what proportion of cases could have been found outside of the band of plus or minus three years of expected year of birth if a sufficiently wide search had have been conducted? One way of arriving at an estimate of this figure is to compare the data from the above Table with that from Wrigley's paper on Colyton.[30]

Table 4. Distribution of Age Matches and Errors, 5 Parishes and Colyton

	Years						
	0	*± 1*	*± 2*	*± 3*	*± 4*	*± 5*	*± 6*
5 Parishes	338	77	13	8	—	—	—
(1851 & 1871)	77.5%	17.7%	2.9%	1.8%			
Colyton	594	173	30	6	4	2	4
	73.1%	21.3%	3.7%	0.7%	0.5%	0.2%	0.5%

The two distributions up to the plus or minus three-year category are similar, particularly when it is remembered that the Colyton figures are based on a comparison of census ages with baptism and not birth dates. If we combine the 0 and plus or minus one-year categories so as to allow for the inaccuracy of Colyton baptism dates, we find that 95.2 per cent of the cases in the five parishes were accurate to within plus or minus one year, compared to 94.4 per cent of the Colyton ones. We would expect the Colyton figure to be smaller as 1.2 per cent of its cases were found outside of the plus or minus three year band. These figures are sufficiently close, to warrant taking the Colyton data on the larger age errors as being representative of the five parishes as well. Thus we should further reduce

[30] I have taken the figures for single men and single women from the Colyton data, as these were most comparable from an age point of view with the information on the five parishes. See Wrigley, *op. cit.*, p. 303.

the 11.0 per cent of census cases not found in the civil register on account of age errors in the census by 1.2 per cent, giving a figure of 9.8 per cent.

We have now reached a final figure for the percentage of census cases not found in the civil register. In order for the census/civil register method to be valid, it is necessary to accept that 9.8 per cent of the cases selected from the 1851 and 1871 censuses for the five parishes of Kingston, Hackney, Black-Torrington, Chigwell and Ringwood, were not registered under civil registration. D. V. Glass published estimates of birth under-registration during this period for England and Wales, derived from a comparison of 1851 Census age returns with national civil register data on births and deaths.[31] He estimates that 8.6 per cent of all births were omitted from civil registration during the period 1841–45, and 6.0 per cent during the following five years, 1846–50.[32] Glass wrote on the subject that "the estimates very probably understate the deficiencies of birth registration",[33] so it is clear that the figure of 9.8 per cent of census cases not found in the civil register is compatible with this independent estimate of under-registration.

In addition to this independent confirmation of the scale of civil under-registration, there is direct evidence from the research involving the comparison of baptism entries with civil registers that leads to the same conclusion. Of the 2,042 cases taken from the 19 baptism registers, 281 could not be found in the civil registers – representing a proportion of 14.3 per cent. A search was made in both the local Superintendent-Registrar's district register, and where cases could not be found locally, in the national index. A check was always made of the yearly quarter in which the baptism took place, and the preceding quarter. A complication arises with this data because of the known delay between birth and baptism, but in three of the parishes, the baptism register gives the date of birth – enabling a very precise search of the civil birth register.

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[31] D. V. Glass, "A note on the under-registration of births in Britain in the nineteenth Century", *Population Studies* (1951); D. V. Glass, "Population movements in England and Wales, 1700 to 1850", Glass and Eversley (eds.) *Population In History* (1965); and "Vital registration in Britain during the nineteenth century" (Appendix 4), D. V. Glass, *Numbering The People* (1973), pp. 181–205.

[32] D. V. Glass, *Numbering The People*, p. 182.

[33] *Ibid*, p. 181.

Table 5. Comparison of Baptism Register Entries With Civil Birth Register Information (3 Parishes)[34]

Parish	*Total Cases in Sample*	*Cases Found in Civil Register*	*Percentage Found in Civil Register*
Iver, Bucks.	100	74	74%
Medmenham, Bucks.	200	192	96%
Kingston, Surrey	200	160	80%
Total	500	426	85.2%

The proportion of cases found varied between 74 and 96 percent, with an average for the total 500 cases of 85.2 per cent. The 14.8 per cent of cases not found could not be reduced, even though the yearly quarter in which the births took place were searched twice in both the local civil registers and in the national index, as were the preceding and following quarters. Also, in the case of the parish of Iver, information on place of residence at time of baptism was available in the parish register, and in every one of the 26 cases not found in the civil register,the parish of residence was Iver itself.

Enough has been said to indicate that the proportion of cases not found in the census/civil register match is not a function of the cross-matching methodology, but is a genuine finding about civil under-registration. We have been forced to diverge widely from the main discussion of the census/baptism register matching process, and in the analysis of the reasons for civil under-registration, a number of points relevant to the evaluation of the census/baptism comparison method were made but not related to the mainstream of the discussion. I will therefore summarise the main relevant findings from this point of view which have now emerged. (1) Of 438 cases selected from 1851 and 1871 Census documents for five parishes, 431 of them (98.4 per cent) were found through civil registration to have been born in their expected parish of birth as defined by the census. (2) An estimate has been made on the basis of baptism/civil register data that 1.2 per cent of names varied sufficiently to lead to missed matches in

[34] The cases were selected as follows: from 1 January 1838, the first hundred baptisms from the Iver Register and first one hundred cases from Medmenham and Kingston; from 1 January 1845, the first one hundred baptisms from the Medmenham and Kingston registers.

the census/baptism comparison process. (3) On the basis of census/civil register data for five parishes and the information on Colyton on census age statement accuracy, it was estimated that 1.2 per cent of age statements were inaccurate beyond the plus or minus 3 year level (the proportion of age errors relevant to the census/baptism comparison would be smaller as the criteria allowed for a plus or minus 5 year error).

The above covers two of the three main criticisms made of the census/baptism register cross-matching methodology, leaving the argument that it failed to take into account the practice of parents baptising their children outside of the parish of birth. In order to evaluate this difficulty, 19 of the original 45 parish registers in the census/baptism matching sample were selected on the basis of having available registers with post–1837 information on baptism, allowing cross-matching with civil registration. Data yielded from this research gives a precise measure of the extent of baptising outside the parish of birth during the late 1830s and 1840s. Table 6 outlines the main body of findings from this work

Table 6. The Evaluation of the Extent of Baptism Outside the Parish of Birth, Through the Cross-Matching of Baptism and Civil Registration Data

Parish	*Period*	*Total Cases*	*Cases Found*	*Born in Parish of Baptism*	*Percentage of Cases Born in Parish of Baptism*
Muker, Yorks.	1838–40	100	100	97	97.0%
Purleigh, Essex	1845–48	100	88	86	97.7%
Barton-Hartshorn, Bucks	1838–40	13	12	12	100.0%
Preston-Bisset, Bucks.	1838–40	67	63	61	96.8%
Horton, Bucks.	1845–52	100	80	76	95.0%
Eton, Bucks.	1845–46	100	77	75	97.4%
Bramfield, Suffolk	1838–43	100	91	91	100.0%
Chetwode, Bucks.	1838–40	19	19	17	89.5%
Lapf'ord, Devon	1845–50	100	91	89	97.8%
Benenden, Kent	1838–40	100	94	93	98.9%
Old Malton, Yorks.	1845–48	100	95	94	98.9%
Chardstock, Dorset	1845–48	100	67	66	98.5%
Chigwell, Essex	1838–40	100	79	77	97.5%
Old Malton, Yorks.	1837–38	43	42	42	100.0%
Medmenham, Bucks.	1838–46	100	95	93	97.9%
Ringwood, Hants.	1838–39	100	86	85	98.8%
Bramfield, Suffolk	1845–5C	100	74	74	100.0%

Parish	*Period*	*Total Cases*	*Cases Found*	*Born in Parish of Baptism*	*Percentage of Cases Born in Parish of Baptism*
Langley-Marish, Bucks.	1838–40	100	75	68	90.6%
Iver, Bucks.	1838–39	100	74	70	94.6%
Kingston, Surrey	1838	100	77	76	98.7%
Medmenham, Bucks.	1845–53	100	97	95	97.9%
Kingston, Surrey	1845	100	83	80	96.4%
Fordingbridge, Hants.	1838–39	100	94	91	96.8%
Total		2,042	1,751	1,708	97.5%

The proportion of cases baptised in their parish of birth varied from 90.6 to 100 per cent, with a total average for the whole sample of 97.5 per cent. Nearly all of the 2.5 per cent of baptisms which occurred outside the parish of birth were in areas immediately adjoining the birthplace, 37 of the 43 cases falling into this category. Even the six exceptions tended to be found in the same regional area: Muker/Middlesborough, Medenham/Reading, Iver/ St. John's London, Kingston/Guildford, Kingston/Hillingdon, Kingston/Gt Portland St London).

Again the question is raised whether the very high proportion of cases born in the parish of baptism is an artefact of the problem of locating cases born outside of the parish in question. I have dealt with this possibility in the discussion of civil registration, where it was concluded that the failure to trace cases was a genuine function of under-registration. In the case of the baptism/civil registration match, additional information is available on the not found cases which confirms this conclusion. For three of the parishes in the sample – Old Malton, Chigwell and Chardstock – 27 cases were traced to the 1851 Census and their stated birth-place was examined: 26 of them were listed as having been born in their parish of baptism, a proportion of 96.3 per cent. For six parishes – Fordingbridge, Old Malton, Lapford, Iver, Horton and Ringwood – systematic evidence on the parish of residence at time of baptism was available in the parish register; 69 out of 75 not found cases were resident in their parish of baptism (92.0 per cent) at the time of baptism. Although this proportion is 5.5 per cent below that for the found cases, the size and nature of the samples is greatly different, and the important point is that the vast majority of these not found cases were resident in their parish of baptism. (If we allocated the not found cases to the categories of "born in parish of baptism" and "born outside parish of baptism" on the ratio of these residence findings, the

proportion of cases born outside the parish of baptism would rise by only 0.2 per cent).

This data on baptism/civil register patterns is limited to the post-civil registration period, whereas the main interest of the census/baptism register comparison lies in the several decades immediately previous to this. There is evidence to suggest however, that a majority of children were baptised outside their parish of birth because as a result of moving from one parish to another, and information on residence is systematically given in many baptism registers after 1812 with the introduction of Rose's Act. Of the 43 cases born outside the parish of baptism, 23 (53.5 per cent) were listed as residing outside of the parish of baptism. The parish register does not always give information on residence, so it is possible that more cases were in this position than is indicated by these figures, but if we assume that listed residence gives us over a half of the cases born outside of the parish of baptism, we can analyse birth/baptism patterns as far back as at least 1813.

Table 7. Patterns of Residence in Relation to Parish of Baptism, 1813–52

Period	*1813–22*	*1823–32*	*1833–42*	*1843–52*	*Total*
Total Cases	3,261	3,721	3,413	1,908	12,303
Cases Residing Outside of Parish at the Time of Baptism	92 (2.8%)	105 (2.8%)	51 (1.5%)	25 (1.3%)	273 (2.2%)

In order to translate the percentages of cases residing outside the parish of baptism into proportions of children born outside the parish of baptism, it is necessary to inflate them by the ratio of 43/23. This gives the following proportions: 1813–22 – 5.2 per cent; 1823–32 – 5.2 per cent; 1833–42 – 2.8 per cent; 1843–52 – 2.4 per cent. The latter two percentages confirm the validity of this inflation procedure as they refer to the period of the baptism/civil register comparison (1838–53) when 2.5 per cent of children were born outside their parish of baptism. The figures derived from Table 7 reveal a distinct trend, with the proportion of children born outside their parish of baptism approximately halving between the 1810s and the 1840s: from 5.2 per cent to 2.4 per cent.

However, the above figures conceal significant variations between the eleven parishes included in the residence sample, and as we shall see, one of them contains over half of the cases of children born outside their parish of baptism.

Table 8. Patterns of Residence in Relation to Parish of Baptism

Parish	*Total Cases*	*Cases Residing Outside Parish at Time of Baptism*
Old Malton, 1813–1850	1,238	13 (1.6%)
Kingston, 1813–1847	3,387	139 (4.1%)
Benenden, 1813–40	1,380	3 (0.2%)
Lapford, 1813–49	755	33 (4.4%)
Eton, 1813–49	2,574	8 (0.3%)
Medmenham, 1813–50	501	16 (3.2%)
Langley-Marish, 1813–40	1,049	33 (3.1%)
Preston-Bisset, 1813–40	522	20 (3.8%)
Chetwode, 1813–40	128	2 (1.8%)
Harton-Hartshorn, 1813–40	110	2 (1.8%)
Bramfield, 1813–40	659	4 (0.6%)
Total	12,303	273 (2.2%)

The percentage of cases residing outside the parish of baptism varies from a low of 0.2 per cent, to a high of 4.4 per cent. The second highest proportion is for Kingston, and although it contains 27.5 per cent of the total sample, it accounts for 50.9 per cent of the "outside parish birth" cases. The total samples were selected on the availability of information on residence (although the really large parishes like Kingston were reduced by half so as to not completely overbalance the sample). If we analyse changes in birth/baptism residence patterns over time for Kingston as against the rest of the sample, the results are as follows:

Table 9. Patterns of Residence in Relation to Parish of Baptism, Kingston Compared to Rest of Sample

	1813–22	*1823–32*	*1833–42*	*1843–52*	*Total*
Kingston					
Total Cases	824	902	897	764	3,387
Cases Residing Outside of Parish at Time of Baptism	45 (5.5%)	64 (7.1%)	13 (1.4%)	17 (2.2%)	139 (4.1%)
Rest of Sample (10 Parishes)					
Total Cases	2,436	2,818	2,515	1,147	8,916
Cases Residing Outside of Parish at Time of Baptism	46 (1.9%)	41 (1.5%)	39 (1.5%)	8 (0.7%)	134 (1.5%)

Not only is the trend much less distinct in these figures, but the absolute level of cases born outside the parish of baptism is lower. If we apply the inflation ratio to the sample excluding Kingston, we arrive at the following estimate of proportions of children born outside of their parish of baptism: 1813–22 – 3.6 per cent; 1823–32 – 2.8 per cent; 1833–42 – 3.0 per cent 1843–52 – 1.3 per cent. The latter estimates still appear to be soundly based: the average of the residence figures for 1833–52 – 47 cases out of 3,662 (1.3 per cent) – inflates to 2.5 per cent when multiplied by 43/23, exactly the same figure of cases born outside of the parish of baptism in the baptism/civil register sample during the same period. Applying the same inflation ratio to the total of the rest of the sample we arrive at a figure of 2.9 per cent of cases born outside of their parish of baptism. These estimates suggest that up to the 1840s, there was an even plateau in the proportion of children born outside of their parish of baptism, with something of a slump after 1842. The results of the Kingston data indicate a different pattern: a slight increase during the 1820s, followed by a very sharp fall in the 1830s and 1840s.

We are now in a position to summarise the findings of this paper with reference to an evaluation of the validity of the census/baptism matching methodology, and will compare the results of the discussion of the Colyton data with that of the census/civil register and baptism/ civil register research material. The summary can be grouped under the three critical headings outlined at the beginning of this paper:

1.a. The percentage of 1851 Census birthplace statement errors for Colyton ranges from 2.1 per cent to 3.7–4.4 per cent, with the probability that the lower figure is more correct than the higher one.

1.b. Census birthplace errors as measured by the census/ civil register comparison method for the five parishes of Kingston, Ringwood, Chigwell, Hackney and Black-Torrington (the parishes with the highest N.I.R. ratios), were zero for the 1851 Census and 3.2 per cent for the 1871 one. There is evidence to suggest that the latter figure is more representative than the former of most age groups in the 1851 Census.

2.a. The Colyton data suggests that 0.9 per cent of census age statements were inaccurate by more than plus or minus 5 years; it has been estimated that name variations would lead to 1.2 per cent of correct matches being missed.

2.b. The census/civil register comparison indicates (along with the Colyton data) that 1.2 per cent of census age statements for the five parishes were inaccurate by more than plus or minus 3 years; from the study of 433 cases in three baptism registers which give date of birth, it was estimated that 1.2 per cent of correct matches would have been missed through name variation.

3.a. Of the people claiming Colyton birth in the 1851 Census, 4.7 per cent were probably born in Colyton but baptised elsewhere.

3.b. Of 2,042 baptism entries from 19 parish registers cross-matched with civil register information, 2.5 per cent were born outside their parish of baptism. Information on residence patterns suggests that for a sub-sample of ten parishes, this proportion of 2.5 per cent for the post-civil registration period ought to be increased to 2.9 per cent to cover the forty-year period 1813–1852. The parish of Kingston appears to be exceptional in its very high proportion of children born outside the parish of baptism as indicated by information on residence: a total of 4.1 per cent of cases residing outside the parish of baptism in the period 1813–1852 indicating a proportion of 7.7 per cent of children born outside of the parish. If we aggregate Kingston with the other ten parishes in the residence sample, we arrive at a total figure of 2.2 per cent of "outside residence" cases, suggesting a figure of 4.1 per cent of children born outside of their parish of baptism during the period 1813–1852.

It should be clear to the reader that the above summary indicates a general agreement between the Colyton findings as analysed in this paper, and the results of the census/civil register and baptism/civil register comparisons. The average of the three percentages of birthplace statement

errors mentioned for Colyton is 3.4 per cent and because such an average tends to overweight the higher figures, the 3.2 per cent figure emerging from the census/civil register work can be seen as a high point of convergence. There is precise agreement on the proportion of missed matches on account of name variance – 1.2 per cent – and the number of age statement errors: 1.2 per cent of statements out by more than three years, and following the Colyton data, 0.9 per cent out by more than five years (which was the age boundary of a match in the census/baptism comparison method). There is somewhat less agreement in the figures on children baptised outside of the parish of birth: the 4.7 per cent Colyton figure is significantly higher than the 2.5 per cent figure for the 19 parishes in the post-civil registration period, and also higher than the estimate of 2.9 per cent for the ten parishes during 1813–52, or even than the 4.0 per cent for the whole of the eleven parishes including Kingston. The latter figure does however, represent the best estimate for the whole sample studied, and therefore again represents a point (although a high one) of convergence.

What are the implications of the above findings for the census/baptism register comparison method? The proportion in the linked baptism register was in the original study 31.0 per cent. If we take the above figures which bring together the conclusions of the Colyton and census/ civil register/baptism studies, we would have to subtract 9.3 per cent (3.2 + 1.2 + 0.9 + 4.0) from the 31.0 in order to correct for deficiencies in the census and other problems. However, as has already been mentioned in discussing the Colyton results, this makes no allowance for under-registration on account of infant mortality before baptism. The three parish registers with birthdate information – Kingston, Iver and Medmenham – can yield data on birth/baptism delays during the post-civil registration period when detailed information on infant mortality is available.

Table 10. Birth/Baptism Delay in Kingston, Iver and Medmenham, 1838–1855

	0–6 days	*7–13 days*	*14–22 days*	*23–29 days*	*30 days*	*2–3 months*	*3–6 months*	*6–12 months*	*1 Year +*
Number	9	5	40	123	209	57	45	8	20
Percentage	1.75%	1.0%	7.8%	23.8%	40.5%	11.0%	8.7%	1.6%	3.9%

The *Registrar-General's Eight Annual Report* (1845), gives a monthly breakdown of deaths of infants under the age of one for the period 1839–44, but this is insufficient for our purposes as much of infant

mortality is known to be concentrated in the first days of life. The earliest information giving this level of detail for England and Wales is for the year 1905. If we compare the distribution of infant mortality between the two periods, we obtain the following Table:

Table 11. Monthly Percentage Distributions of Deaths of Infants Under 1 Year of Age[35]

	Under 1 month	*1–2 months*	*2–3 months*	*3–6 months*	*6–9 months*	*9–12 months*
England, 1839–44	30.9%	11.5%	8.1%	19.1%	15.7%	14.7%
England & Wales, 1905	32.6%	10.9%	8.5%	19.3%	15.6%	13.1%

The two distributions are very similar, and even the absolute levels of infant mortality in the two periods are not greatly different: 150.1 per 1,000 in 1839–44, and 128.2 per 1,000 in 1905. In order to use the detailed information in the 1905 data, I have taken its distributions but inflated them by the ratio 150.1/128.2 in order to correct for these differences in absolute levels. The results of these calculations are contained in Table 12.

Table 12. Estimated Mortality Rates for Infants Under 1 Year in England, 1839–1844

	Under 1 week	*1–2 weeks*	*2–3 weeks*	*3–4 weeks*	*1–2 months*	*2–3 months*	*3–6 months*	*6–12 months*
Mortality Rate Per 1,000 Births	29.5	7.1	6.9	5.4	16.3	12.8	29.0	21.6

If we apply these rates to the birth/baptism data contained in Table 10, and make the conservative assumption that none of the percentage groups in the various time periods were exposed to the mortality rates within the periods,[36] we arrive at the following Table of the numbers dying:

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[35] For the source of the data in this Table see the *Registrar-General Eighth Annual Report* (1845), pp. 187, 268; the *Registrar-General's Sixty-Eighth Annual Report* (1905).

[36] Other assumptions about an even distribution of mortality within time periods would only have marginal effects on the estimates, as mortality tends to be concentrated in the first few weeks when the vast majority of children remain unbaptised.

Table 13. Estimated Mortality Rates of Infants Dying Before Baptism, 1839–44

	Under 1 wk	*1–2 wks*	*2–3 wks*	*3–4 wks*	*1–2 mths*	*2–3 mths*	*3–6 mths*	*6–12 mths*	*1 Yr +*	*Total*
Proportion of Total Born Exposed to Mortality before Baptism	98.3%	97.3%	89.5%	65.7%	25.2%	14.2%	5.5%	3.9%	2.9%	
Mortality Rate per 1,000 Born	29.0	6.9	6.2	3.5	4.1	1.8	1.6	0.8	0.6	54.5

The total estimated mortality of infants dying before baptism in the period 1839–44 is 54.5 per 1,000 born, i.e. 5.45 per cent. Can we assume that the same proportion of under-registered births on account of infant mortality before baptism in pre-civil registration periods? In my first paper on the census/baptism comparison method, I published distributions of birth/baptism delays on 311 cases in the sample of 45 parishes where this was available (covering the period 1760–1834). If we apply the estimated mortality rates contained in Table 13 to this distribution, the total estimated mortality of infants dying before baptism drops to 51.4 per 1,000 (5.14 per cent). This slight drop occurs in spite of a significantly smaller birth/baptism delay in the pre-civil registration era, and this is because much of infant mortality is concentrated in the first few days after birth which was not affected by the shortening of the interval between birth and baptism. A higher overall level of infant mortality would of course have a significant effect on the proportion of under-registered infants, but this should not be exaggerated. Even if the infant mortality rate was as high as 250 per 1,000 in the earlier period, the proportion of infants dying before baptism would only be of the order of 8.6 per cent, i.e. an increase of 3.5 over the estimated 5.1 per cent unregistered during 1760–1834.

We can therefore estimate that between 5.1 per cent and 8.6 per cent of unregistered births omitted because of infant mortality before baptism, ought to be added onto the general N.I.R. ratio, but this is not the only positive correction that ought to be made to the N.I.R. rate: D. C. Levine pointed out in a paper on the census/baptism register comparison method that as formulated in the first paper it risked "crediting a successful cross-match when the baptism ... used referred to someone who had

subsequently died".[37] This problem of identifying a correct match was dealt with in the first paper by using independent information on parents names, although the actual data derived on parents was not used to make corrections to the N.I.R. ratio, because of a desire to standardise the cross-matching procedure. If we make these corrections, it becomes necessary to add a net figure of 3.4 per cent to the overall N.I.R. rate. The additions on account of infant mortality before baptism (5.1–8.6 per cent) and over-matching of incorrectly identified cases (3.4 per cent), in effect balance out the 9.3 per cent of cases not found in baptism registers on account of census and other errors.

The conclusions of this paper are therefore as follows: (i) the 1851 Census is very reliable, with birthplace errors for Colyton and Kingston, Black-Torrington, Chigwell, Hackney and Ringwood no higher than 3.2 per cent of the total, and significant name variations and age errors greater than 5 years (the N.I.R. criteria) of the order of 1.2 per cent and 0.9 per cent respectively; (ii) the practice of baptising outside of the parish of birth in 19 parishes selected from the original sample of 45 was very limited: only 2.5 per cent of all children baptised in the parish were born outside of them during the 1840s, and it is estimated that no more than 4.0 per cent of children were baptised outside of their parish of birth during the period 1813–52; (iii) applying the national infant mortality figures from the civil register and data on birth/baptism delays in the parishes of Kingston, Iver and Medmenham, we find that 5.45 per cent of infants died before the time of baptism and this figure is not likely to have been higher than 8.6 per cent for the pre-civil registration period, even assuming a much higher rate of infant mortality; (iv) making allowances for the 3.4 per cent of cases which were over-matched in the original N.I.R. sample, the proportion of N.I.R. cases due to census errors of birthplace, name and age, and the practice of baptising outside the parish of birth, appear to be of the same order of magnitude as missed cases not registered through baptism on account of infant mortality before baptism and the over-matching of cases because of the standardisation of procedure.

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[37] D. C. Levine, "A re-evaluation of baptism as a form of birth registration through cross-matching parish register and census data; together with some proposals for remedying gross deficiencies", *Cambridge Colloquium On Historical Demography And Social Structure* (1973), fn. 17.

This conclusion cannot of course be final, as a great deal more work must be done on a more representative sample of parishes.Also, only the most laborious research which compared census/baptism and civil register data for a systematically covered number of neighbouring parishes could hope to come up with final conclusions. These will probably involve other sources of data as well, so that the cross-comparison methodology will eventually be applied to all relevant surviving historical sources of information. For the present it can be concluded that although there are a number of deficiencies and problems associated with the census/baptism comparison method, because of the counter-balancing effects of factors which led to an under-estimation of the N.I.R. ratio, it provides a very simple standardised way of evaluating the adequacy of particular Anglican baptism registers, as well as forming the basis of a more general assessment of changes in registration accuracy over time.[38]

[38] There has been some misunderstanding on the conclusions reached from the overall N.I.R. rates; it has been assumed, for example, by Wrigley and Levine that the census/baptism method should take account of additional factors such as non-conformist baptism registration. This misses the fundamental point about the cross-matching methodology: it was designed as an economical method by which Anglican baptism could be evaluated as a form of birth registration. The logic behind this was that so much historical research has depended, and inevitably will depend in the future, on Anglican records.

Chapter 6

A Critique of "An Interpretation of the Modern Rise of Population"[1]

This essay is a critique of the work of McKeown, Brown and Record, who argued that an increase in the per capita consumption of food was the key variable in explaining the fall in mortality in the eighteenth and nineteenth centuries. In this paper I present evidence to show there was little or no increase in food consumption during the first four decades of the nineteenth century, when mortality appears to have been declining rapidly.[2]

The finding of a fall in mortality during the first half of the nineteenth century led me to revise my thinking on the role of inoculation. The latter was generally practised at the end of the eighteenth century, and therefore cannot have accounted for nineteenth-century mortality decline. In the present essay I examine the evidence for improving personal hygiene, and how this may have been a factor in improved health and reduced mortality during the early nineteenth century.

In their article "An interpretation of the modern rise of population in Europe",[3] McKeown, Brown and Record have attempted to generalise conclusions drawn from a study of post-registration data for England and Wales to a number of other European countries – Sweden, France, Ireland and Hungary. Their mode of argument takes the form of a hypothetical discussion of the plausibility of various medical and non-medical explanations of population growth since the eighteenth century, based not on detailed

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1 First published in *Population Studies*, Vol. 28 (1974).

2 See Chapter 4.

3 Published in the November 1972 edition of *Population Studies.*

historical or statistical evidence for the relevant period, but derived from conclusions reached from the study of later national civil registration returns. This type of analysis is grounded on the apparent belief that demographic and medical evidence before civil registration is valueless. The recent proliferation of detailed demographic studies using new techniques of analysis and sources of data is dismissed with the bald statement that for "the pre-registration period ... national data are not available. It is hardly possible to read the literature of the past two decades without being acutely aware of the deficiencies."[4] In defence of McKeown *et al.*, it might be argued that very little of this new work – for example, family reconstitution – has yet been published. However, I want to argue in this paper that sufficient new material has been made available to cast very serious doubt on their thesis; I will confine my discussion mainly to the evidence for England and Wales, which will allow an examination of the key point of origin of their analysis. Their thesis has been summarized as follows:

> population growth was not influenced by improved sanitation before about 1870 or by specific medical measures before the introduction of the sulphonamides in 1935 ... the rise of population in the eighteenth and early nineteenth centuries (was due) to a decline of mortality which resulted from improvement of diet ... [through] a large increase of food production.[5]

The thesis assumes that it was a decline in mortality rather than an increase in fertility that was responsible for the increase in population. Wrigley has recently published family reconstitution work on the parish registers of Colyton and Hartland which indicates that increasing fertility was important as a source of increasing population; two parishes, however, are obviously not adequate as a sample for national generalizations and it will be necessary to wait for the completion of the Cambridge Group's work before generalization can be made from this material, and even then findings will have to be evaluated in the context of a number of technical demographic considerations. In my recently published paper on the reliability of parish register data, I have calculated national birth and death rates from parish register and census returns

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[4] T. McKeown, R. G. Brown and R. G. Record, "An interpretation of the modern rise of population in Europe", *Population Studies*, Vol. 26, (1972), p. 345.
[5] *Ibid*, p. 341.

for the period 1801–40;[6] these rates have not only been derived through a direct analysis of the reliability of register data, but have been evaluated through detailed census information. Although further work will be required before they can be accepted as being definitive, there are substantial grounds for taking them as a basis of discussion. The birth and death rates for England and Wales calculated on the basis of corrected population returns are as follows:[7]

Period	*Birth Rate (per 1,000 population)*	*Death Rate (per 1,000 population)*
1800–10	41.4	30.5
1811–20	42.0	29 8
1821–30	40.1	25.9
1831–40	35.9	22.8

As there was little change in the age structure of the population during this period according to the 1821 and 1841 age censuses, it is safe to conclude that there was a significant fall in mortality during this period.

The data so far considered support the argument developed by McKeown *et al.*, in their paper about the importance of diminished mortality. Further evidence, however, fundamentally contradicts their thesis. We would expect, if food supply was the crucial variable, mortality reductions to be concentrated almost exclusively amongst the poorer sections of the community. Wealthy groups such as the aristocracy should be unaffected if the food supply hypothesis were true, yet one of the most consistent conclusions of recent historical demographic work is that there were marked reductions in mortality in just such groups as the aristocracy. For England and Wales the most reliable evidence is to be found in Hollingworth's study of the British peerage: he found that there were significant falls in mortality throughout the eighteenth and nineteenth centuries. This conclusion can be summarized by the following figures of changing expectation of life at birth for aristocratic females:[8]

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[6] See Chapter 4 of this book.

[7] See Chapter 4.

[8] T. H. Hollingsworth, "The demography of the British peerage", *Supplement to Population Studies*, Vol. 18, (1964) p. 57.

Cohort born	*Expectation of Life at Birth (Years)*
1700–24	36.3
1725–49	36.7
1750–74	45.7
1775–99	49.0
1800–24	51.7
1825–49	58.4

Reductions in mortality occurred across the whole range of age groups, although there was a particularly significant fall amongst young children under five.[9] Similar results have been found in the study of the mortality of gentry families during the same period[10] and for the "middling and higher classes" who were nominated for the government life annuities and tontines during the eighteenth century.[11] Peller in his large-scale study of Europe's ruling families came to almost identical conclusions: expectation of life of females in these families rose from 33.7 years in the seventeenth century to 38.2 years in the eighteenth and 48.1 years in the first half of the nineteenth.[12]

It is surprising in a way that these findings have not received more attention from historical demographers and medical historians than they have. Clearly, from a methodological point of view, no general explanation of population growth during the eighteenth and nineteenth centuries which cannot explain the falls in mortality among the wealthy aristocratic and upper-class families can be considered adequate. This does not mean that one can rule out explanations like increases in food supply, for it is possible that these were important for some groups in the population and not others. But the scale of the falls in mortality amongst upper-class groups which could account for more or less the whole population increase in England and Wales if occurring amongst the general population suggest that the food supply hypothesis should be looked at very critically.

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[9] *Ibid*, p. 55.

[10] See Chapter 1.

[11] P. E. Razzell, *The Role Of Smallpox Inoculation In The Growth Of Population In Eighteenth-Century Britain* (Oxford University D.Phil. Thesis), p. 54. See also the discussion of tontines in Chapter 7.

[12] S. Peller, "Births and deaths among Europe's ruling families since 1500", D. V. Glass and D. E. C. Eversley (eds.), *Population In History* (London, 1965), p. 95.

McKeown *et al.* unfortunately do not discuss the evidence for the food supply hypothesis in any detail; with respect to England and Wales they merely state that "in relation to interpretation of population growth, it is unnecessary to consider in detail the timing and character of the changes in agriculture which led to the increase in food supplies".[13] In their general review of the evidence, they note that "British agriculture was not only feeding many more people; it was, at least until 1767, feeding them better".[14] As an elaboration to this conclusion, they note in a footnote that Deane and Cole in their *British Economic Growth 1688–1959* "suggest that production of cereals and meat *kept pace* (my italics) with population growth until the last decade of the century".[15] Yet it was during the latter half of the eighteenth century when food consumption was at best static (it may well have been decreasing) that population began to accelerate, whereas during the first half of the century when food consumption per head increased, population was more or less stationary.[16] It is not sufficient for food supply to keep pace with population to bring about a reduction of mortality – it is necessary for consumption per head to increase so as to improve health. The evidence available for the eighteenth century is much more consistent with a reversed hypothesis – that standard of diet was a function of population change.

The evidence on food consumption per head tends to be much more reliable for the nineteenth century than for the eighteenth, and this is partly a function of the fact that accurate population figures are available from 1801 onwards as a result of the introduction of the national census. This is of particular relevance for the present paper which has traced a sharp decline in mortality during the first 40 years of the nineteenth century. Mitchell and Deane have published a series of figures which come nearest to a set of

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[13] S McKeown, Brown and Record, *loc. cit.*, p. 353 fn. 1.

[14] *Ibid*, p. 352.

[15] *Ibid*, footnote 26.

[16] There is little direct evidence on food consumption per head in the eighteenth century. The series of real wage indices for London and Lancashire building labourers published by Mitchell and Deane suggest the conclusion reached in the text: 1703–07 – 227; 1743–47 – 280; 1783–87 – 250. See B. R. Mitchell and P. Deane, *Abstract Of British Historical Statistics* (1962), pp. 346, 347. (1994): Since the above was written, new evidence suggests that population did begin to increase significantly at the beginning of the eighteenth century. See Chapter 7.

reliable national indices of United Kingdom consumption per head for this period:[17]

	Coffee (Pounds)	*Tea (Pounds)*	*Sugar (Pounds)*
1801	0.07	1.49	22.53
1811	0.53	1.24	23.15
1821	0.36	1.27	18.19
1831	0.94	1.24	18.92
1841	1.06	1.37	16.99

There was a very sharp rise in coffee consumption, but a fall in both tea and sugar consumption during this 40-year period. The rise in coffee consumption is unlikely to have had much effect on the standard of diet of most of the poorer section of the population, as coffee drinking during this period was more a middle-class than a working-class habit.[18] The decline in tea and sugar consumption per head is matched by a similar decline in the consumption of beer: 1800–4 – 33.9 gallons; 1810–14 – 30.2; 1820–24 – 29.0; 1831 – 21.3; 1841 – 19.6.[19] This was during a period when the custom of home brewing was waning and so the decline in beer drinking would be even greater than indicated by these figures.

To some extent, of course, figures for the commodities considered so far are secondary to the more basic ingredients of diet such as bread and meat – although as G. R. Porter pointed out, consumption of commodities such as sugar, tea and coffee formed a useful index of the ability of the ordinary population to consume above the subsistence level.[20]

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[17] *Ibid*, pp. 355, 356.

[18] See John Burnett, *Plenty And Want* (1966), p. 10. Perhaps the best single piece of evidence on this is the survey of diet among agricultural labourers and unskilled town workers conducted by Dr Edward Smith in 1863. Only 41 per cent of agricultural labourers consumed any coffee whatsoever and the figures for town workers were at the same relatively low level. The average amount of coffee consumed by these workers' families was about 1 ounce a week, compared to a consumption of about 2.5 ounces of tea. See Dr Edward Smith, *Report On The Food Of The Poorer Labouring Classes In England; Sixth Report Of The Medical Officer Of The Privy Council,* (Parliamentary Papers, 1864, XXVIII.)

[19] Burnett, *op. cit.*, in footnote 17, p. 12.

[20] *Ibid*, p. 10.

Unfortunately, there are no exact figures of the national consumption of basic commodities such as bread and meat during the period under discussion, although none of the estimates made of consumption per head of these commodities has suggested a rise. R. N. Salaman in his study of the social history of the potato estimated consumption per head of wheat and potatoes as follows:[21]

Wheat *United Kingdom*		*Potatoes* *England & Wales*	
1770s	1.36 pounds	1795	0.40 pounds
1830s	0.90 pounds	1838	0.62 pounds

Although on these figures there was an increase in the consumption of potatoes, this was more than matched by a decline in wheat consumption. The agricultural historian L. Drescher has estimated "that wheat cultivation in England and Wales increased from 3 to 3.8 million acres between 1798 and 1846, and that yields increased from 20–24 bushels per acre to 32–34 bushels ... wheat production just failed to keep pace with population".[22] Since Drescher made these estimates of wheat production, the agricultural historians Healy and Jones have confirmed his estimate of the scale of change in wheat yields during the period relevant to the present analysis. They have published a series of wheat yield figures which were derived from direct observations in a number of different parts of the country using a standard box measure of yield. According to these statistics there was an increase from 31.2 bushels per acre in 1815–19 to 36.6 bushels per acre in 1837–41 (this increase from the first to the last five years is representative of the trend over the whole period).[23] This increase represents a proportionate change of 17.3 per cent, which must be set against a population increase of 36 per cent during the same period.[24] All this evidence suggests a decline rather than an increase in bread consumption during the first 40 years of the nineteenth century.

This conclusion is also generally applicable to the consumption of meat. Deane and Cole have published estimates of sheep and cattle production

[21] Mitchell and Deane, *op. cit.*, in footnote 15. p. 358.

[22] R. M. Hartwell, "The rising standard of living in England, 1800–1850", *Economic History Review*, Vol. 13, (1960–61), p. 408.

[23] M. J. R. Healy and E. L. Jones, "Wheat yields in England, 1815–59", *Journal Of The Royal Statistical Society,* Series A, 125 (1962), p. 578.

[24] Mitchell and Deane, *op. cit.*, in footnote 15, p. 8.

which suggest a decline in consumption per head of both: 26.7 million sheep in England and Wales in 1800, declining to 24.0 million in 1841; under 32 million cattle in England and Wales in 1779 which had only increased to 5.2 million for the whole of the United Kingdom by 1832 – and the evidence which does exist suggests no increase in the weight of cattle.[25] The only direct statistical evidence available for this period confirms this conclusion about declining meat consumption per head. The following figures are for the cattle and sheep slaughtered at Smithfield market:[26]

	Cattle ('00s)	*Sheep ('000s)*	*London's Population ('000s)*
1799–1803	125	793	1,117
1839–1843	177	1,443	2,239

The slaughter of sheep kept pace with the growth of London's population but there was a marked decline in the proportion of cattle slaughtered per head of population. The overall conclusion from this review is that with the exception of coffee (which was mainly a middle-class drink) and potatoes, there is no evidence to support McKeown's thesis that there was an increase in average food supply in Great Britain during the latter half of the eighteenth and first half of the nineteenth century; on the contrary, the evidence that does exist suggests a decrease in food consumption per head.

In the light of the above considerations, it is all the more important to consider carefully all possible medical explanations of falling mortality. McKeown and colleagues have not examined the medical and historical evidence in detail, but rather have come to overall conclusions on the basis of a priori general considerations. A major example of this is their brief comment on the smallpox inoculation hypothesis; they write that in their view there are three objections to the idea that smallpox inoculation had a significant effect in reducing mortality:

> it assumes a substantial and prolonged decline of the disease which cannot be confirmed by national data; it postulates an effectiveness of inoculation which is not accepted by virologists who know smallpox; and it attributes to this crude and dangerous measure an influence on

[25] Deane and W. A. Cole, *British Economic Growth* (1969), pp. 69, 195.
[26] Mitchell and Deane, *op. cit.*, in footnote 15, pp. 19, 354.

total mortality which would not be expected from any modern immunization procedure, supported by the full resources of the laboratory and health education.[27]

It is impossible to comment in any detail on points that raise so many complex issues; I have dealt with the subject at length in my book on the history of smallpox inoculation[28] and I will briefly comment here on some of the points made by McKeown and his colleagues:

(1) Although there are no national data for England and Wales during the eighteenth and early nineteenth centuries, there is an abundance of local and literary evidence which demonstrates a radical reduction in smallpox mortality. The best single piece of evidence for the long-term decline in smallpox mortality is the series of smallpox censuses sponsored by Jurin and others during the 1720s; the survey covered towns and six villages in different parts of the country and although not randomly selected, included a very careful study of 13,192 cases of smallpox. Out of this total number of cases of people who caught smallpox, 2,167 are recorded as having died – a case fatality rate of 16.5 per cent.[29] Given the fact that smallpox was a universal disease – reflected in its mainly childhood incidence – a very significant proportion of the population died from the disease. The above figures are likely to minimize mortality for a number of reasons discussed elsewhere.[30] As smallpox deaths formed only 1.5 per cent of deaths per 100 children born during the period 1838–44,[31] (the first available national statistics after the introduction of civil registration) it is clear that there was a very significant decline of mortality. The national statistics for Sweden during the latter half of the eighteenth century confirm this conclusion about the severity of smallpox before the effective introduction of inoculation and vaccination;[32] similarly for other countries considered

[27] *Ibid*, p. 142.

[28] Peter Razzell, *The Conquest Of Smallpox* (1977).

[29] *Ibid*, p. 130.

[30] *Ibid.*

[31] *Ibid*, p. 157.

[32] In the ten year period 1757–66 when national statistics were first compiled, there were 64,956 smallpox deaths out of a total of 543,212 cases in Sweden (the number of smallpox deaths at this time, however, included deaths from measles). See *Royal Commission on Vaccination, First Report*, (1889), p. 112.

by McKeown there is evidence to support the same conclusion, e.g. 21 per cent of all deaths in Dublin during the period 1660–90 were from smallpox.[33]

(2) On the question of the effectiveness of inoculation, no medical historian has ever questioned the effectiveness of inoculation in preventing further attacks of smallpox during the lifetime of the person inoculated – unlike vaccination which gave a limited period of protection against further attacks – and this is a function of the degree of attenuation of the virus injected and the number of antibodies produced in reaction to the injection. Occasionally eighteenth-century writers noted that somebody inoculated had a much later attack of smallpox, but this was obviously a very rare event, because of the amount of comment it provoked.

(3) The notion that inoculation was a 'crude and dangerous measure' is also not supported by the historical evidence. It is true that early inoculation did in a very small proportion of cases lead to the death of the person inoculated, and that it did occasionally spread the disease to those not protected against it. It is the latter point which has led most medical historians to reject the argument that inoculation reduced smallpox mortality; in fact there is evidence that inoculation only very rarely spread the disease and even where it did, it was irrelevant from a demographic point of view because smallpox was already a universal illness affecting the total population. In fact, ironically these isolated cases of inoculation spreading smallpox provoked 'general inoculations' of whole communities based on the fear that the uninoculated would be particularly vulnerable – and this fear even provoked some parish authorities to resort to compulsory inoculation.[34] Also, as the technique of inoculation developed throughout the eighteenth century, it became safer both with respect to the dangers of dying from the operation and in connection with the risk of spreading the natural disease. There is evidence to suggest that early vaccination was in fact only a more attenuated form of inoculation, both in the nature of the injection and in the effects produced.[35] When McKeown *et al.* question

[33] *Ibid*, p. 137.

[34] *Ibid*, pp. 55, 68.

[35] Peter Razzell, *Edward Jenner: The History Of A Medical Myth* (1978).

> the effectiveness of inoculation on the grounds that it was not "supported by the full resources of the laboratory and health education", they also question the effectiveness of vaccination, which has been universally accepted as having combatted smallpox in a highly effective way.

Although I have emphasized the role of smallpox inoculation and vaccination in reducing mortality (inasmuch as there was a difference in these two injections during the early period, the former was more important in Great Britain and the latter in the rest of Europe), it is clear from the demographic evidence quoted earlier that this particular explanation cannot explain the total fall of mortality during both the eighteenth and nineteenth centuries. Inasmuch as inoculation eliminated smallpox during the late eighteenth century, it, or vaccination, could not have been responsible for the very significant fall in mortality as measured by the national crude death rates for England and Wales during the period 1801–41 or the sharp falls in mortality amongst the aristocracy during the same period. Even on the inoculation hypothesis, however, some of the reduction in smallpox mortality would have occurred during the early nineteenth century, but only to an insignificant extent. It is necessary in the light of these considerations to re-examine the literature on improvements in health during the century 1750–1850 to see whether it is possible to put forward additional hypotheses to explain the unresolved significant decline in mortality. Such hypotheses must by their very nature be tentative and act only as pointers to further research.

II

One obvious factor in the improvement of health which could account for reduced mortality amongst all sections of the community is a qualitative change in the environment. During the late eighteenth and early nineteenth centuries contemporaries were virtually unanimous in agreeing that the drainage of marshland through the reclamation of land associated with agricultural improvements led to an improvement in health. It would appear from the description of the illness eliminated through drainage works that in many cases it must have been a mild form of malaria – most accounts emphasize the debilitating effects of the disease on the agricultural working population rather than direct mortality from the

illness.[36] However, there is some statistical evidence that mortality was significantly higher in the marshy areas of the fenlands of East Anglia than in dry areas elsewhere, and Wrigley has suggested that in low-lying badly drained areas it might have been difficult to avoid diseases like tuberculosis and typhoid, as well as malaria.[37] This is obviously a hypothesis which must be explored through further detailed work on particular parishes, but it is difficult to see how it could apply to more than a very small area of the whole country.

Another explanation in the same general category is the improvement of the environment through deliberate measures, such as better drainage of towns, widening and cleansing of streets. There are a number of difficulties against accepting this particular explanation as being important in accounting for reduced mortality. First, only about 20 per cent of the total population lived in towns with a population of more than 10,000 in 1801, and therefore town improvements could hardly have been decisive. Second, it is difficult to believe that the public health of the Victorian town was much improved over that in the eighteenth century: the average death rate of Birmingham, Leeds, Bristol, Manchester and Liverpool in 1840 was 30.8 per 1,000,[38] compared with a national average of 22.9.[39] Even if there had have been an improvement in the public health of towns, this might have been more than outweighed by the fact that a larger proportion of the total population were living in the relatively unhealthy towns, compared to country areas – by 1851 about half the population were living in towns of 10,000 and above. Third, there is some evidence that town improvements were confined largely to the areas mainly inhabited by the wealthy rather than the poor; for example, Dr Southwood Smith stated in his report of 1838:

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[36] For a good discussion of malaria in England see M. C. Buer, *Health, Wealth And Population* (1926), pp. 210, 222. *The Statistical Account Of Scotland* which was published in the 1790s and ran to 21 volumes, contains a number of references to the disappearance of malaria due to land drainage and reclamation. Usually the accounts emphasise the loss of working time through the disease rather than any mortality resulting from it.

[37] E. A. Wrigley, *Population And History* (1969), p. 100.

[38] G. Talbot Griffith, *Population Problems Of The Age Of Malthus* (1967), p. 186.

[39] Mitchell and Deane, *op. cit.*, in footnote 15, p. 36.

> While systematic efforts on a large scale have been made to widen streets, to remove obstruction to the circulation of free currents of air, to extend and perfect the drainage and sewerage, and to prevent the accumulation of putrefying animal and vegetable substances in the places in which the wealthier classes reside, nothing whatever has been done to improve the conditions of the districts inhabited by the poor.[40]

Of course, this improvement in the environment of the wealthy might go some way in explaining the diminishing mortality amongst the aristocracy during the first half of the nineteenth century, but the limitations of such an explanation as far as the general population are concerned are self-evident.

The relative ineffectiveness of public health measures before the latter half of the nineteenth century is readily understandable in the context of the British social structure – weak central and local authorities within a tradition of laissez-faire individualism. The emphasis on public health measures in previous attempts to explain reductions in mortality are not based on empirical evidence – a part of a tradition in social history which lists a number of theoretically possible explanations, particularly those couched in terms of intentionally designed improvements. McKeown et al. have quite rightly criticized this type of explanation with respect to developments of medical knowledge. In fact, it could be argued that most medical advances have occurred through empirical trial-and-error, rather than through sophisticated theoretical advances. Ironically, McKeown, Brown and Record themselves fall victim to this over-emphasis on sophisticated scientific medicine; for example, they insist that modern laboratory methods are necessary to reduce mortality significantly, when it is clear that empirically evolved methods such as inoculation and vaccination were highly effective against smallpox without being based on a sophisticated scientific technology.

There were a number of empirical medical discoveries developed during the eighteenth century which are now known to be scientifically valid. The discovery that citrus fruit was effective in eliminating scurvy and that cod liver oil prevented the development of rickets both fall in this category. There is little evidence, however, that these diseases were ever

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[40] S. and V. Leff, *From Witchcraft To World Health* (1958), p. 170.

particularly fatal for the population at large, or that the remedies were ever applied on a large scale in the period in which we are interested.[41] It is possible that the increased consumption of potatoes could have diminished the effects of rickets through increasing intakes of vitamin D, but there is evidence that rickets persisted amongst the population at large into the twentieth century.[42]

There remains one empirical discovery made in the eighteenth century which I shall argue in this paper could have had a very marked effect on health and made a substantial contribution to reducing mortality – although it is likely that improvement in health came about not so much through the deliberate application of the discovery but rather through its utilization on social and economic grounds. From about the middle of the eighteenth century onwards, a number of medical pioneers began to teach, through their writings and practice, the importance of hygiene and general cleanliness: Sir John Pringle discovered the importance of hygiene for preventing dysentery in army camps, James Lind demonstrated how it was possible to prevent typhus in navy hospitals and ships through rules of hygiene, and Sir Gilbert Blane reduced the incidence of hospital fever through an insistence on scrupulous cleanliness.[43] All these innovations were limited to institutions where some degree of centralized authority made it possible to impose rules of hygiene from above, and so necessarily only influenced a small proportion of the total population. However, the emergence of the dispensary and Lying-In movements at the end of the eighteenth century led to a wider diffusion of this principle of hygiene. Lettsom for example claimed that the influence of the General Dispensary in London had brought about improvements in the way many ordinary people treated their sick relatives, by encouraging cleanliness and better personal hygiene.[44] Lettsom also believed in that "in the nurture and management of infants, as well as in the treatment of lying-in women, the reformation hath equalled that of the smallpox [through inoculation]".[45] A

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[41] This conclusion is based on a reading of Burnett, *op. cit.,* footnote 17, and J. C. Drummond & Anne Wilbraham, *The Englishman's Food* (1957).

[42] Burnett, *op. cit.*, in footnote 17, p. 272; Drummond and Wilbraham, *op. cit.*, in footnote 40, p. 439.

[43] Buer, *op. cit.,* in footnote 35, pp. 119, 120.

[44] Griffith, *op. cit.*, in footnote 37, p. 224.

[45] Buer, *op. cit.*, in footnote 35, p. 150.

part of this reformation had consisted of an insistence on cleanliness, which undoubtedly would have helped to reduce mortality.

In order for this new attitude towards hygiene to affect more than an institutional minority it had to be widely diffused at the individual level; yet Willan writing in 1801 noted that "most men resident in London and many ladies though accustomed to wash their hands and face daily, neglect washing their bodies from year to year".[46] After this date, however, the situation changed radically and I shall argue in this paper that it was an improvement in personal hygiene rather than a change in public health that was responsible for the reduction in mortality between 1801 and 1841. I should emphasize that this argument will be presented very much in the form of a hypothesis, partly because there has been no serious scholarly study of the social history of personal hygiene. The subject has traditionally been treated as a source of amusement and has been presented in the context of social history as entertainment. I will initially outline in summary form current medical opinion on effects of personal hygiene on health and subsequently present fragments of evidence supporting the notion that there was a marked change in personal hygiene during the first half of the nineteenth century.

There are basically two classes of disease which are affected by personal hygiene: (i) diseases of the intestinal tract, and (ii) diseases transmitted from person to person by body lice. In the first category the most important diseases relevant to the period under discussion were probably (a) gastro-enteritis, (b) typhoid fever, and (c) dysentery; and in the second category (d) typhus, (e) relapsing fever, and (f) trench fever. Personal hygiene basically affects the former through the transmission of the pathogenic organisms in the faeces via the hands or through flies; it affects the latter through the cleanliness of the body and clothing which determines whether the body louse can survive or not. In addition, personal hygiene can prevent secondary bacterial infection which can influence the outcome of other diseases not mentioned above. Personal hygiene prevents both classes of disease: for example, in the case intestinal diseases, "all pathogenic organisms will be removed if they are washed in soap and water";[47] in the case of lice-borne infections hygiene is crucial because "lice are not usually found on the bodies of human beings who possess the

[46] *Ibid*, p. 62.

[47] Ronald Hare, *Bacteriology And Immunity For Nurses* (1967), p. 79.

necessary facilities for keeping themselves clean. But when it becomes impossible to obtain soap, hot water and a change of clothing, they almost always become infested with lice".[48] It is not necessary here to discuss in any greater detail the actual processes through which the diseases are transmitted from person to person; it is sufficient for the present purposes to indicate the consensus of medical opinion on the importance of personal hygiene for preventing the diseases under discussion. The earlier quotation from Willan's writings in 1801 indicated that even amongst the genteel population of London personal hygiene was of a very low standard; the women, unlike the men, at least appeared to have washed their hands and faces daily. According to Lawrence Wright, who has written a general social history of personal hygiene, this was typical of the state of cleanliness amongst all social classes before the beginning of the nineteenth century. Wright points out that Pepys only once mentioned his wife having a bath in the nine years he kept his diary:

> My wife busy in going with her woman to the hot house to bathe herself, after her long being within doors in the dirt, so that she now pretends to a resolution of being hereafter very clean. How long it will hold I can guess.[49]

Amongst the advice that Lord Chesterfield gave to his son was the following on personal hygiene:

> Washing yourself, and rubbing your body and limbs frequently with a flesh-brush will conduce as much to health as to cleanliness. A particular attention to the cleanliness of your mouth, teeth, hands and nails, is but common decency, in order not to offend people's eyes and noses.[50]

The fact that Lord Chesterfield felt it necessary to advise his son to wash his hands and mouth although he did not recommend washing his face or body – suggests the lack of the actual practice in this respect. A manual of etiquette dated 1782 merely advised wiping the face every morning with a white linen and warned against washing it in water as that made the face too sensitive to cold and sunburn.[51] Perhaps Dr

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[48] *Ibid*, p. 62.

[49] Lawrence Wright, *Clean And Decent* (1960), p. 76.

[50] Ronald Fletcher, *The Parkers At Saltram 1769–89* (1970), p. 116.

[51] Wright, *op. cit.*, in footnote 48, p. 138.

Johnson was speaking for his age: "Smell, Madam?" he is reputed to have said, "I positively stink".[52]

There is evidence of a somewhat more objective nature for the lack of personal cleanliness amongst the upper classes in the eighteenth century. Not only is there no evidence of the presence of bathrooms in the ground-plans of Georgian houses but "inventories which chronicle the most trivial utensils found in the kitchen, fail to recognize or describe the common bath."[53] It is probable that when people did bathe they, like Mrs Pepys, went to a public bath-house. The history of the wash-basin tends to confirm the literary evidence about the lack of personal cleanliness amongst the "respectable" classes: not until about 1770 did a wash-stand appear that was capable of holding soap and even then the wash-basin itself was of minute proportions.[54] This, however, was a hint of the new cleanliness to come, along with the introduction of Bramah's water-closet in 1778.[55] Before the use of the water-closet, sanitary arrangements even in genteel circles were surprisingly unrefined: one foreign visitor noted that in good society "the sideboard, too, is furnished with a number of chamber pots and it is a common practice to relieve oneself whilst the rest are drinking; one has no kind of concealment ..."[56]

The above evidence, of course, only applies to the wealthier social classes; unfortunately there is little readily available information on hygiene among ordinary people. This kind of evidence is crucial of course not only for explaining changes in overall mortality but even for accounting for decreases in mortality amongst groups such as the aristocracy: most food would be prepared and served by domestic servants – and their personal hygiene would have a marked effect on the health of their employers. It is, of course, very unlikely that the personal hygiene of the ordinary man was any better than that of wealthier groups. Francis Place, who was fascinated by the transformation of manners and morals at the beginning of the nineteenth century, had much first-hand experience of the English working classes and succinctly summarized in 1822 the change that had taken place amongst them with respect to personal cleanliness:

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[52] Fletcher, *op, cit.*, p. 117, fn. 49.

[53] A. S. Turberville (ed.), *Johnson's England, Vol. 2* (1952), p. 130.

[54] Wright, *op. cit.*, p. 112, footnote 48.

[55] *Ibid.*, p. 107.

[56] Fletcher, *op. cit.*, p. 53, footnote 49.

> the change ... has taken place, not only in London, but all over the country, in the habits of the working classes, who are infinitely more moral, more sober, more cleanly in their persons and their dwellings, than they were formerly, particularly the women; partly from the success of the cotton manufactures, which has enabled them to discard the woollen clothes which were universally worn by them, which lasted for years, and were seldom, if ever washed; partly from increased knowledge in domestic concerns, and the nursing and general management of children. Notwithstanding the vice, the misery, and the disease which still abounds in London, its general prevalence has been greatly diminished.[57]

It might be thought that dirty clothing was fairly closely correlated with social class, yet again Dr Johnson in good-humoured fashion suggested otherwise:

> I have often thought that if I kept a seraglio, the ladies should wear linen gowns – or cotton; I mean stuffs made of vegetable substances. I would have no silk; you cannot tell when it is clean; it will be very nasty before it is perceived to be so. Linen detects its own dirtiness.[58]

It is clear from the above descriptions that all sections of the community would have been prone to both intestinal and louse-borne diseases; perhaps the wealthier classes would have been somewhat less affected by the latter because of the greater frequency of changing clothes, but this is an empirical question which can only be settled by more research. Creighton in his study of the history of epidemics in Britain presented evidence to suggest that typhus was more prevalent amongst the poor than the rich during the eighteenth century, but this must be qualified by the fact that fatality of the disease appeared to have been much greater amongst the wealthy.[59] There is also some evidence to suggest that the rich were particularly prone to fevers of various sorts[60] most of which could probably

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[57] Francis Place, *Illustrations And Proofs Of The Principle Of Population* (1930 ed.), p. 253.

[58] Turberville, *op. cit.*, p. 278, footnote 52.

[59] Charles Creighton, *History Of Epidemics In Britain*, Vol. 2 (1965), pp. 102, 134, 141, 266, 290.

[60] C. S. Peel, "Homes and habits", G. M. Young (ed.), *Early Victorian England*, Vol. 1 (1934), p. 84.

be linked to inadequate personal hygiene. Fever was generally regarded by contemporaries as being responsible for a significant proportion of total mortality and the Carlisle Table of Mortality for the years 1779–87 indicates that fever caused at least 10 per cent of all deaths[61] – although the inadequate classification of these diseases makes it difficult to say precisely what form they took. Gastro-enteritis was also responsible for killing large numbers of infants, although again the classification of the cause of death is so imprecise as to make it very difficult to know what proportion of total mortality was due to this disorder.

The London Bills of Mortality show dramatic declines in mortality from fevers and the variously designated infantile complaints – particularly diarrhoea – after the end of the eighteenth century.[62] The Bills of Mortality are, however, unreliable as a source of evidence.[63] Contemporaries certainly believed that greater cleanliness had reduced mortality from these diseases; for example, Blane writing in 1813 referred to the "counteraction of typhus by means of cleanliness and ventilation".[64] According to the Registrar-General's statistics, 'typhus' only accounted for 4.8 per cent of all deaths in the period 1838–42, in spite of the fact that typhus proper was being confused with typhoid and relapsing fever.[65]

The question must, of course, be raised about the actual evidence in favour of the idea that there was a marked improvement in personal hygiene in the first half of the nineteenth century. We have already seen that Place believed that this was the case amongst the working classes and that this was linked to the availability of cheap cottons. Certainly, the price of cotton goods fell dramatically during this period – G. R. Porter quoted the following statistics to illustrate this point: the price paid for weaving 24 yards of Cambric at Stockport fell from 25 shillings in 1802 to ten shillings in 1812, while the selling price of 728 Calicoes fell from £1 8s. in 1814 to seven shillings in 1841;[66] combining these figures suggests that

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[61] Buer, *op. cit.*, p. 269, fn. 35.

[62] *Ibid*, p. 270. Joshua Milne, *A Treatise on the Valuation of Annuities*, 2 vol. (1815).

[63] See the discussion of the unreliability of Hackney parish register – which formed part of the Bills of Mortality – in Chapter 4.

[64] Buer, *op. cit.*, p. 238, fn. 35. See also Griffith, *op. cit.* pp. 227, 228, fn. 37.

[65] Creighton, *op. cit.*, pp. 183, 198, fn. 58. Mitchell and Deane, *op. cit.*, p. 34, fn. 15.

[66] F. W. Hirst (ed.), *G. R. Porter's The Progress Of The Nation* (1912), p. 298.

the 1841 price would only have been about a tenth of that charged in 1802. This dramatic reduction in price led to a marked increase in the consumption of cotton goods: in 1801 52 million pounds of raw cotton imports were retained for United Kingdom production; by 1841 this figure had reached 438 million pounds – an increase of over eight times.[67] As the proportion of cotton goods exported remained more or less constant during this period,[68] domestic consumption of cotton goods per head would have increased by over four times. This did not take place at the expense of other types of cloth: wool, silk and linen all appear to have at least kept pace with population expansion[69] This, of course, is not inconsistent with Place's statement about cotton displacing wool, for whereas twice as much wool as cotton was consumed at the beginning of the nineteenth century, by the end of the 1830s the ratio was more than reversed, with about two-and-a-half times as much cotton being used as wool.[70]

There is also good statistical evidence that personal hygiene improved through the increasing use of soap. Total soap consumption approximately doubled between 1713, when figures are first available, and 1801, the year of the first census: from 24.4 million pounds to 47.6 million pounds.[71] If Gregory King's estimate of population in the 1690s is anything to go by, the population of England and Wales was of the order of 5.5 million at the beginning of the eighteenth century; as population in 1801 was over 9 million, there probably was a modest increase in consumption per head during the century. Most of this increase was concentrated at the end of the century, although without firm population figures it is difficult to be absolutely certain of this. After 1801, it is possible to be very precise about average consumption figures; according to figures published by Porter, soap consumption per head of population nearly doubled between 1801 and 1841, from 4.84 pounds to 9.20 pounds.[72]

The figures published by Mitchell and Deane indicate a somewhat smaller increase – from 6.1 pounds in 1799–1803 to 9.7 pounds in 1841 –

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[67] Deane and Cole, *op. cit.*, p. 179, fn. 24.

[68] *Ibid*, p. 187

[69] *Ibid*, pp. 196, 210; Mitchell and Deane, *op. cit.*, p. 184, fn. 15.

[70] *Ibid*, p. 179; Deane and Cole, *op. cit.*, p. 196, fn. 24.

[71] Mitchell and Deane, *op. cit.*, p. 265, fn. 15.

[72] Hirst, *op. cit.*, in footnote 64, p. 422.

an increase still of the order of 60 per cent.[73] These figures must be treated with some caution; not only was soap produced illegally to escape the excise duty – and this varied during the 40-year period – but soap was used in manufacturing processes as well as for domestic consumption. Nevertheless, the statistics are consistent with the argument that there was a marked improvement in personal hygiene during the period under discussion.

There is some literary evidence to support this conclusion, particularly with respect to the wealthier social classes. Professor Wilson has summarized the transformation in personal hygiene as follows:

> It was the Duke of Wellington who probably did most to spread the fashion for the daily bath among the upper and middle classes. ... By the sixties, a daily bath was usual among those who afford the coal for heating the water and the labour to carry the great jugs from which the hot water was poured into the movable tub. A little lower in the social scale, the bath was a weekly ritual but washing took place daily, and everywhere the wash-hand stand, with its basin, jug, and soap dish, was making its appearance in the Victorian bedroom.[74]

This improvement of hygiene was reflected in the proliferation of various makes of portable bath during the Victorian period,[75] the fixed bath as a part of a bathroom not really existing until the end of the nineteenth century. The absence of a proper bathroom did not stop the Victorians from bathing every day; for example a parson's daughter referring to the period after 1847 wrote that "there were no bathrooms then, and all hot

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[73] Mitchell and Deane, *op. cit.*, p. 265, fn. 15. Professor Charles Wilson in his history of Unilever has given yet a third figure per head for 1801: 3.6 pounds – which is nearer Porter's figure than Mitchell and Deane's. One of the reasons for the increased consumption of soap during this period might have been a decrease in price; the duty on hard soap was reduced from 2s. 3d. per pound in 1801 to ls. 5d. per pound in 1841, but more importantly there is some evidence to suggest that the overall price of soap fell from about 9d. per pound in the 1790s to about 5d. a pound in the 1830s. See Charles Wilson, *The History of Unilever*, Vol. 1 (1954), p. 9; Hirst, *op. cit.*, p. 422, fn. 64; F. M. Eden, *The State Of The Poor* (1928), p. 242; Burnett, *op. cit.*, p. 53, fn. 17.

[74] Wilson, *op. cit.*, p. 6, fn. 71.

[75] Wright, *op. cit.*, p. 165, fn 48.

and cold water had to be carried from the kitchen and scullery. But we all had baths every day in spite of that".[76] Obviously, the mass of the population did not bath every day, partly because in some cases they had to buy water and also because of the expense of heating water. According to one social historian who has made a scholarly study of the Victorian home:

> Servants washed each day and bathed once a week, as did the respectable better-off poor. Those who were neither respectable nor better-off washed when and how they could, or did not wash.[77]

The latter conclusion is later qualified by the statement that the poor "went on trying to be clean, washing in the costly water clothes which when hung out to dry (in industrial areas) were quickly covered in smuts".[78] This is clearly an area which requires careful research for clarification, perhaps through a study of working-class budgets of the period to see how much was spent on soap and cleaning materials; for example, it would appear from budgets of cotton workers living in Manchester and Dunkinfield in 1841 that some families consumed about 2 pounds of soap a week[79] suggesting that personal hygiene was of a very high standard.

The improvement of personal cleanliness through more frequent washing and bathing was accompanied by much more effective sanitation, at least in the case of the wealthier classes. According to one contemporary, water-closets had come into general use by the year 1814[80] although the social historian C. S. Peel believed that this was not the case until about 1830.[81] Inasmuch as the water-closet replaced unhygienic methods of sanitation, health would have been significantly improved, particularly through eliminating fly-borne diseases. However, it is doubtful whether the water-closet was used by more than a small minority of the general population by the end of the 1830s, it obviously being expensive to buy and instal.

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[76] Peel, *op. cit.*, p. 90, fn. 59.
[77] *Ibid.*, p. 87.
[78] *Ibid*, p. 142.
[79] Burnett, *op. cit.*, pp. 53, 70, 77, fn. 17.
[80] Wright, *op. cit.*, p. 108, 78, fn. 48.
[81] Peel *op. cit.* p. 85, fn. 59.

In conclusion, the question must be raised as to whether the argument developed in this paper is relevant to countries other than England and Wales. It is likely that the historical evidence on food supplies is just as problematic for these other countries; for example McKeown *et al.* emphasize the importance of the potato in Irish population growth, yet the potato was generally used about 100 years before the period of most rapid population expansion – and the evidence is that earlier diet was nutritionally much more adequate than that during the first half of the nineteenth century.[82] In practice, it is highly unlikely that any one explanation will be adequate for all European countries, which had significantly different social structures during this period. Only detailed historical research will begin to resolve the issues raised in this paper, but such research must be guided by hypotheses which are both theoretically sound and consistent with the known empirical evidence.

[82] See Chapter 4.

Chapter 7

The Growth of Population In Eighteenth-Century England: A Critical Reappraisal[1]

The growth of the English population in the eighteenth century has long interested economic historians and, since the time of Thomas Malthus, has provoked much debate about the relationship between population change and economic growth. In our own time, scholars have focused on the nature and chronology of change: whether economic development preceded and prompted population growth or vice versa. The structure of demographic change has, however, yet to be resolved. Prior to the nineteenth century, English demographic data are incomplete: there were no national censuses before 1801, and civil registration of births, marriages, and deaths did not begin until 1837. Demographic research on the pre-nineteenth-

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[1] First published in *The Journal of Economic History*, Vol. 53, No. 4 (Dec. 1993). Although there were several people who commented on various drafts of this paper, the person who I owe most to is Dr Ruth Richardson, who made a number of suggestions for re-writing sections of the paper, as well as providing invaluable encouragement and support. Peter Lindert made several useful critical comments on the structure of the paper, as did Pamela Evans and the referees who encouraged me to re-write an earlier draft version. Jim Oeppen helped with a number of technical calculations, particularly the expectation of life figures in Table 10. Ros Davies of the Cambridge Group provided a print-out of the Colyton reconstitution schedules which form the basis of Table 4. Frank Leeson made available information on the tontines, as did Anthony Camp in various genealogical issues. I appreciate the comments on different drafts of the paper by Christopher Hill, John Habakkuk, Tony Wrigley, Keith Snell, Richard Wall, Richard Smith, Michael Anderson and my brother, Edward Razzell.

century period has relied mainly on parish registers, which list baptisms, marriages, and burials. The accuracy and coverage of these materials is uncertain, and their survival is uneven.

Despite these difficulties, all demographers have discerned a rise in the rate of English population increase in the second half of the eighteenth century, and many have emphasized fertility as the key mechanism of population growth. These ideas have received added weight from the ambitious programme of research undertaken since the 1960s by the Cambridge Group for the History of Population and Social Structure. The Cambridge Group's demographic findings were presented in *The Population History Of England*, written by two of the group's leading members, Tony Wrigley and Roger Schofield.[2] The authors argued that English population grew in the latter half of the eighteenth century mainly because of a rise in fertility. This rise, they hypothesized, was due to a reduction in the age at marriage, itself a consequence of rising real incomes caused by economic development. This article questions the validity of their conclusion and develops an alternative chronology and explanation of the demographic transition in England.

NUPTIALITY AND MARITAL FERTILITY

The Cambridge Group have used two methodologies in its demographic work: 'back projection' and 'family reconstitution'. I will evaluate each in turn and offer evidence suggesting that the reliability of both methods as applied to the English data is open to question.

Back Projection

Back projection was a technique used by Wrigley and Schofield to estimate earlier population levels by retrospectively adding the number of deaths and net emigrants to the various age groups enumerated in the nineteenth century censuses, extending their process back into the sixteenth century. They used records of baptisms, marriages, and burials from a sample of 404 parish registers, which in theory allowed them to reconstruct the

[2] E. A. Wrigley and R. S. Schofield, *The Population History Of England* (1991).

numbers of people living at all periods, as well as to compute marriage, birth, and death rates. The method involves using a number of assumptions of unknown reliability, with scope for the compounding of errors and assumptions over long periods of time. Although the technique was developed using a very sophisticated computer programme, the unknown reliability of the raw data and the uncertain assumptions used in the programme led Schofield himself to compare it with looking "through a glass darkly".[3]

Ronald Lee, an active associate of The Cambridge Group, expressed his own reservations about the method in the following terms: "Back projection attempts an impossible task, and can only arbitrarily select one demographic past from among an infinite set of equally plausible and acceptable ones, which are consistent with the input data".[4] Recognition of the method's problems led other scholars to propose adjustments to the technique. Lee advocated its replacement with what he termed "inverse projection"; he claimed to have validated Wrigley and Schofield's findings by applying this new method to their basic data. More recently, Wrigley and Schofield have themselves advocated a variant of a method pioneered by Jim Oeppen, "generalized inverse projection".[5]

However, such methods require reliable data on births, deaths, migration, age structure, and mortality by age for the appropriate period – though they differ in their exact demands for reliability. Lacking accurate source material, the advocates of these methods have had to adjust their back-projected data in various ways.

For example, to correct for the under-registration of births, Wrigley and Schofield have inflated the number of baptisms by various ratios, derived from a comparison of expected births with actual records of baptisms. The estimates of expected births were calculated by taking the various census age groups and adding the estimated number of those born into the groups, who died or migrated in the period before the census. A crucial factor in this computation is the magnitude of the various age groups, because it is

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[3] R. S. Schofield, "Through a glass darkly: the population history of England as an experiment in history", *Journal Of Interdisciplinary History* (1985).

[4] R. D. Lee, "Inverse projection and back projection: a critical appraisal, and comparative results for England, 1539 to 1871", *Population Studies*, Vol. 39, No. 2 (July 1985), p. 190.

[5] Wrigley & Schofield, *op.cit.*, p. xvii.

the starting point for the process of estimating expected births. A poor estimate of the number of people in each age group would affect the inflation ratios used to correct the figures for baptisms, and hence would affect back-projected estimates of birth rates.

Peter Lindert argued that the Wrigley–Schofield findings were distorted by the changes they made to census age figures. He concluded that "life tables and nineteenth-century censuses suggest that birth registration was worse before 1780 than after. Yet Wrigley and Schofield turn the suggestion upside down, arbitrarily revising the censuses instead".[6] Lindert has calculated the inflations they made to the birth rate in a tabular form, reproduced here in Table 1.

Table 1. Birth Rates per 1,000 Population, England and Wales[7]

	1749–1753	*1814–1818*
Birth Rate Before Inflation	29.70	27.99
Penultimate Estimates (after Inflating for Non-Conformity and Delayed Baptisms)	32.14	32.69
Final Estimates after "Residual" Inflations	33.76	41.92

Lindert's disquiet at the transformation of the pattern of fertility through the use of these inflation ratios seems justified. The inflations adopted by Wrigley and Schofield progressively increase the birth rate, though the critical inflation is for "residual" non-registration. This residual inflation increases the birth rate for the period of 1814–1818 from 32.69 to 41.92 per 1,000, transforming the pattern of fertility in the period. Before this residual adjustment Wrigley and Schofield's original data suggested a constant birth rate during the latter half of the eighteenth century; after it, a very significant increase was apparent. That increase was due entirely to the inflation ratios derived from their assumptions about the age structure of the population applied to the original data.

My own research also throws doubt upon those inflation ratios. I have compared census statements directly with the expected baptism register entries for individuals living in 45 parishes selected from all parts of England. Table 2 displays the two sets of figures.

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[6] Peter H. Lindert, "English living standards, population growth, and Wrigley–Schofield", *Explorations In Economic History*, Vol. 20 (1983), p. 136.

[7] For the source of this table see Lindert, *op. cit.*, p. 138.

Table 2. Individuals Listed in the 1851 Census but Not Found in the Baptism Register Versus The Cambridge Group's Inflation Ratios[8]

Period	*Percentage Not Found in Register (Razzell)* (1)	*Period*	*Wrigley & Schofield's Inflation Ratios (%)* (2)
1761–1770	32.4	1760–1769	8.4
1771–1780	27.9	1770–1779	9.3
1781–1790	32.6	1780–1789	13.1
1791–1800	36.0	1790–1799	20.9
1801–1810	32.0	1800–1809	28.8
1811–1820	33.0	1810–1819	38.0
1821–1830	30.0	1820–1829	34.1
1831–1834	27.4	1830–1839	26.0

The figures in column 1 are based on direct empirical evidence; those in column 2 are derived from theoretical reconstruction.[9] The two series are radically different in their trends over time; the census-baptism register data show little or no change over the period, whereas Wrigley and Schofield's figures show a sharp deterioration in registration accuracy from 1781 onward.

The critical ingredient in their inflation ratios Wrigley and Schofield used was their adjustment of age structure data derived from the nineteenth century censuses. They themselves pointed out that one of their major

[8] See Peter Razzell, "The evaluation of baptism as a form of birth registration through cross-matching census and parish register data: a study in methodology", *Population Studies*, Vol. 26 (1972), p. 129 (published as Chapter 4); and Wrigley and Schofield, *The Population History*, p. 561.

[9] For column 2, I calculated the percentages that Wrigley and Schofield used to inflate baptisms in order to produce the number of births (excluding non-registration due to delayed baptism.) The census-parish comparison method has attracted criticism on three grounds: (1) the 1851 census mis-stated the birthplaces of individuals enumerated; (2) many parents had their children baptised in neighbouring parishes; and (3) the 1851 census mis-stated names and ages. From research linking census, parish and civil registration data (see Chapter 5), it has been established that the "false negatives" arising from these three factors amounted to about 10 per cent for the whole sample of 45 parishes. The "false negatives" were counter-balanced by "false positives" due to using over-strict criteria for successful matches and to infants dying before baptism.

assumptions was "that the age data for the older age groups became progressively less trustworthy with rising age, until above the age of 70 very substantial corrections to the published totals are necessary".[10] This is not a minor step in their calculations. It is not only central to the question of baptism registration adequacy, but it can be crucial for estimates of population size using back projection. Older age groups in the nineteenth-century censuses form the starting point of back projection, and any change in their numbers makes a critical difference to estimates of population size because of the compounding of errors with each "pass" through the computer programme. For example, Wrigley and Schofield reduced the size of the group aged 90 to 94 in 1871 by 44 per cent; if they had chosen instead to reduce that age group by 40 per cent, their estimate of the English population in 1541 would have been about 9 per cent greater.[11]

How reasonable are Wrigley and Schofield's assumptions? When we examine age statements by comparing the census with baptism register entries, a very different picture emerges from that assumed for the back-projection programme. For the census-parish register sample of 45 parishes, 88.8 per cent of all adult ages in the 1851 census were accurate to within two years, 97.8 per cent to within five years. Contrary to Wrigley and Schofield's assumptions, there was no deterioration in the accuracy of age statements above the age of 70; the reliability of age statements in the 70 to 80 age group was the same as for the total sample. Only in the 80 to 90 age group was there any decrease in accuracy. But even there, 74.5 per cent of the ages were accurate to within two years, and 90.2 per cent to within five years.[12] This conclusion is confirmed by Wrigley himself from his detailed work on the 1851 Colyton Census: "The generally high standards of statements of age is clear. Only a tiny percentage of ages were out by more than two years ... Even at advanced ages this holds true in general ... Only one of the 26 [cases aged over 70] mis-stated his age by more than three years".[13]

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[10] Wrigley and Schofield, *Population History*, p. xiv.

[11] R. D. Lee and D. Lam, "Age distribution adjustments for English censuses, 1821 to 1931", *Population Studies*, Vol. 37 (1983), pp. 445–464.

[12] Razzell, "The evaluation of baptism", *op. cit.*, pp. 126, 127.

[13] E. A. Wrigley, "Baptism coverage in early nineteenth century England: the Colyton area", *Population Studies*, Vol. 29 (1975), p. 304.

On the substantive issue of the increase of the eighteenth-century population, the evidence suggests no increase in the birth rate during the latter half of the century. Wrigley and Schofield, however, supported their argument about the central role of a rise in fertility by quoting data from their research on family reconstitution, which purports to show that a rise in fertility associated with a reduction in the age at marriage (rather than a fall in mortality), was responsible for eighteenth-century population growth. Although they expressed a caveat about the reliance on a very small number of parishes in their reconstitution work – about 13 from a total of about 10,000 have formed the basis of the sample to date – these scholars used their family reconstitution findings to underpin the conclusions they reached from back projection. Yet there are also grounds for disquiet about the accuracy of their use of the reconstitution method. This is a theme of such importance as to deserve detailed examination.

Family Reconstitution

Family reconstitution involves the study of individual families at the parish level. Individuals are traced in the baptism, marriage, and burial registers, and certain assumptions are made to establish family links among the individuals traced. From those links data are generated on a range of demographic variables, including age at marriages, fertility, and mortality rates. Family reconstitution is only applicable to individuals who remained in their parish of origin, as those who left disappeared from local records. For example, in the case of marriage, those who migrated after baptism invariably married elsewhere and would be excluded from the age-at-marriage calculations. Wrigley and Schofield worked on the assumption that those who remained in a parish were representative of the whole population, including migrants.

Ever since Peter Laslett's well-known 1960s' study of Clayworth and Cogenhoe, social historians have increasingly come to recognize just how mobile the English population was. A general study of migration in early modern England by Peter Clark and David Souden found that up to 80 per cent of the population was mobile, the percentage varying by place and over time, with increased mobility during periods of population growth.[14]

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[14] Peter Clark and David Souden, *Migration And Society In Early Modern England* (1987), pp. 32, 122, 123, 222.

As migrants are excluded from reconstitution studies, these very high levels of migration mean that reconstitution charts include only minorities of the population.

Evidence suggests that, because of the association between migration and social status, these minorities were atypical. Clark and Souden found that "more respectable members of local society tended to be less mobile than small craftsmen, servants and labourers" – though this may have varied over time.[15] Most evidence on geographical mobility and social status shows that they were very strongly correlated. From his work with the Cambridge Group, Souden noted "the high mobility of labourers in the reconstitution material" and commented on the "high mobility of labourers and many craftworkers and the relative immobility of farmers and food retailers." He concluded that "the marked lifetime immobility of farmers – of yeomen and husbandmen – contrasted with labourers ... would show the degree to which landholding, or its prospect, would condition movement".[16] Those included in the reconstitution cohorts – the stayers – were much more likely to be farmers and other property owners, whereas the migrants were invariably labourers, servants, and other propertyless groups. Labourers, servants, and other impoverished groups formed a significant proportion of the population at this time – perhaps up to half the total. Their relative exclusion would raise major questions about the validity of reconstitution methodology.

Migration also serves to distort reconstitution calculations in a more technical way, that can most easily be illustrated with respect to calculations of the average age at marriage. Wrigley's study of Colyton indicated that the proportion of women born and married in the parish fell from 43 per cent in the 1560–1646 period to 25 per cent in 1720–1769 before rising to 31 per cent between 1770 and 1837.[17] Such a significant shift in the amount of migration would affect calculations of age at marriage, if migration was not evenly distributed among the various age groups. For example, if for some reason a larger proportion of women in their late twenties migrated out of a parish, this would have the apparent effect of

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[15] *Ibid*, pp. 122, 123.

[16] David Souden, *Pre-Industrial English Migration Fields* (D.Phil., University of Cambridge, 1981), pp. 250, 254, 310.

[17] R. S. Schofield, "Age-specific mobility in the eighteenth-century rural English parish", *Annales De Demographie Historique* (1970), p. 262.

lowering the age at marriage: women marrying at older ages would have left the sample before they could be included in the reconstitution age-at-marriage calculations, and only the younger ones would be recorded. Thus, even where there were no real changes in the age at marriage, variations in migration patterns could create the illusion of change, because of the calculation method used in reconstitution work. Without a detailed knowledge of migration, it is impossible to say precisely what effect it would have on age-at-marriage calculations. Clearly, the effect could be significant.

Various sources suggest that the number of widow and widower remarriages as a proportion of the total number of marriages fell from approximately 30 per cent at the beginning of the eighteenth century to about 10 per cent at the end.[18] Whether this reduction occurred as a result of falling mortality or of changes in the propensity to remarry is an open question, but the fall itself could influence the accuracy of reconstitution by reducing the number of older men and women marrying in a parish. Most parish registers do not give information on the marital status of the marrying parties; for men, this could lead to a systematic over-statement of first-marriage ages in the earlier period by accidentally including marriage ages of widowers. Large numbers of women of unknown marital status listed in the marriage registers could also distort reconstitution findings because of the greater likelihood of confused identity.

The problem of identity confusion also arises when parish register information is inadequate. The linking of baptism and marriage dates in reconstitution work is essentially speculative, based on the assumption that a similar name within a certain time period confers a common identity. Yet there are grounds for believing that this assumption is unjustified. As we will see, it was a widespread practice in England to give the name of a dead child to a subsequent sibling of the same sex, and many parish registers were defective in registering the baptism and burial of those

[18] Wrigley and Schofield, *Population History*, pp. 258, 259. The parish registers of Stoke Poges, Eton, and Farnham Royal in Buckinghamshire; of St Margaret's Rochester in Kent; and of Barnstable in Yorkshire give information on previous marital status during the civil registration period of 1653 to 1658. Total marriages of widows ranged between 25.7 and 37.0 per cent. The marriage licences of East Kent and West Sussex show a fall in the proportion of widows, from over 30 per cent in the first half of the seventeenth century to approximately 10 per cent in the early nineteenth century.

subsequent siblings. The registration of burials – and possibly baptisms – improved in at least some of the reconstitution parishes during the eighteenth and early nineteenth centuries, which might have affected calculations of the changing mean age at marriage. The Cambridge Group uses identical names in the baptism and marriage registers as the basis for calculating marriage ages. The non-registration of subsequent same-name siblings would inflate marriage ages by incorrectly linking the first dead sibling with the sibling of the same name listed in the marriage register. This would have been more significant in the earlier period of course, because of the less adequate registration of same-name individuals.

There are therefore four serious grounds for questioning the validity of reconstitution methodology as it has been applied to English marriage data: (1) The sociologically unrepresentative nature of reconstituted cohorts due to the exclusion of migrants; (2) the technical distortion effects of migration upon the calculation of reconstitution statistics; (3) the unknown effect of changes in the proportion of widows and widowers in the marriage registers; and (4) the effect of changing patterns of same-name sibling registration on the calculation of marriage ages.

Given the uncertain reliability of back projection and family reconstitution as they have been applied to English historical data, it is necessary to carefully examine other forms of demographic evidence for the seventeenth- and eighteenth-century period to see what they reveal.

AGE AT MARRIAGE DURING THE SEVENTEENTH AND EIGHTEENTH CENTURIES

The mean age at first marriage for women in the Cambridge Group's reconstitution sample was at its highest for the period from 1650 to 1699 – 26.2 years.[19] In historical terms this is a high figure, and its magnitude is largely responsible for the subsequent fall in the age at marriage found by the group. It is therefore important to evaluate that mean carefully, as it represents the key element in the pattern of marriage ages generated by reconstitution.

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[19] E. A. Wrigley and R. S. Schofield, "English population history and family reconstitution: summary results 1600–1799", *Population Studies*, Vol. 37 (1983), p. 164.

Two forms of marriage were legal in England in the seventeenth and eighteenth centuries: marriage by licence and marriage by banns. Although both types were included in parish registers, marriage licences were recorded separately by the ecclesiastical authorities and often contain a great deal more information – such as age at marriage – not found in parish registers. Marriage by licence was marginally more expensive than marriage by banns, and therefore was more socially exclusive. In particular, labourers tended to marry by banns, though all other occupational groups appear to have been well-represented by licences.[20] However, the flexibility of marriage by licence – it allowed marriage in any parish without having to call banns on three successive Sundays – meant that this type of marriage became very popular in the seventeenth and eighteenth centuries. For example, over 50 per cent of all marriages in the Diocese of Canterbury were by licence between 1677 and 1725.[21] Indeed, in some parishes in the Diocese of London at that time, the proportion rose to over 80 per cent.[22] For demographers licences have the advantage of giving information on migrants as well as non-migrants, and of covering large groups of parishes; they therefore help overcome the problem of concentrating on individual, and possibly atypical, parishes.

The accuracy of age statements in marriage licences seems to have been high. Vivian Elliott evaluated marriage ages in a sample of 69 cases of London licences at the beginning of the seventeenth century: the averages were 23.47 years in the licenses and 23.50 years by reconstitution – that is, by comparing baptism and marriage dates in the parish register. A similar exercise for 50 Leicestershire marriages at the end of the same century yielded averages of 24.8 and 23.8 years respectively, indicating a difference of about one year.[23] This may be due to inaccuracies in marriage

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[20] D. J. Steel, *General Sources Of Births, Marriages And Deaths Before 1837* (National Index Of Parish Registers, Vol. 1, 1976), p. 227.

[21] The number of licence marriages is listed in J. M. Cowper (ed.), *Canterbury Marriage Licences* (1894) and (1898). The total number of marriages in Kent is given in *Enumeration Abstract, 1841 Census*. The proportion marrying by licence was 50.74 per cent for the period between 1677 and 1725.

[22] See, for example, the St. Michael Cornhill, St. Mary Aldermary, and St. Helen's Bishopsgate marriage registers for this period.

[23] Vivian B. Elliott, *Mobility And Marriage In Pre-Industrial England* (D.Phil. thesis, Cambridge University, 1978), pp. 291, 325.

licence age statements or to a confusion of identities in the parish register as a consequence of same-name registration problems.

In the late seventeenth century, high-quality information is available from licences taken over 1,000 parishes in five counties in different regions of England: Kent, London, Nottinghamshire, Suffolk, and Yorkshire.

Table 3. Age at First Marriage of Women Listed in Licences, 1660–1714[24]

Period	*Region*	*N*	*Mean Age at Marriage*	*Reconstitution Mean Age at Marriage, 1650–1699*
1662–1714	Yorkshire	7,242	23.76	
1660–1702	London	500	21.93	
1661–1700	Kent	1,000	24.06	26.2
1670–1709	Nottinghamshire	3,284	24.44	
1690–1709	Suffolk	356	23.60	

Table 3 shows that the mean age at marriage in the four counties other than London lies within a narrow band of 23.60 to 24.44 years. The overall average age at first marriage for the five counties is 23.56 years. significantly lower than the mean age found in the reconstitution sample for the same period: 26.2 years. In the 1840s, the earliest years of civil registration, the mean age at first marriage of women was about 25.[25] The data in Table 3 suggest, therefore, no fall in the mean age at first marriage, but on the contrary, a long-term rise of about 1.5 years.

[24] Sources: For Yorkshire: M. Drake, "An elementary exercise in parish register demography", *Economic History Review*, Vol. 14 (1962), p. 444; for London: George Armytage, *Allegations For Marriage Licences ... At London* (Harleian Society, Vol. 24, 1886) – selecting the first 100 cases from the beginning of each decade; for Kent: Cowper, *Canterbury Marriage Licences*, (1876, 1898) – selecting the first 500 cases from each volume; for Nottinghamshire: T. M. Blagg and F. A. Wadsworth (eds.), *Abstracts Of Nottinghamshire Marriage Licences* (British Record Society Index Library, Vol. 58, 1930) – selecting all cases listed; for Suffolk: W. B. Bannerman (ed.), *Allegations For Marriage Licences In The Archdeaconry Of Sudbury* (Harleian Society, Vol. 69, 1918) – selecting all cases listed.

[25] *Registrar-General's Fifty-Eighth Annual Report* (1897), p. ix; and *Registrar-General's Twenty-First Annual Report* (1860), p. iii.

THE HISTORY OF MORTALITY

Because the evidence considered in the previous section offers no support for a decline in age at marriage – nor for a rise in fertility – it is necessary to look elsewhere to explain English eighteenth-century population growth. In this section I will argue that the key demographic change was a decline in mortality, that was particularly marked in the first half of the eighteenth century.

Population studies covering the centuries prior to reliable civil registration largely depend on data derived from parish registers. These registers invariably include information on baptisms (not births), marriages, and burials (not deaths). The reliability of the burial registers is obviously crucial to the study of mortality. For their calculation of reconstitution mortality rates, Wrigley and Schofield assumed burial registration accuracy of 100 per cent. Yet evidence suggests that in certain respects burial registration was significantly more defective in the seventeenth and eighteenth centuries than at a later period.

I have developed a method for measuring the adequacy of burial registration that may be termed the "same-name evaluation technique". It is based on child-naming customs prevalent in early modern England. It was extremely rare to give two living children identical Christian names; for example, of 2,221 children named in sixteenth century Essex Wills, only 0.5 per cent of living siblings shared the same name. An examination of seventeenth century census returns from different parts of the country revealed no clear cases of living brothers and sisters with the same name.[26] On the other hand, it was widely customary to pass a dead child's name on to the next-born sibling of the same sex.

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[26] F. G. Emmison (ed.), *Essex Wills, 1558–1565* (1982). The censuses searched were the Ealing 1599 census, the Clayworth censuses for 1676 and 1688, and the 1695 Marriage Duty Act censuses for London, Lyme Regis, Swindon, and Wanborough. The London census was published in D. V. Glass (ed.), *London Inhabitants Within The Walls* (London Record Society, Vol. 2, 1966). The Bristol census is in Elizabeth Ralph and Mary Williams (eds.), *The Inhabitants Of Bristol In 1696* (Bristol Record Society, Vol. 25, 1968). Copies of the other censuses can be found in the library of the Cambridge Group.

A look at two parishes used intensively in reconstitution work, Hartland and Colyton, enables us to estimate the frequency with which this same-naming custom was observed. In Hartland in the period from 1725 to 1743, a sample was chosen from the parish register of 50 dead children whose parents bore subsequent children of the same sex. Thirty of the subsequent children – 60 per cent – were given the same name as their pre-deceased sibling.[27] In Colyton, a similar examination of the data has proved possible over a much longer period by means of a re-analysis of the reconstitution schedules from 1538 to 1851.[28] There was a total of 789 families in the parish in which a child was baptised after the death of another of the same sex. Of those families, 508 – 64.4 per cent – gave the name of a previously baptised dead child to a subsequent child. The changes over time in the proportion of same-named children were as follows: 1538–1600: 54.9 per cent; 1601–1650: 55.5 per cent; 1651–1700: 76.9 per cent; 1701–1750: 70.0 per cent; 1751–1800: 73.5 per cent; 1801–1837: 63.4 per cent; and 1837–1851: 62.2 per cent. These are sufficiently large proportions of the total number of families to form the basis of an evaluation of burial registration during the whole 400-year period covered by the reconstitution schedules.

The importance of same-naming to the study of burial register accuracy can be illustrated as follows. During the middle part of the eighteenth century, Thomas Turner, a Sussex shopkeeper, kept a detailed diary and compiled notes on his family's history.[29] He listed his children's births and deaths as follows:

Peter (born August 19, 1754, died January 16, 1755)
Margaret (born March 20, 1766)
Peter (born June 1, 1768)
Philip (born November 9, 1769)
Frederick (born December 8, 1771, died November 7, 1774)
Michael (born April 29, 1773)
Frederick (born May 3, 1775, died June 13, 1775)
Frederick (born December 17, 1776)

[27] See the *Hartland Parish Register*.

[28] A computer print-out of the reconstitution schedules of Colyton was kindly provided by Ros Davies of the Cambridge Group. The grouping of the families is specified in the print-out. Families with just interpolated baptisms were not included in the analysis because doing so would introduce bias into the analysis.

[29] G. H. Jennings (ed.), *The Diary Of A Georgian Shopkeeper* (1979), pp. 79–84.

Turner's first wife died after the birth of his eldest son Peter, and he subsequently remarried. The list of his children reveals the pattern of same-naming: the first Peter and the first two Fredericks each died, and the next child of the same sex was given the dead child's name. Thomas Turner had lived all his married life in the parish of East Hoathly, and it is instructive to compare this list of births and deaths with the record of baptisms and burials of his children in the East Hoathly parish register:[30]

Peter baptised August 31, 1754.
Margaret Turner baptised April 23, 1766.
Peter baptised June 28, 1768.
Philip baptised November 5, 1769.
Frederick baptised December 30, 1771.
Michael baptised May 19, 1773.
Frederick baptised May 14, 1775, buried June 13, 1775.
Frederick baptised January 10, 177.

All of Turner's children were baptised and registered in the parish, but only one of the three dead children was recorded in the burial register – the second Frederick, who died in 1775. Turner's diary reveals that Peter and the first Frederick were in fact buried in the neighbouring parish of Framfield, where their grandparents had died and been interred.

The Cambridge Group's reconstitution rules work on the assumption that all family events occur within the parish of residence. Given this, the demographic history of the Turner family, in which two children were buried outside the parish, would be misrepresented. The group's reconstitution rules would generate a calculated child mortality rate of 12.5 per cent (one out of eight children), whereas in fact the true mortality rate was 37.5 per cent (three out of eight children).

The practice of same-naming, however, allows us to assess the adequacy of parish registers in registering the deaths of children. For example, though we would not know from the East Hoathly burial register what had happened to Peter and the first Frederick, the repetition of their names in the baptism register would tell us that they had died, even though no record of their burial was available. We can thus assess the reliability of burial

[30] I am grateful to the East Sussex Record Office for conducting a search of the East Hoathly parish register.

registration of a particular parish register by measuring the proportion of same-name baptisms against registered same-name burials.

Application of this technique to a sample of cases selected from the Hartland parish register reveals that the accuracy of burial registration varied over time. Two hundred children baptised with the same name as a subsequent sibling were selected in alphabetical sequence from the register index for the period of 1558 to 1837.[31] Sixty-three of them – 31.5 per cent – were missing from the burial register. The first 100 cases, in the period from 1558 to 1724, had an omission rate of 39 per cent, whereas the second hundred cases, from 1725 to 1837, had a rate of only 24 per cent. These provisional results suggest a significant improvement in burial registration in Hartland during the eighteenth century.

A similar analysis of the 508 families in the Colyton reconstitution schedules who gave two or more of their children the same name yields the results shown in Table 4.

Table 4. Analysis of Burials Registration of Same-Name Siblings in Colyton, 1538–1837[32]

Period	*N*	*Number Found in Burial Register*	*Percentage of Cases Unregistered*
1538–1600	95	62	34.7
1601–1650	121	71	41.3
1651–1700	114	86	24.6
1701–1750	84	54	35.7
1751–1800	94	60	36.2
1801–1837	77	64	16.5
1837–1851	38	34	10.5
Total	623	431	30.8

The omission rate for the whole Colyton sample – 30.8 per cent – is similar

[31] The initial identification of names was provided by the Hartland parish register index. In the earlier period only the father's name was available for establishing a correct identity, but when two or more families had the same name, place-names were used as an additional criterion.

[32] All calculations were made from Colyton reconstitution schedules supplied by Ros Davies of The Cambridge Group. The identity of same names is specified in the schedules, and in every case those names were selected for analysis.

to that found in Hartland, and registration accuracy there also seems to have varied over time. The Colyton registers reveal a sharp improvement at the beginning of the nineteenth century, which is consistent with what is known generally about the relative accuracy of Anglican burial registration at the time of the introduction of civil registration.[33]

I have made a special study of the Colyton Anglican burial register between 1837 and 1851, the period immediately following the introduction of civil registration. The civil registration records there list the deaths of 199 children under the age of ten during this period. Of that number, 170 were registered in the Anglican burial register, giving an omission rate of 14.6 per cent – slightly higher than the 10.5 per cent figure found using the same-name technique for the same period. However the civil registers included young infants who died before baptism and were therefore often denied full burial status by the church. If we exclude infants who died in less than 24 days – the approximate mean age of baptism in Colyton at the time – the burial omission rate declines to 10.8 per cent.[34] We must not make too much of the almost identical findings of the same-name technique and the civil-Anglican burial register comparison method, as the sample in the former study is small. Nevertheless, the similarity in the results of these two methods indicates a degree of reliability.

There were a number of reasons why Anglican burial registration was so deficient before the nineteenth century. The major factor was probably the negligence of clerks and clergymen in registering burials that had taken place in their parish.[35] Of all same-name cases in Colyton during the period from 1538 to 1851, 30.8 per cent were missing from the burial register. We can evaluate this figure by comparing it with the proportion of people dying in Colyton who left wills, but who were not registered in the burial register. Information is available on 124 people living in Colyton or who

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[33] Glass estimated that about 20 per cent of all deaths were omitted from Anglican burial registration in the early period of civil registration, but this figure was lower in rural parishes like Colyton. See D. V. Glass, "Population and population movements in England and Wales, 1700 to 1850", in D. V. Glass and D. E. C. Eversley (eds.), *Population In History* (1965), pp. 221–246.

[34] This analysis is based on a list of Anglican burials and civil registration deaths that took place in Colyton between 1837 and 1851. The list was kindly provided by Richard Wall of the Cambridge Group.

[35] See the discussion of this topic in Chapter 4.

specified burial in the parish churchyard and made wills between 1554 and 1797; of this number 35 – 28.1 per cent – were not registered in the burial register.[36] The similarity between this and the same-name figure suggests that there was a general under-registration of burials – both of adults and children – during the period.

We have seen in the case of the Turner family another reason for unrecorded burials was the interment of children in neighbouring parishes – a practice described by Schofield as a "traffic in corpses".[37] This probably accounts for some of the missing burials in a parish like Colyton. In the reconstitution schedules, information is sometimes given on the residence of a family, and there is a correlation between place of residence and registration reliability between 1538 and 1837, the period covered by the schedules. Of 65 same-name cases in which the father was listed as living in the town of Colyton, 48 were found in the burial register, an omission rate of 26.2 per cent. When families lived outside the town, in hamlets and outlying farms, the omission rate was as high as 43.9 per cent, only 83 out of 148 same-name cases being found in the burial register. Some of these missing cases were probably buried in neighbouring parish churchyards, that were closer to the outlying areas than was the Colyton parish churchyard. Children baptised in Colyton but buried in surrounding parishes would not appear in the reconstitution statistics of infant and child mortality, and their omission would lead to an under-statement of mortality.

Wrigley and Schofield's assumption of absolute accuracy of the parish registers used in their reconstitution work was based partly on their having carefully selected high-quality parish registers, eliminating those with obvious defects. In the case of baptism registration, their assumption may be justified – particularly as missing baptisms can be interpolated from information on child burials, and registers can be selected on the basis of having the right pattern of birth intervals (that is, baptisms of children in a particular family occurring approximately every two years).

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[36] S. A. Smith, *Extracts From (302) Wills Proved In P.C.C. Relating To The Parishes Of Shute And Colyton* (1901); Edward A. Fry, *Calendars Of Wills ... Relating To ... Devon And Cornwall*, Vol. 1 (1908) and Vol. 2 (1914); and the *Colyton Parish Register*. Information is usually given on the dates of the making and proving of wills, which allows a precise check against the burial register.

[37] R. S. Schofield, "Through a glass darkly: the population history of England as an experiment in history", *Journal Of Interdisciplinary History*, Vol. 15 (1985).

No such interpolation or selection is possible with burial registers, however, and the evidence derived from the same-name technique as applied to Hartland and Colyton suggests that death registration was unreliable throughout the sixteenth- to eighteenth-century period. The deficiency was probably greater than that measured by Table 4. The same-name technique can only be applied to cases in which baptisms were accurately registered, and it is likely that children whose baptism registration was defective, also had more deficient burial registration. As we have seen, neither does the technique allow for children who died before baptism, and many of them would not have appeared in the burial register.

The Cambridge Group's estimates of infant and child mortality rates for Hartland and Colyton in the seventeenth and eighteenth centuries are low by historical standards: in the range of 83 to 106 per 1,000 between 1600 and 1749, falling to 57 to 97 per 1,000 between 1750 and 1799.[38] The results of the same-name technique indicate higher rates for all periods. If we allow for the various factors just discussed, which would further inflate registration unreliability, it is likely that infant mortality in Hartland and Colyton in the seventeenth and eighteenth centuries has been under-estimated by between 35 and 50 per cent.

According to the group's figures, the average infant mortality rate for the 13-parish reconstitution sample for 1600 to 1749 lay in the range of 161 to 169 per 1,000.[39] If we inflate this rate as indicated earlier, it would increase infant mortality to between 250 and 340 per 1,000. Given that national infant mortality was about 150 per 1,000 under early civil registration in the late 1830s, infant mortality probably dropped significantly during the eighteenth and early nineteenth centuries. However, it is too early to reach firm conclusions about the overall direction of this type of mortality; further research is needed on the registration reliability of other reconstitution parish registers.

The uncertain reliability of parish registers increases the value of other forms of evidence on mortality during the seventeenth and eighteenth centuries. Nearly all of these data concern adult mortality. In a 1974 article on parental loss, Peter Laslett commented on an apparent decline in the number of orphans in the seventeenth and eighteenth centuries. Community surveys of eleven localities taken between 1500 and 1706 revealed a

[38] Wrigley and Schofield, "English population history", *op. cit.*, p. 179.
[39] *Ibid*, p. 177.

median of 25 per cent (with a mean of 22.5 per cent), whereas eight surveyed between 1724 and 1811 had a median of 16.5 per cent (with a mean of 15.9 per cent). Laslett concluded that the decline in the number of orphans probably "arose because of shifts in demographic rates, particularly in mortality".[40]

Of the communities Laslett studied, perhaps the most famous is Clayworth, in Nottinghamshire. The disappearance of large numbers of people in this community between 1676 and 1688 was used to illustrate the high level of mobility at that time. What has not been sufficiently realized is that in the case of adult heads of household, most of them disappeared through death rather than migration. Of 95 heads of household living in Clayworth in 1676, 44 were no longer living in the parish in 1688 – 10 may have left through migration, but the remaining 34 died between the two censuses.[41] Allowing for the effects of migration, those 34 deaths represent a mortality rate of 3.05 per cent per annum – over twice the 1.39 per cent adult mortality rate found in England under civil registration 150 years later.[42]

In his discussion of orphans, Laslett quoted the civil marriage returns for the Manchester area in the 1650s, which recorded the father's name, parish of residence, and father's mortality status. Using these data, it is possible to calculate the mortality rate of fathers. Of 380 spinsters married in the Manchester area between 1654 and 1600, the fathers of 226 were dead at the time of their marriage. That is, the fathers of 59.5 per cent of these women were dead.[43] Assuming an average age at first marriage for women of about 23, this represents an annual mortality rate of fathers of 2.59 per cent per annum, well above the figure found in early civil registration. The fathers of these women marrying in Manchester came from all parts of Lancashire as well as from other northern counties. There appears to have been little variation in mortality between different areas

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[40] Peter Laslett, "Parental deprivation in the past: a note on the history of orphans in England", *Local Population Studies*, No. 13 (Autumn, 1974), p. 15.

[41] Peter Laslett and John Harrison, "Clayworth and Cogenhoe", in H. E. Bell and R. L. Ollard (eds.), *Historical Essays 1600–1750 Presented To David Ogg* (1963), pp. 157–184, p. 15.

[42] *Registrar-General's Ninth Annual Report* (1849), Appendix.

[43] These figures were calculated from all marriages listed in the marriage register between 1654 and 1660. See the *Manchester Cathedral Parish Register*.

within Lancashire. Of the 49 fathers who came from Manchester itself, 61.2 per cent were dead at the time of their daughter's marriage – a proportion close to that for the whole sample covering all areas. (Evidence from tontines, marriage licences and other material suggests that the urban–rural gradient post-dates the seventeenth century.)

This evidence suggests a radical long-term decline in mortality between the seventeenth and nineteenth centuries. It also fits traditional ideas of a high mortality rate in the pre-industrial era. However, it is at variance with the Cambridge Group's reconstitution work on adult mortality, which found only a very modest rise of about three years in life expectancy for men at age 30 during the 250 years between 1550 and 1799.[44] Most of the problems associated with the reconstitution of marriage ages – unreliable parish registers, sociologically unrepresentative samples, and the technically distorting effects of migration – are also applicable to the study of adult mortality. With the adult mortality cohorts there is the additional problem of very small sample sizes. For example, approximately 21.5 per cent of all females born in Colyton between 1560 and 1646 were included in the adult mortality cohort, with equivalent figures for 1720 to 1769 and 1770 to 1837 of 12.5 per cent and 15.5. per cent.[45] In other words, in some instances the Cambridge Group's mortality cohort was derived from only an eighth of the total population. Reliable conclusions about mortality cannot safely be based on such small samples.

There is, however, another source of information that allows a provisional assessment of adult mortality over the 300-year period between the sixteenth and eighteenth centuries: marriage licences. The licences issued in the Diocese of Canterbury are of particularly good quality and run continuously (except for the interregnum period of 1646 to 1660) from 1568 through to 1809 and beyond. The diocese covers the East Kent region and includes 289 parishes. Seventeenth-century marriage licences record information on the parents of bachelors and spinsters at all ages, particularly

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[44] Wrigley and Schofield, *The Population History*, p. 250.

[45] Insufficient evidence has been published to calculate exact figures, but for Colyton approximately half of the initial cohort of married women was included in the mortality sample: applying that ratio to the proportion of females included in the marriage sample yields the figures quoted in the text. See E. A. Wrigley, "Mortality in pre-industrial England: the example of Colyton, Devon, over three centuries", *Daedalus*, Vol. 97 (1968).

for young women. By canon law, the consent of parents or guardians was required before a marriage licence could be granted: those marrying under 21 had to provide it in writing or in the form of a sworn affidavit.[46]

The allegations attached to the licences issued from 1619 to 1646 and from 1661 to 1676 nearly always refer to parental consent, particularly in the former period: over 96 per cent of licences give information on parental consent between 1619 and 1646. The richness of this information allows us to examine whether a father or parents were alive or dead for virtually all those marrying by license in that period: 42.4 per cent of the total population. The licences give information on age and occupation, which allows a study of both of those variables. Table 5 summarises an analysis of parental mortality by age for a sample of 1,000 individuals.

Table 5. Parental Mortality by Age of Daughter in East Kent, 1619–1646[47]

Age of Daughter	*Number in Sample*	*Father Alive, Consenting (%)*	*Father Dead, Mother Consenting (%)*	*Both Parents Dead (%)*
16–20	280	58.2	23.2	18.6
21–25	484	42.1	23.1	34.7
26–30+	236	26.7	25.0	48.3
Total	1000	43.0	23.6	33.4

This table reveals a high level of parental mortality: a third of women had lost both parents by the time of their marriage, a figure that increased to

[46] Steel, *op. cit.*, pp. 226–268.

[47] In preparing Table 5, I adopted the following rules: (i) if a father was listed as giving his consent, he was assumed to be alive; (ii) if a father was not mentioned, and a mother was stated as giving her consent, the father was assumed to be dead and the mother alive; and (iii) if a guardian was listed as giving consent, both parents were assumed to be dead. In the majority of cases, particularly during the earlier periods, information is given directly on mortality status of parents – for example, a mother giving consent is recorded as a widow of a lately deceased husband, or both parents are recorded as being dead. For the source material for this table see: J. M. Cowper (ed.), *Canterbury Marriage Licences* (1892, 1894, 1896, 1905 and 1906); and Arthur J. Willis (ed.), *Canterbury Marriage Licences* (1967, 1969 and 1971).

48.3 per cent for women aged 26 and above. So nearly one-half of women had lost both parents by their late twenties. In seventeenth-century Kent, only a minority of women – 43 per cent – had two living parents at the time of their marriage. These figures speak for themselves: adult mortality was very high in this period.

We can calculate the adult mortality rate of fathers by dividing the numbers dead by the average age of their daughters. Fully 57 per cent of all fathers were dead at the time of their daughter's marriage, and they had died during a 23-year period (the average age at marriage of their daughters). This yields an annual mortality rate of 2.48 per cent per annum, almost identical to that found from the Manchester marriage register for the period of 1654 to 1660. These fathers probably died over a fairly even period between the birth and marriage of their daughters: a small sample of 35 cases in which the date of death was given indicates that on average fathers died 10.6 years before the date of their daughter's marriage.

The long-term change in mortality can be measured by comparing these figures with those compiled under civil registration 200 years later. Among men living in Kent of roughly the equivalent age group – between 30 and 55 – mortality was virtually halved between the early seventeenth and early nineteenth centuries: from 2.48 per cent in 1619 to 1646 down to 1.31 in 1838 to 1844.[48]

The chronology of change in the pattern of mortality among the marriage licence population can be traced through an analysis of the marriages of all women marrying under the age of 21. This information is available in the Diocese of Canterbury for all periods except between 1701 and 1750. Table 6 depicts the exact chronology of decline in mortality.

[48] See *Registrar-General's Ninth Annual Report* (1849), Appendix, pp. 17–20.

Table 6. Mortality Amongst Parents of Spinsters Marrying Under Twenty-One by License in East Kent[49]

Period	*Father Alive, Consenting (%)*	*Father Dead, Mother Consenting (%)*	*Both Parents Dead (%)*	*Total Number in Cohort*
1619–1646	53.33	27.06	19.61	1,275
1661–1676	55.70	25.23	19.07	515
1677–1700	58.86	19.82	21.32	333
1751–1779	74.29	21.00	4.29	700
1780–1809	76.89	17.68	5.43	1,233

This table suggests a marked reduction in adult mortality from the mid-seventeenth to the mid-eighteenth century. The proportion of cases in which both parents were dead dropped particularly sharply: from 21.32 per cent in 1677–1700 to 4.29 per cent in 1751–1779. This was matched by the fall in the percentage of fathers dead – from 46.67 per cent to 25.71 per cent – representing a fall in mortality, all else being equal of 44.9 per cent. The reduction in mortality appears to have commenced after the 1660s, though the changes in the late seventeenth century appear to have been relatively slight. The main fall in mortality seems to have occurred between the end of the seventeenth and the middle of the eighteenth century.

For the earlier periods, information is invariably given in the Kent licences on the occupation of both husbands and living fathers, though not usually for fathers who had died. This allows an occupational analysis of mortality, and Table 7 illustrates what is possible in this respect.

[49] For the source material for this table, see Cowper, *Canterbury Marriage Licences* (1892, 1894, 1896, 1905 and 1906); and Willis, *Canterbury Marriage Licences* (1967, 1969 and 1971).

Table 7. Mortality Among Parents of Spinsters Marrying Under 21 by Occupation of Husband in East Kent, 1619–1809[50]

Occupation by Period	*Father Alive, Consenting (%)*	*Father Dead, Mother Consenting (%)*	*Both Parents Dead (%)*	*Number in Cohort*
Gentlemen and professionals				
1619–1646	60.49	16.10	23.41	205
1661–1700	61.83	19.85	18.31	131
1751–1809	72.33	20.12	7.55	159
Total	64.65	18.38	16.97	495
Yeomen and farmers				
1619–1646	58.76	25.18	16.06	274
1661–1700	57.99	15.98	26.03	169
1751–1809	84.54	12.08	3.08	207
Total	66.77	18.62	14.62	650
Husbandmen				
1619–1646	49.77	29.58	20.66	213
1661–1700	60.66	22.95	16.30	122
1751–1809	80.56	16.67	2.78	108
Total	60.27	24.60	15.12	443
Artisans and tradesmen				
1619–1646	54.18	28.48	17.92	491
1661–1700	50.61	29.45	19.94	326
1751–1809	74.31	20.40	5.29	397
Total	59.80	25.86	14.33	1214
Mariners and fishermen				
1619–1646	58.33	25.69	15.97	144
1661–1700	55.34	29.13	15.53	103
1751–1809	75.95	22.15	1.90	158
Total	64.44	25.19	10.37	405

Overall, there is little correlation between husbands' occupations and parental mortality – except in the earlier period, which shows a lower rate for gentlemen and a higher one for husbandmen, with a slightly higher mortality for gentlemen in the later period.

[50] For sources, see *Ibid.*

Although labourers and the unemployed are not covered by Table 7, groups such as husbandmen and fishermen were characterised by similar level of income and were certainly very much poorer than gentlemen and yeomen farmers.[51] The higher mortality among husbandmen indicates that economic forces may have been a factor in shaping mortality patterns in the earlier period. However, the fact that there were very substantial increases in life expectancy among all occupational groups during the eighteenth century suggests that economic factors were not primarily responsible for the reduction in mortality. For the later period we have information on a number of labouring families: of 91 women under the age of 21 marrying labourers in East Kent from 1751 to 1809, 83.52 per cent had fathers living at the time of their marriage – a figure second only to that of yeomen in the proportion of fathers still living. This finding is consistent with those on occupational mortality in the nineteenth century: labourers in agricultural counties in the post-1860 period had one of the lowest mortality rates recorded.[52]

Although no other reliable evidence covering the general population exists, a variety of information is available on special groups, which allows a supplemental assessment of changing mortality. One of the most accurate forms of data available is on Members of Parliament. Biographical information on MPs exists for the period from 1660 to 1820, except for 1691 to 1714. Date of birth, entry, and death to the nearest year is known for 94.58 per cent of the 5,995 MPs who first entered Parliament in 1660–90 and 1715–1820 – an unrivalled level of demographic accuracy for the period.[53]

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[51] Gregory King estimated that the average income of "common seamen" was £20 per annum, not significantly greater than that of "labouring people and out servants" (£15 per annum). See Gregory King, *Natural And Political Observations*, pp. 48, 49.

[52] See Michael Haines, "Conditions of work and mortality decline", in R. S. Schofield *et. al.* (eds.) *The Decline Of Mortality In Europe* (1991), p. 183. According to the East Kent licence date, all rural occupational groups – yeomen, husbandmen and labourers – had a lower parental mortality than the more urban ones in the late eighteenth century.

[53] See Basil Duke Henning (ed.), *House Of Commons, 1660–1690* (1983); Romney Sedgwick, *House Of Commons, 1715–1754* (1970); Lewis Namier and John Brooke, *House Of Commons, 1754–90* (1964); and R. G. Thorne, *House Of Commons, 1790–1820* (1986). The proportion of total cases with information on birth, entry and death by period are as follows: for 1660–1789, 95.7 per cent; for 1715–1754, 89.4 per cent; for 1755–1789, 95.8 per cent; and for 1790–1820, 98.2 per cent. A special study of these data is in process, but the preliminary findings are presented in Table 8.

Table 8. Average Number of Years Lived, Members of Parliament, 1660–1820, by Age at First Entry[54]

	Average Number of Years Lived After Entry into Parliament		
Date of First Entry	*Aged Under 29*	*30–39*	*40+*
1660–1690	25.71 (429)	22.58 (458)	17.87 (633)
1715–1754	30.83 (541)	28.17 (422)	18.52 (347)
1755–1789	37.13 (480)	29.86 (354)	21.16 (431)
1790–1820	38.06 (571)	32.04 (432)	22.40 (572)

There were sharp gains in life expectancy between 1660 to 1690 and 1715 to 1754, particularly for the younger age groups (under the age of 39). Mortality continued to fall during the later period, but was confined to MPs under the age of 29, and mainly for the period between 1715–1754 and 1755–1789.

The finding of a significant fall in mortality during the first half of the eighteenth century is supported by a number of existing studies. Perhaps the most important – and most neglected – is a study of government appointments made by John Finlaison, the actuary to the National Debt Office, which was published in 1829. Finlaison's data derived from four tontines run by the British government in the eighteenth century. A tontine was a device to raise revenue; it involved the payment of annuities to subscribers based on the survival of their nominees. Subscribers buying tontine shares were allowed to nominate whomever they wished. Most of them nominated themselves or, more frequently, their children. The annuity paid out by the government depended on the survival of individual nominees – survivors shared a fixed annuity sum among themselves – and their deaths were monitored by the Exchequer until the last nominee died, in very old age. For example, the last survivor of the 1693 tontine died in 1783.

Although a self-selected group, the subscribers came from all parts of the country and there is evidence that they were demographically representative of the social groups from which they originated.[55] The

[54] Calculations are to the nearest year and include only cases with full information on date of birth, first entry, and death. Figures in parentheses indicate number of cases.

[55] In the 1789 tontine, the government nominated over half of the nominees by lot, and their mortality rates were similar to that of the nominees of the subscribers. See John Finlaison, *Report On Life Annuities* (Parliamentary Papers, 1829, 3), pp. 7, 66, 67.

subscribers to the tontines were a mixture of aristocracy, gentry, merchants, and professional people, and though this was a limited social range, the precision and accuracy of the data helps counter-balance that limitation.[56] The smallest number of nominees was for the 1693 tontine (just over one thousand), but the numbers grew progressively throughout the eighteenth century. Table 9 summarises the mortality experience of the four tontines.

Table 9. Mortality Rates per 1,000 of all Nominees to British Tontines, 1693–1789[57]

	Date of Tontine			
Age Group	*1693*	*1745+*	*1773*	*1789*
5–15	9.12	5.65	5.75	6.75
16–30	18.44	9.27	10.32	10.14
31–45	20.21	12.61	11.88	11.05
46–60	31.57	22.93	17.09	18.57
61–75	66.09	66.81	51.89	77.39

There were marked falls in mortality among all age groups under the age of 60, most of which occurred between the first two tontines. For example, mortality among the 16–30 age group almost exactly halved between the 1693 and 1745 tontines. A majority of the nominees entered the tontines as children, though the survivors went on to be included in mortality calculations for the later age groups. The pattern of mortality revealed by the tontine data indicates that most of the reduction in mortality occurred in the first half of the eighteenth century.

A number of more recent studies confirm the above conclusion. Table 10 brings together all the available evidence, expressed in the form of male life expectancy at 25 years of age. The data are arranged in the sequence in which they were published.

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[56] In 1693 the proportion of subscribers listed as gentlemen (including aristocrats) was 59.1 per cent; professionals, 11.2 per cent; and merchants and others 29.7 per cent. The equivalent proportions in 1745 were 56.8, 10.5 and 32.7 per cent respectively. See *The British State Tontine of 1693*; and F. Leeson, *Guide to the British State Tontines* (1964), p. 7

[57] See John Finlaison, *Report on Life Annuities* (Parliamentary Papers, 1829, 3), pp. 66, 67.

Table 10. Expectation of Life (Years) for Males Aged 25 Years[58]

	Approximate Period				
	1600–1649	*1650–1699*	*1700–1749*	*1750–1800*	*1800–1824*
Social Group					
Tontine Nominees	—	28.0	34.5	36.4	—
Aristocracy	25.4	26.9	31.8	36.4	37.2
Reconstitution Sample	32.9	31.4	33.6	35.4	—
South of England Quakers	26.1	27.6	31.7	31.5	—
Scottish Advocates	28.8	31.1	38.0	38.1	—
Fathers Listed in Marriage Licences	26.9	28.6	—	37.9	—
Members of Parliament	—	25.7	30.8	37.1	38.0

The overall finding is that with the exception of the reconstitution sample and South of England Quakers, there was an increase in adult life

[58] These figures were prepared with the help of Jim Oeppen. In the case of the marriage licences, it was assumed that: (i) the average newborn child had a mother aged 32 and a father aged 35; (ii) the average child was aged 20 at marriage; (iii) Model North in Coale and Demeny was used for translating survivorship between the ages of 32 and 52 for women (35 and 55 for men) into expectation of life at age 25. For the reconstitution sample and the Quakers, conversion was made to expectation of life at age 25 by using the relationship between expectation of life at ages 25 and 30 in Coale and Demeny Model North life tables. More details can be obtained from Jim Oeppen at The Cambridge Group. Sources: The figures for: tontines, Finlaison, *Report On Life Annuities*; for the aristocracy T. H. Hollingsworth, "The demography of the English Peerage", *Population Studies*, Vol. 18, No. 2 (Supplement, 1964), p. 56; for the reconstitution sample (men aged 30) Wrigley and Schofield, *The Population History*, p. 250; for the southern Quakers (men aged 25–30) Richard Vann and David Eversley, *Friends In Life And Death* (1992), p. 229; for Scottish advocates Rab Houston, "Mortality in early modern Scotland", *Continuity And Change*, Vol. 7 (1992), p. 51; for fathers in marriage licences, data in this paper. For Members of Parliament the figures used are those listed in Table 8 of this article; they include MPs aged under 29 when entering Parliament.

expectancy between the seventeenth and eighteenth centuries of about ten years. Table 10 shows that the increase occurred throughout the whole eighteenth century, though the earlier and more detailed analysis revealed particularly sharp gains at its beginning. Whether this fall in mortality was sufficient to account for the whole of population growth is a question that can only be answered by further research.[59]

EXPLANATIONS OF THE FALL IN MORTALITY

What were the reasons for this radical decline in adult mortality? I have previously argued that smallpox inoculation made a significant impact on mortality in the late eighteenth century. In rural areas, where the majority of the population lived, this would have led to a reduction in adult mortality as well as child mortality, in spite of a significant increase in the virulence of the disease.[60] The data for Members of Parliament, the aristocracy, and the Quakers indicate a strong increase in life expectancy after 1750, which could be accounted for by the practice of inoculation during that time. However, smallpox inoculation was not practised on any scale in the first half of the eighteenth century and therefore cannot account for the marked fall in mortality found then. It is therefore necessary to consider other explanations for that period.

Real incomes probably rose for most of the population during the first half of the eighteenth century,[61] so it is possible that this improvement played a part in reducing mortality. Certainly the evidence of higher mortality among husbandmen in the early seventeenth century would suggest that economic factors were important during this early period, but

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[59] A ten-year increase in expectation of life at birth would be more than adequate to explain population growth between 1695 and 1841, assuming that fertility was high during the eighteenth century. Given that the marriage licences indicate a low age at first marriage of women in the last seventeenth century, this assumption is not unrealistic. The development of a model of population change reflecting the mortality changes discussed in this paper, is one of the priorities of future research. I am grateful to Jim Oeppen for commenting on the implication of the changes in mortality for population growth.

[60] Peter Razzell, *The Conquest Of Smallpox* (1977).

[61] Wrigley and Schofield, *The Population History*, p. 643.

the weight of evidence suggests that they were not central in bringing about the overall fall in mortality. The substantial mortality gains among all the socio-economic groups discussed in this article indicate that non-economic forces were of primary importance, but only further research will definitively settle this issue.

It is possible that there was a spontaneous decline in the severity of various diseases at the end of the seventeenth century. However, there is no evidence for this; smallpox, for example, was increasing in virulence throughout the eighteenth century. Certain changes in the environment associated with economic development may have played a role in reducing mortality; there is good evidence that malaria was present in the marshlands of south-eastern England, and the draining and enclosure of those areas may have reducing mortality.[62] However, the disease was probably confined to restricted areas of the country.

We can provisionally explore one hypothesis that fits all the known evidence: that the main fall in mortality during the early eighteenth century occurred because of the marked improvement in domestic hygiene associated with the rebuilding of English housing at that time. It was linked with a move away from older building materials – in particular, earthen floors. Such floors had been commonplace since medieval times in the houses of rich and poor alike. In the seventeenth century, according to M. W. Barley, even among the clergy, "Earth floors were almost universal; even if suitable stone was available locally for flagging the hall, the service room still had earth floors throughout this period ... The use of brick for paving, as for infilling, belongs to the period after 1660."[63] In their history of English housing Bill Breckon and Jeffrey Parker drew his attention to a neglected, if colourful, area of social history:

> Up to the 18th century ... the ground floor of the house was simply beaten earth ... dusty and strewn with straw, rushes or grasses ... [with] some nastiness seeping into the floors, not only from dog and cat excrement but with human urine as well, for our ancestors were not too bothered about sanitation. Whatever its source, the result was

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[62] Mary Dobson, "The last hiccup of the old demographic regime: population stagnation and decline in late seventeenth- and early eighteenth-century south-east England". *Continuity And Change*, Vol. 4, No. 3 (1989), p. 413.

[63] M. W. Barley, "Rural housing in England", in Joan Thirsk (ed.) *The Agrarian History Of England And Wales* (1967), Vol. 4, p. 727.

> that the floors soaked up material rich in nitre – the 'saltpetre' used in making gunpowder. Since this was scarce, the Crown turned to floors as a rich source of much-needed war material, and empowered 'saltpetre men' to enter people's homes, dig up and take away their floors.[64]

The demand for saltpetre for the manufacture of gunpowder was of such critical importance, that these men were allowed to dig up the floors of bedrooms, halls, butteries and other rooms in the house, as well as the floors of churches, town halls, pigeon lofts, and stables.[65] This activity created passionate opposition, particularly when it involved the digging up of earth under the beds of invalids, pregnant women, and old people.[66] Some householders managed to avoid having their houses disturbed by bribing the government's men. However, the importance of the extraction of saltpetre from houses from our point of view is that it indicates the highly unhygienic state of the floors of many English houses in the seventeenth century. The "powers of seisin" of the saltpetre men were revoked in 1656, although the practice of using house floors as a source of saltpetre seems to have continued until the end of the seventeenth century, when its importation by the East India Company made the practice redundant.[67]

Barley gives a detailed account of the history of farmhouses and cottages, in which earthen floors persisted until the early eighteenth century. Church records for Lincolnshire and Bedfordshire reveal that in parsons' houses during Queen Anne's reign

> Earthen floors were still very much the rule rather than the exception ... some houses could be found with nothing else ... The next best thing was brick, and about half of the Lincolnshire houses had one room so paved ... usually the hall. In Bedfordshire the majority of halls were paved, and so were about half the kitchens.[68]

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[64] Bill Breckon and Jeffrey Parker, *Tracing The History Of Houses* (1991), pp. 135–36.

[65] E. A. B. Hodgetts, *The Rise And Progress Of The British Explosives Industry* (1909), pp. 12–28, 213–300.

[66] *Ibid.*

[67] See William Clarke, *The Natural History Of Nitre* (1670), p. 21, for a reference to the continuation of the practice after the 1656 legislation.

[68] M. W. Barley, *The English Farmhouse And Cottage* (1961), p. 258.

The persistence of earthen floors into the late seventeenth century perhaps explains some unsanitary practices of the aristocracy during this period. When Charles II and his court spent the summer of 1665 in Oxford to escape the plague, they were castigated by the diarist Anthony Wood: "Though they were neat and gay in their apparell, yet they were very nasty and beastly, leaving at their departure all their excrements in every corner, in chimneys, studies, colehouses, cellars".[69] That such unhygienic practices were commonplace is suggested by Pepys's diary; he himself used a chimney for not dissimilar purposes.[70] This behaviour was probably due to the absence of toilets in some houses, even those of the rich, until the eighteenth century.[71]

Barley's work suggests that earthen floors were gradually replaced as brick was widely introduced for domestic house building, a process triggered by the great town fires that swept through England during the late seventeenth and early eighteenth centuries. The timing of the process of rebuilding in brick and tile coincides with the early eighteenth-century decline of adult mortality previously discussed.[72] This rebuilding of houses appears to have enabled a revolution in domestic hygiene to take place. As early as 1727 De Saussure could write:

> The amount of water English people employ is inconceivable, especially for the cleansing of their houses. Though they are not slaves to cleanliness, like the Dutch, still they are very remarkable for this virtue. Not a week passes by but well-kept houses are washed twice in every seven days, and that from top to bottom; and every morning most kitchens, staircase, and entrance are scrubbed. All furniture, and especially all kitchen utensils, are kept with the greatest cleanliness.[73]

Whether this account was true of just London or the whole country is open to question, but certainly the eighteenth-century English acquired a repu-

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[69] Quoted in Lawrence Wright, *Clean And Decent* (1960), p. 76.

[70] Christopher Hibbert, *The English: A Social History* (1987), p. 335.

[71] *Ibid*, pp. 196, 335.

[72] E. L. Jones and M. E. Falkus, "Urban improvement and the English economy", in Peter Borsay (ed.) *The Eighteenth-Century Town, 1688–1820* (1990), pp. 120, 145, 146.

[73] Caesar De Saussure, *A Foreign View Of England* (1902), p. 157.

tation for domestic cleanliness that was reflected in the writings of other foreign visitors.[74]

CONCLUSION

The growth of population in eighteenth-century England was primarily due to a fall in mortality, which was particularly marked during the first half of the century. The fall appears to have affected all socio-economic groups and does not seem to be explained by economic improvements. The introduction of smallpox inoculation contributed to the phenomenon, but the major hypothesis considered here is that there was a very significant improvement in domestic hygiene linked with the rebuilding of housing in brick and stone, triggered by the great town fires that swept England in the late seventeenth and early eighteenth centuries.

The population growth that resulted from falling mortality had profound consequences for both the economy and the social structure of England. It both stimulated demand through increasing the number of consumers and transformed the organisation of production because of the impact of surplus labour. These economic changes culminated in the industrial revolution, which was accompanied by a polarization of wealth. But even this polarization was strongly influenced by population growth, since the impoverished surplus labour force was largely created by the rapid increase in population.

This article poses major questions about population, economy and society. More research is required before authoritative conclusions can be reached, particularly about the causes of population growth. Research using local censuses, parish registers, and marriage licences will allow an analysis of variations in mortality by town and region and of changes over time. Additionally, detailed work will have to be undertaken on the history of hygiene and its impact on health and illness. Only when this research has been undertaken – which is likely to constitute a major project over a number of years – will be possible definitively to explain population growth in eighteenth-century England.

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[74] Francesca Wilson, *Strange Island* (1955), pp. 119, 125, 129.

Chapter 8

Recent Research on Eighteenth-Century Population History

This essay summarises my latest thinking and research findings on eighteenth-century English population history. It includes a review of both the work of other scholars and some of my own recent unpublished research findings. Two subjects are covered: (1) The discussion of the reliability of parish burial registers. (2) The explanation of the decline in eighteenth-century mortality. The conclusion of this review is that there is still great uncertainty about the nature and explanation of eighteenth-century population growth, and that the successful resolution of this conundrum is likely to involve a range of different explanations of mortality decline.

In this concluding essay, I wish to address two major topics which have a direct bearing on the themes dealt with in this book: the issue of parish register reliability, and the explanation of eighteenth-century population growth. The main reason for the uncertainty about the increase of population is the unknown quality of the basic source material, parish registers. It was on the basis of parish register evidence, that historians came to believe that population growth accelerated in the latter half of the eighteenth century. My own recent research suggests that this chronology is incorrect. Evidence from the study of groups with reliable non-parish register information – the aristocracy, members of parliament, subscribers to tontines, fathers of brides marrying by marriage licence, and Scottish lawyers – shows that mortality began to fall sharply at the *beginning* of the eighteenth century, and probably decreased throughout the rest of the century. This evidence contradicts that generated by the Cambridge Group, based on back projection and family reconstitution, which showed only an insignificant fall in mortality in the eighteenth century. The detailed reasons for this

discrepancy have been discussed earlier in the book, but in this concluding essay I will focus on the central issue of burial register reliability.

Wrigley and Schofield have assumed in their reconstitution work that parish registers were 100 per cent accurate during the period 1538–1837. They have made this assumption partly on the basis of selecting what they believed to be reliable parish registers. For their aggregative work, they have generated burial inflation ratios through various statistical calculations.[1] On the basis of these calculations, Wrigley and Scholfield have assumed that about 10 per cent of burials were missing from parish registers up to the end of the eighteenth century, increasing to 26 per cent by the early nineteenth century.[2] These assumptions are clearly critical, because any change in them would have an effect on the pattern of mortality.

In order to evaluate these assumptions, it is necessary to analyse all sources of information: parish registers, bishops' transcripts, monumental inscriptions, wills, poor law returns, local censuses, apprenticeship documents, and manorial records. It is only through the "triangulation" of different sources that reliable results can be achieved. I will illustrate what is possible in this respect by quoting provisional results of a comparison of wills with burial registers.

There are two aspects to burial registration reliability: the recording of burials generally, and the registration of individuals in a particular parish. The first affects both back projection and family reconstitution, whereas the second only affects family reconstitution. This is because back projection relies on aggregative data which covers groups of individuals not necessarily linked in any way. With family reconstitution, links are made between events associated with the same individual – for example a person being traced in both the baptism and burial register. It is assumed in family

[1] It is impossible to summarise the nature of the calculations here, the reader is advised to read E. A. Wrigley and R. S. Schofield, *The Population History Of England* (1981), for the source of these calculations. I have given a critical discussion of the inflations adopted for baptisms in the previous chapter, and similar considerations would apply to burial inflations.

[2] The exact figures are: 1538–99: 9.8%; 1600–49: 8.7%; 1650–99: 10.6%; 1700–49: 5.7%; 1750–99: 10.3%; 1800–38: 26.0%. I have calculated these percentages by multiplying initial and secondary sources of burial under registration listed in Wrigley and Schofield, *op. cit.*

reconstitution that all events take place in the parish of residence, so if an individual was baptised in his family's parish of residence, but buried outside it, he would not be registered in reconstituted mortality statistics. We therefore need to check both the general unreliability of the recording process, and the extent of the practice of burying individuals outside their parish of residence – the "traffic in corpses".

We are fortunate in having a source of data which allows a check on both forms of registration unreliability. The Birmingham and Midland Society for Genealogy and Heraldry have recently completed an index of burials in all Staffordshire parish registers for the period 1538 to 1837 – about one million entries. Ten parishes located centrally in the county were chosen for study, each having surviving wills and burial registers for the period covered. 200 wills were selected in sequence from the probate lists – 20 from each parish – and were chosen to cover the three-hundred period involved. A search was made for the burials of this will-leaving population, both in the Staffordshire burial index and the individual parish registers.[3] Table 1 presents the results of this search, giving the proportions of burials traced in each parish.[4]

[3] The search was made for a ten-year period prior to the date of probate in the burial index, and five years in the parish registers. The index search was kindly conducted by Tony Bowers of the B. M. S. G. H., while I carried out the parish register search.

[4] The criteria for a traced case was that a burial had taken place in the parish of residence less than ten years previous to the date of probate. A conservative procedure was adopted of assuming that all similar names in wills and burial registers during this period were correct matches — this would tend to over-estimate the proportion of traced cases. Most cases were found in the previous year to the probate date – 88.1 per cent – and 69.8 per cent were found less than six months previous to that date. Given uncertainty about correct identification, it was decided to define a traced case occurring in a non-residential parish when a burial took place six months before the date of probate in that parish. The figures for Bushbury had to be adjusted to take account of large gaps in the register before 1662.

Table 1. Numbers of People Leaving Wills Traced in Burial Registers in Staffordshire, 1538–1837

Parish	*Traced*	*Not Traced*
Barlaston	14	6
Caverswell	13	7
Cheadle	15	5
Dilhorne	18	2
Stone	10	10
Bushbury	14	6
Darleston	17	7
Penkridge	13	7
Walsall	15	5
Wednesbury	14	6

There were significant variations in the numbers of burials traced, from a high proportion of 90 per cent in Dilhorne, to a low figure of 50 per cent in Stone. The overall average of burials not traced is 33.5 per cent, suggesting that about a third of all adult burials were not registered in parish registers. Only a small minority of traced burials – 9 out of 143 (6.3 per cent) – were found in neighbouring parishes, indicating that the "traffic in corpses" was not of major significance. (This should not surprise us, as virtually all wills stipulate burial in the parish of residence.)

The proportion of burials traced also varied over time. The following table summarises the changing proportion of untraced burials for the whole sample during the three-hundred year period.

Table 2. Proportions of People Leaving Wills Untraced in Burials Registers, Staffordshire 1538–1837

Period	*Traced*	*Not Traced*	*Total Cases*	*Proportion Not Traced*
1538–1649	31	23	54	42.6%
1650–1749	48	18	66	27.3%
1750–1837	66	14	80	17.5%

This table suggests that burial registration improved significantly during the three-hundred year period, and that it was particularly poor in the late sixteenth and early seventeenth century. However, it is not possible to reach firm conclusions on a sample of 200 cases. What is required is a large-scale study of will/burial registers for a randomly representative

sample of parishes, including information on the dates of making and probating of wills.

In one respect the tracing of the burials of people leaving wills is a mild test of burial registration accuracy. Those who made wills were adults – usually males – who owned property and were of high social status. We would expect families of such people to ensure the registration of their burials, particularly because of the legal implications of property transfers. One way of comparing the burial registration accuracy of the rich and the poor is to check the burials of will-leavers against that of paupers. Many parishes paid for the burial of the poor, leaving detailed records of burial expenses, including the provision of coffins and the carrying of the dead to the grave. Lyn Boothman has made such a comparison for the parish of Long Malford in Suffolk. Of 97 people who left wills in 1559–1610, 20 (20.6%) could not be located in the burial register, compared to 34 of 52 (65.4%) paupers who were buried in a similar period.[5] Lyn Boothman has suggested that the very high burial omission rates amongst the Long Melford poor may have been due to the non-payment of burial fees by poor law authorities.[6]

However, the proportion of unregistered pauper burials was not necessarily disproportionate. The pauper burials in Whitchurch, Oxfordshire and Folkestone, Kent, were checked in the appropriate parish registers, with the following results:

[5] I am grateful to Lyn Boothman for sending me details of the burials of the will-leaving population and the paupers of Long Melford.

[6] See Lyn Boothman, "Letter on Long Melford parish registers", *Local Population Studies,* No. 50, Spring 1993, pp 80, 81.

Table 3: Comparison of Poor Law Records and Parish Registers[7]

Period	*Burials Not Found*	*Total Burials*	*Proportion Burials Not Found*
Whitchurch			
1651–1750	19	93	20.4%
1751–1800	15	68	22.1%
Folkestone			
1732–51	10	57	17.5%
1752–87	6	57	10.5%

The percentages of missing pauper burials in Whitchurch and Folkestone are smaller than that found in Long Melford, although they were still significant – about a fifth in the pre-1751 period. They are in the range of untraced burials found amongst the will-leaving population in Staffordshire during the same period (27.3% for 1650–1749 and 17.5% for 1750–1837). These early finding on the registration of pauper burials suggest an improvement of burial registration over time, although the samples are too fragmentary to allow firm conclusions.

The probable explanation of poor burial registration was given to the Select Committee on Parochial Registration in 1833. One of the witnesses, Mr William Durrant Cooper, a solicitor, had extensive experience of tracing individuals in parish registers for property cases, and concluded that parish registration was "exceedingly defective ... [with] a very large number of marriages, deaths and baptisms ... not entered at all...especially deaths".[8] To illustrate this, he gave the following example:

> On the sale of some property [in 1819] from Mr Cott to Lord Gage, it was necessary to procure evidence of the death of three individuals, Mrs Pace, Mr Tuchnott and Mrs Gouldsmith. They were at different places, all in Sussex; Mrs Pace was regularly entered; Mr Tuchnott was buried at Rodmell, about five miles from Lewes, and on searching for the register of burial we found no entry whatever. On making an inquiry in the churchyard of the sexton, he stated he

[7] I am grateful to Richard Adair for providing the figures for Whitchurch.

[8] *Report Of The Select Committee On Parochial Registration*(Parliamentary Papers, 1833, XIV), p. 24.

> recollected digging the grave, and the ceremony being performed; Mr Gwynne, the rector, whose neglect in that and other parishes is well known, had omitted to enter it ... Mrs Gouldsmith, who was buried at Waldron, in the same county, was not entered, but on going to the parish clerk, who was a blacksmith, he stated he recollected the circumstance, and accounted for her burial not being entered in this way: he said it was usual for him, and not the clergyman, to take an account of those who were buried, and he entered them in a little sixpenny memorandum book ... If it so happened that the fee [of one shilling] was paid at the time, as was the case with affluent persons, no entry would appear in his book, he only booked what was due to him, and as the clergyman entered the parish register at the end of the year from his book, and not at the time of the ceremony, all burials that were not entered in his book would not find their way into the register.[9]

The above account cautions us against the assumption of a necessary correlation between socio-economic status and burial registration. (In Waldron, the rich who paid their burial fees were ironically excluded from the burial register, whereas those parishioners who were lax in payment, were usually registered.) English parish registration was a haphazard process and much more evidence is needed before it is possible to reach general conclusions. However, we can provisionally conclude from existing evidence that many burials were omitted from the parish register because of clerical negligence, a topic further discussed in Chapter 4.

Although the Cambridge Group have attempted to select reliable parish registers for its reconstitution work, in practice problems remain. As we have seen in the last chapter, 28.1 per cent of people leaving wills in Colyton could not be traced in the burial register, and the evidence from the same-name technique also suggested similar problems with the burial registration of infants and children – over 40 per cent of same-name burials could not be traced in the Colyton burial register in the first half of the seventeenth century. Some of this may have been due to the practice of giving same-names to two living siblings, but the evidence is that this only happened on a very limited scale.[10] The evidence from Colyton also

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[9] *Ibid*, p. 25.

[10] I estimate from a provisional study of wills – which give information on living same-name children – that about 15 per cent of all same-name cases at the end of the sixteenth century were two living siblings.

suggested that burial registration amongst children improved over time, particularly during the early nineteenth century.

How typical was Colyton of other parishes in its same-name practices? We have seen that about two-thirds of eligible families (a family was eligible when at least one of its children was baptised after the burial of a sibling of the same sex) gave same-names to their children in Colyton. I have analyzed the reconstitution schedules for the parishes of Dawlish, Eccleshall, Bridford, Austey, March and Aldenham. The following table summarises the proportion of eligible families who gave same-names to their children in these parishes.

Table 4. Number of Families with Same-Names as a Proportion of Eligible Families

Parish	*Number of Families with Same Names*	*Number of Eligible Families*	*Proportion of Families with Same Names*
Dawlish	206	307	67.1%
Eccleshall	268	443	60.5%
Bridford	84	139	60.4%
Austey	102	155	65.8%
March	482	678	71.1%
Aldenham	296	524	56.6%
Colyton	472	733	64.4%
Total	1910	2948	64.8%

Most parishes had at least 60 per cent of its families giving same-names to their children, and the overall average – 64.8% – is almost identical to the Colyton proportion (64.4%). Given that about two-thirds of all families practised same-naming, we can provisionally conclude that the families practising it were representative of the general population.

An analysis of same-name cases in nine reconstitution parishes, suggests that burial registration for infants and children improved over time.

Table 5. The Analysis of Burial Registration of Same-Name Siblings in Colyton, Hartland, Aldenham, Dawlish, Ansty, Bridford, Eccleshall, March, Shepshed, 1538–1837

Period	*Total Same Name Cases*	*Burials Not Found*	*Proportion of Burials Not Found*
1538–99	358	122	34.1%
1600–49	465	144	31.0%
1650–99	617	167	27.1%
1700–49	858	191	22.3%
1750–99	594	160	27.0%
1800–37	451	104	23.1%
1838–50	72	7	9.7%

A number of qualifications need to be made about these figures, and they do not represent a reliable measure of trends in burial registration accuracy. The parishes included in the sample varied from period to period, and there were strong variations from parish to parish which would affect the overall results. Only two parishes – Colyton and Shepshed – were included in the 1838–50, and the small numbers in that period mean that its proportion of missing burials is not a reliable measure of overall burial registration. Also the above figures make no allowance for burial non-registration on account of the delay between baptism and burial (children often not being baptised) – and this will inflate the figures in Table 5. (Although this might be counter-balanced to some extent by the allowance to be made for living same-name children.)

Only detailed research on a substantial number of registers – and using a variety of sources for comparison purposes – will reveal the overall pattern of burial registration reliability. However it is clear from the evidence on wills, pauper records and same-name patterns that burial registration was much more defective in the sixteenth, seventeenth, and early eighteenth centuries, than has been previously thought.

It is likely there were similar problems with baptism as there was with burial registration. Assumptions about proportions of births omitted from baptism registers affect calculation of the number of births and measures of fertility, and this is discussed in Chapter 7. The question of baptism registration adequacy is dealt with in the fourth and fifth essays of this book. The main conclusion of this work – that about a third of all births were omitted from baptism registers from at least the 1760s onwards – is consistent with findings on burial registration. It will be possible to further

evaluate the accuracy of baptism registers by means of the application of the same-name technique to different parish registers, which involves tracing baptisms of dead siblings bearing the same name.

One important topic not covered fully in this book is marriage – a key issue which is central to the Malthusian interpretation of population change. In Chapters 1 and 7, I discuss the question of age at first marriage, but there is virtually no discussion of the propensity to marry. In my first essay I briefly touched on the issue, and concluded that the marriage rate was more-or-less constant during the eighteenth century. There is now some evidence to cast doubt on this conclusion. It is likely that the clerical negligence which occurred with the registration of births and deaths, also applied to the recording of marriages. Lyn Boothman found that many marriages were missing from the Long Melford parish register in the late sixteenth century: 35 marriages were found in the bishops' transcripts but not in the marriage register – about a quarter of the total. Other than the bishops' transcripts, there are few alternative sources of information to parish registers to evaluate the reliability of marriage registers. It is likely that Hardwicke's Act of 1753 improved marriage registration, with more marriages accurately registered after this date. As a result, there were probably more marriages in the early eighteenth century than recorded in the parish registers. This means that statistics of marriage rates based on parish register evidence probably conceal a decline in the propensity to marry which took place in the eighteenth century.

There is virtually no direct evidence on the propensity to marry in the eighteenth century. There is however some data on the propensity to remarry based on widow and widower remarriages. Wrigley and Schofield note in their *Population History of England* that the proportion of widow marriages to all marriages fell from about 30 per cent at the end of the seventeenth century to approximately 10 per cent at the end of the eighteenth.[11] This conclusion was based on fragmentary parish register returns – most registers do not give the marital status of marrying parties – but information in marriage licences from a number of counties points in the same direction. Perhaps the most reliable evidence to illustrate this is that for the Diocese of Canterbury, involving 289 East Kent parishes.

[11] Wrigley and Schofield, *op. cit.*, pp 258, 259.

Table 6. Proportion of Widows and Widowers Married by Licence in the Diocese of Canterbury, 1568–1809[12]

Period	*Total Number Of Marriages*	*Number of Marriages Of Widows*	*Percentage Of Widow Marriages*	*Number of Marriages Of Widowers*	*Percentage Of Widower Marriages*
1568–1618	1,000	323	32.3%	—	—
1619–1646	1,000	318	31.8%	318	31.8%
1661–1676	1,000	283	28.3%	316	31.6%
1677–1700	1,000	261	26.1%	326	32.6%
1701–1725	1,000	197	19.7%	221	22.1%
1726–1750	1,000	216	21.6%	208	20.8%
1751–1780	1,000	152	15.2%	179	17.9%
1781–1809	1,000	120	12.0%	181	18.1%

These figures confirm the very major decline in the proportions of widow and widower remarriages in the eighteenth century – by nearly two-thirds for widows, and a half for widowers. What could be the explanation of this decline?

Evidence exists which suggests that the propensity to remarry was changing sharply during the period. The East Kent licences give information on whether or not a widowed mother had remarried by the time of her daughter's marriage, allowing a calculation of the proportions of widows remarrying. The following table is for mothers of women marrying under the age of 21.

Table 7. Proportion of Widowed Mothers Remarrying in East Kent[13]

Period	*N*	*Numbers Remarried*	*Proportion Remarried*
1619–1646	100	49	49%
1661–1676	72	37	51%
1751–1780	100	10	10%
1781–1810	100	9	9%

This table reveals that the proportion of remarried widowed mothers at the

[12] These figures are derived from J. M. Cowper (ed.), *Canterbury Marriage Licences* (1876), (1892), (1894), (1896), (1898), (1905), (1906); and Arthur Willis (ed.), *Canterbury Marriage Licences* (1967), (1969) and (1971).

[13] *Ibid.*

end of the eighteenth century was a *fifth* of what it had been at the end of the seventeenth. It therefore indicates a radical decline in the propensity to remarry. This finding is consistent with the evidence on the declining proportion of widow marriages in Table 6. The explanation of this change is beyond the scope of this chapter; it may be that there were fewer opportunities for remarriage as a result of declining mortality and a reduction in the number of widowers, and that the declining proportions of widow and widower marriages were a function of the fall in mortality depicted elsewhere in the book.

Clearly, some very fundamental changes in the structure of marriage were occurring in the eighteenth century. The factors involved are likely to be complex. There is some evidence that the declining propensity to remarry was associated with a general decline in the tendency to marry. The following table offers a comparison of marital status by age in the 1695 and 1851 Lichfield censuses.

Table 8. Age and Marital Status in Lichfield, 1695 and 1851

(N = Total Number In Age Group,
P = Total Proportion Married Or Widowed)[14]

Age Group	*Period*			
	1695		*1851*	
	N	*P*	*N*	*P,*
15–19	171	0.6%	199	1.0%
20–24	147	15.0%	146	21.2%
25–29	144	50.0%	147	53.7%
30–34	111	77.5%	115	60.9%
35–39	138	84.1%	101	77.2%
40–44	62	95.2%	113	77.9%
45+	274	98.2%	432	81.5%

These figures indicate that there was a fall in the total proportion of women marrying between the end of the seventeenth and middle of the nineteenth centuries. In Lichfield in the 1690s it is clear that almost all women had been married at least once during their lifetime: only 2 per cent remained

[14] The 1695 proportions are derived from figures given by D. V. Glass and D. E. C. Eversley (eds.), *Population In History* (1965), p. 181; the ones for 1851 are based on a 1 in 2 sample of the 1851 census schedules for Lichfield.

unmarried after the age of 45. By comparison, in 1851 the figure was very much higher, rising to nearly 20 per cent.

It is of course not possible to make generalizations on the basis of a single community (although Gregory King believed Lichfield to be demographically typical.) There is some evidence that Lichfield was not unrepresentative of other communities at the end of the seventeenth century: for example, there were no known spinsters among the 69 women over the age of forty-five living in Chilvers Coton, Warwickshire in 1684.[15] However, as we might expect, there were local variations, so that for example 15 out of a total of 161 women over the age of forty-five – 9.2 per cent – living in Stoke-on-Trent, Staffordshire were unmarried in 1701.[16] Also evidence from wills indicates that most people married in the early seventeenth century: of 204 men who left wills in Essex during 1597–1603, only 19 (9.3%) were unmarried at the time of death, even though some of them were clearly young men. When linked with parish registers, information in wills should enable an analysis of the changing proportions of will-leaving men who had married by certain ages during the period 1538–1837.

Additionally, a comprehensive survey of all late seventeenth-century censuses with relevant information on age and marital status should allow a comparison of the marriage/age distributions with the equivalent data for 1851, as in Table 8.

Although fragmentary, the evidence considered above suggests that the propensity to marry and remarry declined in the eighteenth century. This strengthens the argument that it was falling mortality, rather than rising fertility, which was the key factor in eighteenth-century population growth.

II

My work suggests that mortality declined over the whole of the eighteenth and early nineteenth centuries, although the magnitude of the fall and its exact chronology had yet to be determined. Six major factors emerged

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[15] This information was kindly provided by Peter Laslett of the Cambridge Group.
[16] *Ibid.*

from the essays in this book for the explanation for the decline in mortality, which can be listed in approximate chronological order as follows:

1. The improvement of domestic hygiene associated with the rebuilding of houses in brick and tile – in particular the replacement of earth floors with brick, tile and timber flooring during the eighteenth century.
2. The growth in real incomes during the first half of the eighteenth century.
3. The progressive elimination of malaria with the drainage of marshlands associated with agricultural improvements.
4. The practice of smallpox inoculation and vaccination after the middle of the eighteenth century.
5. The replacement of woollen clothing with linens and cottons, allowing more frequent and thorough washing at the end of the eighteenth century.
6. The improvement in personal hygiene associate with the introduction of the water-closet and the bath at the beginning of the nineteenth century.

I will briefly comment on recent research on these various topics, and point to ways in which these hypotheses can be further elaborated. Little or no work has been done on the history of domestic hygiene, and I will return to this subject at the end of this essay.

The history of the standard of living in the eighteenth century is still a matter of controversy. Most evidence suggests that average real incomes were rising during the first half of the eighteenth century[17], and it is possible that this contributed to falling mortality. However, recent work by Nicholas, Steckel and Komlos on the average heights of the working population suggests that there was an overall decline of the standard of living from about 1730 to 1860[18] (although this is a matter

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[17] Wrigley and Schofield, *op. cit.*, p. 643.

[18] S. Nicholas and R. H. Steckel. "Height and health of English workers during the early years of industrialization", *Journal of Economic History*, Vol 51 (1991); John Komlos, "The secular trends in the biological standard of living in the United Kingdom, 1730–1860", *Economic History Review*, Vol 46, No. 1 (1993); John Komlos, "A Malthusian episode revisited: the height of British and Irish servants on colonial America", *Economic History Review*, Vol 46, No. 4 (1993).

of controversy[19]), and it is therefore unlikely that economic factors played a role in improving the expectation of life in this later period. Also, mortality appears to have fallen amongst all socio-economic groups – rich and poor alike – and therefore economic factors, including the per capita consumption of food, are not likely to have been of major importance. Professor Livi-Bacci has recently questioned whether nutrition was a significant factor in historical mortality patterns.[20] It therefore seems doubtful whether improving standards of life and nutrition made more than a very minor contribution to the reduction of mortality.

The history of malaria has been investigated in detail by Dr Mary Dobson, and although her full results have yet to be published, she has presented sufficient of her work to come to certain provisional conclusions. She has demonstrated that there were marked variations in mortality in different south-eastern parishes, with much higher mortality rates in marshland areas.[21] This conclusion can only be tentative at this stage, as Dobson has yet to present any evaluation of burial registration reliability in the different types of parish. It is unclear whether environmental improvements significantly reduced the amount of malaria in England, as the disease was still to be found in marshland areas in the nineteenth century. Also, it is not clear what contribution the elimination of malaria made to the total reduction of mortality, as the disease was confined to specific and limited areas of the country.

In my original article on the impact of smallpox inoculation on population growth – reproduced in Chapter 1 – I claimed that it largely accounted for the increase of population in the eighteenth century. In the light of new evidence considered in this book, this claim was clearly overstated. However, that the impact of inoculation was very significant, is confirmed by J. R. Smith in his book *The Speckled Monster*.[22] Smith concludes that inoculation was very widely practised after 1765, and rightly stresses the importance of general inoculations – the inoculation of

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[19] See R. Floud, K. Wachter and A. Gregory, *Height, Health And History; Nutritional Status In The United Kingdom 1750–1980* (1990)

[20] Massimo Livi-Bacci, *Population And Nutrition* (1991), p. xiii.

[21] Mary Dobson, "The last hiccup of the old demographic regime: population stagnation and decline in late seventeenth and early eighteenth-century south-east England", *Continuity And Change*, Vol. 4, No. 3 (1989).

[22] J. R. Smith, *The Speckled Monster* (1987).

whole parishes – which usually arose when there was a threat of an epidemic. As contemporaries believed inoculation spread smallpox to the unprotected, nearly everyone living in a parish was inoculated. It was in the interest of property owners to pay for the inoculation of the poor, as the cost of nursing and burying smallpox victims was so high. Additionally, in market towns trade was ruined for long periods – sometimes for over a year – on account of smallpox epidemics, and it was in the interest of merchants and traders to ensure the eradication of the disease as soon as possible.

Smith concludes that inoculation probably reduced childhood mortality in the eighteenth century by between 10 and 20 per cent, but cautions against excessive claims on its demographic impact.[23] In the light of the new evidence considered in this book, that conclusion may not be entirely unwarranted, although this issue can only be settled through further research. There are two points which Smith does not fully address: 1. The secondary mortality arising from smallpox. 2. The increasing virulence o the disease. It should be possible to settle the first issue by comparing the subsequent mortality of those catching natural smallpox, as against those who had been inoculated or vaccinated. (This would involve a research strategy very similar to that used by Rutten in his work on a Dutch municipality, to be discussed later.) On the question of the increasing virulence of the disease, we know from a number of surveys that the case-fatality rate of smallpox was about 16.5 per cent in the 1720s but had increased to over forty per cent by the mid-nineteenth century.[24] As smallpox was a universal disease, inoculation and vaccination may have prevented the deaths of up to a third of the population in the nineteenth century.

Smith raises the important point about the practice of inoculation outside of the South of England, suggesting that the technique may have been adopted ten years or so later in the North. We know from the writings of the Chester physician, Haygarth, that it was practised on a large scale in the North, but we have less information on its exact chronology and extent. Recently Deborah Brunton has reviewed the history of inoculation and its demographic impact in Scotland.[25] She presents evidence to show that its practice was more limited than it was in England, and that this was

[23] *Ibid*, p. 67.

[24] Peter Razzell, *The Conquest Of Smallpox* (1977), pp. 126–134.

[25] Deborah Brunton, "Smallpox inoculation and demographic trends in eighteenth-century Scotland", *Medical History*, Vol. 36 (1992).

partly the consequence of the periodicity of smallpox, as well as the persistence of Calvinist religious opposition. In the lowland areas, the disease was more-or-less endemic, spreading continuously through scattered and thinly populated areas, and affecting mainly infants and young children. This engendered a fatalistic attitude similar to that encountered in large towns in England. In the highlands and other areas of Scotland, smallpox tended to strike much more as an epidemic disease affecting all age groups, creating panic and provoking mass inoculations, particularly when the disease took a virulent form. This again was very similar to the experience of the English countryside, where the majority of the population lived.

On the basis of clinical evidence, it had been previously suggested by myself and others that the elimination of smallpox may have increased the overall level of fertility. Willibrod Rutten has established that in at least one nineteenth century Dutch municipality, there was little or no reduction in fertility amongst those men who had suffered a smallpox attack.[26] Given Rutten's new evidence, we can provisionally conclude that the gradual elimination of smallpox did not increase fertility, which is compatible with the findings on fertility discussed in Chapter 7.

Little or no research had been carried out on the question of clothing and personal hygiene since I wrote on those topics in the essay which forms Chapter 6 of this book. Contemporaries believed that the move away from woollens had improved hygiene and health. Gilbert White in his history of the rural parish of Selborne in Hampshire, commented on the abandonment of woollen clothing, improving health as a result. He wrote in 1778: "The use of linen changes, shirts or shifts, in the room of sordid and filthy woollen, long worn next to the skin, is a matter of neatness comparatively modern; but must prove a great means of preventing cutaneous ails".[27] Forty-four years later, Francis Place noted that "the success of the cotton manufactures" had enabled the working classes to "discard the woollen clothes which were universally worn by them, which lasted for years, and were seldom, if ever washed".[28]

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[26] Willibrod Rutten, "Smallpox, sub-fecundity, and sterility: a case study from a nineteenth-century Dutch municipality", *Social History of Medicine*, Vol. 6, No. 1 (1993).

[27] Gilbert White, *The Natural History And Antiquities Of Selborne*, (1789), p. 222.

[28] Francis Place, *Illustrations And Proofs Of The Principle Of Population* (1930), p. 253.

Whether this substitution of linen and cotton for woollen clothing had a significant impact on health is an open question; it is possible that the more frequent washing of clothes reduced body lice, fleas and other parasites, which in turn helped to eliminate dirt diseases such as typhus. These improvements would have been reinforced by the introduction of the bath and water-closet, subjects which also warrant more research attention. This leads naturally to a discussion of domestic hygiene, associated with the rebuilding of housing which began at the end of the seventeenth century. The topic is covered briefly in Chapter 7, and I will attempt now to give a fuller historical account of the subject.

The link between poor domestic hygiene and ill health was made as early as the sixteenth century. In 1517 Erasmus gave his well-known description of English housing and its effect on health.

> I am often surprised and distressed by the question how it can be that England has for so many years now been beset by continual pestilence, and in particular by the sweating sickness, which almost seems to be its speciality. We read somewhere of a city set free from a pestilence of long standing by rearranging the buildings on the advice of a philosopher. Unless I am much mistaken, a similar policy might set England free ... [In the buildings] the floors are generally spread with clay and rushes from some marsh, which are renewed from time to time but so as to leave a basic layer, sometimes for twenty years, under which fester spittle, vomit, dogs' urine and men's too, dregs of beer and cast-off bits of fish, and other unspeakable kinds of filth. As the weather changes, this exhales a sort of miasma which in my opinion is far from conductive to bodily health ... I should be confident that the island would be far healthier if the use of rushes were abolished, if rooms were constructed as to be open to the air of heaven on two or three sides, and all the glass windows were so made that they could be opened fully.[29]

These conditions persisted throughout the sixteenth century, and when the German traveller Paul Hentzner visited England in 1598, he noted that the floor of the "presence chamber" in Greenwich Palace was "strewn with

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[29] R. A. B. Mynors and Alexander Dalzell (eds.), *The Correspondence Of Erasmus* (1992), pp. 471, 472.

hay after the English fashion".[30] Similarly in the following year, Thomas Platter found the flooring in the "queen's quarters" at Hampton Court "strewn with rushes", and went on to describe his visit to the royal palace of Nonesuch:

> We were led very soon into the presence chamber ... [and this] apartment like all the others leading into this one was hung with fine tapestries, and the floor was strewn with straw or hay; only where the queen was to come out and up to her seat were carpets laid down worked in Turkish knot.[31]

Fragments of evidence suggest that conditions were even worse in the houses of the poor. For example, in 1591, the mayor and jurats of Maidstone issued a warning to the people living in the almshouses in Stone Street, Pudding Lane, and on the bridge, that if they continued to "keep hogs or swine in the rooms or houses where they lived", they would each be fined.[32]

Earth floors appear to have been more-or-less universal until the end of the seventeenth century, and we know many of them were in a highly unsanitary condition because they were the source of saltpetre (potassium nitrite), which arose as a result of the exposure of earth to urine and animal manure. Saltpetre was used for the manufacture of gunpowder, and the demand for gunpowder led the crown to license saltpetre men to dig up the floors of bedrooms, halls, butteries and other rooms in houses. These "powers of seisin" were eventually revoked by the Commonwealth Government of 1656, although the house floors continued to be a source of saltpetre until the end of the century. Covered in straw and in a highly unhygienic state, house floors were breeding grounds for fleas, rats and lice, all carriers of human disease. The building of covered floors reduced this population of parasites and was therefore probably a major factor in the improvement of health. It is possible that the elimination of earth floors in basements and lower ground floors in London, resulting from the rebuilding of the city after the great fire of 1666, was partly responsible for the disappearance of the plague.

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[30] Paul Hentzner, *Travels In England In The Year 1598* (1797), p. 33.

[31] Clare Williams (ed), *Thomas Platter's Travels In England in 1599* (1937), pp. 192–195, 202.

[32] J. M. Russel, *History Of Maidstone* (1881), pp. 223, 225.

The only statistical source of information on earth floors is that derived from glebe terriers (surveys of church property), compiled from the seventeenth century onwards. Some of this evidence has been presented by Maurice Barley in his work on English vernacular architecture, and is discussed in Chapter 7. Since Barley's work was published, the glebe terriers for Cornwall have appeared in print, which allows a statistical analysis of flooring in parsonages at the end of the seventeenth and beginning of the eighteenth centuries. In 1679, 50 per cent of the 62 parsonages surveyed had all floors made of earth, whereas by 1727 this proportion had fallen to 14 per cent (12 out of 41). Even more significant was the reduction of the proportion of kitchens with earth floors: 71 per cent (25 out of 35) in 1679, falling to 23 per cent (11 out of 48) by 1727.[33] These changes in the internal conditions of houses co-incided with the decline in mortality at the beginning of the eighteenth century and discussed earlier in the book.

There is insufficient evidence to say whether improvements in the housing of the ordinary population improved in the same way. There were undoubtedly great improvements in urban houses during this period, starting with the rebuilding of London after the great fire of 1666. Jones and Falkus have argued that the rebuilding of houses in brick and tile in early eighteenth-century towns "transmitted near-metropolitan models of a way of life and standards of consumption to almost the whole rural population".[34] We know that this conclusion applies to the housing of clergymen and other elite groups, but little in detail is known about the housing of the poor.

It is known however that earth floors survived into the nineteenth century. A number of the reports published in Edwin Chadwick's survey of sanitary conditions published in 1842 mentioned earth floors. Most references described them to be in a highly unsanitary condition, for as the report from Bedfordshire stated, "the bare ground ... cannot possible be cleaned".[35] The correspondent from Tranent in Scotland found the floors

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[33] Richard Potts (ed.), *A Calendar Of Cornish Glebe Terriers, 1673–1735* (1974).

[34] E. L. Jones and M. E. Falkus, "Urban improvements and the English economy in the seventeenth and eighteenth centuries", in Peter Borsay (ed.), *The Eighteenth-Century Town, 1688–1820* (1990), pp. 120, 145, 146.

[35] Edwin Chadwick, *On An Inquiry Into The Sanitary Condition Of The Labouring Population* (Local Reports, House Of Lords 1842, XXVII), p. 131.

of the colliers' houses to be in an indescribable condition: "These [earth] floors are very dirty, and so uneven as to make a stranger fall ... The odour of these apartments is most offensive and sickening, from the long-continued presence of human impurities. Persons not familiar with such situations will be unable to form the most remote idea of the disgusting nature of the atmosphere; but delicacy forbids a more detailed account".[36]

A report on the cottages of labourers living on the Scottish border explicitly drew a parallel with Erasmus's account more than three hundred years earlier:

> This earth floor ... is one of the causes to which Erasmus ascribed the frequent recurrence of epidemic sickness ... It is not only cold and wet, but contains the aggregate filth of years, from the time of its first being used. The refuse and dropping of meals, decayed animal and vegetable matter of all kinds, which had been cast upon it from mouth and stomach, these all mix together and exude from it.[37]

A similar account was given for parts of Buckinghamshire:

> The cottages of Waddesdon, and some of the surrounding parishes in the Vale of Aylesbury, are constructed of mud, with earth floors and thatched roofs ... The earth of the floor is full of vegetable matter, and from there being nothing to cut off its contact with the surrounding mould, it is peculiarly liable to damp. The floor is frequently charged with animal matter thrown upon it by the inmates, and this rapidly decomposes by the alternative action of heat and moisture ... Fever of every type and diarrhoea are endemic diseases in the parish and neighbourhood.[38]

Earth floors were also reported in Bedfordshire, Devon, Dorset, Lancashire, Monmouthshire, Northumberland, Rutlandshire and Scotland.[39] They were to be found in the basements of the houses of the poor in cities like Liverpool, although "generally ... [the floors were] flagged, and in a few cases, boarded".[40] In some areas, the correspondent gave a contemporary

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[36] *Ibid*, XXVIII, p. 86.
[37] *Ibid*, XXVI, p. 22.
[38] *Ibid*, p. 268.
[39] *Ibid*, XXVI, pp. 6, 8; XXVII, pp. 12, 100, 101, 113, 132, 159, 284, 306, 419.
[40] *Ibid*, XXVII, p. 284

history of housing, including the type of flooring. Mr Chrisp, a farmer from Northumberland wrote:

> The older cottages in the country, such as were generally built by the farmers for his servants, consist of a rough wall of lime and stone, covered with thatch, and with nothing but an earthen floor, except a flag-stone or two near the hearth. Cottages of a superior description have been latterly erected by the landlord when he agrees to build, being covered with blue slate, the side walls plastered, and the floor of stone flagging.[41]

But in some ways the reports in Chadwick's survey are misleading. He had invited medical officers and others to submit details of sanitary problems, rather than objective accounts of typical conditions. This can be illustrated by the report made on Berkshire housing, where the medical officer wished to draw attention to the problem of dampness in cottages:

> In Berkshire the floors of the cottages are laid with red tiles, called 'flats', or with bricks of a remarkable porous quality ... The cleanly housewife, who prides herself on the neat and fresh appearance of her cottage, pours several pails upon the floor, and when she has completed her task with the besom, she proceeds to remove with a mop or flannel so much of the water as the bricks have not absorbed. After having cleansed the cottage, the fire is usually made up to prepare the evening meal, and vapour is created by the action of the heat upon the saturated floor".[42]

Often the condition and cleanliness of cottages were only revealed in asides, as when a medical officer gave an account of the life of the Sussex labourer: "I will first describe the cottage and mode of living of a Sussex labourer as to make him one of the most distressed of his class ... On entering, the cottage displays a room about 20 feet long by 15, paved with brick ... The cottage ... is clean and well-drained".[43] Sometimes correspondents did explicitly state that unhygienic conditions were not a health problem in their area:

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[41] *Ibid*, p. 419.
[42] *Ibid*, XXVI, p. 269.
[43] *Ibid*, XXVII, p. 38.

> I have the more pleasing duty to state, that throughout the greater part of these counties [Kent and Sussex] comparatively few diseases are found to arise from the want of sanitary precautions. Mr Evans, the medical officer of part of the Ticehurst union, says – "The situation of this district being high and dry, and the cottages of the labourers in general well-ventilated and clean, and no accumulation of filth about them, we have no cause of complaint".[44]

The high standard of domestic hygiene was probably one of the reasons why agricultural labourers had a very low adult mortality rate in East Kent at the end of the eighteenth century.[45] A number of correspondents contrasted the hygienic conditions of the interior of cottages and houses, with that outside. The medical officer of Spalding in Lincolnshire believed that the accumulation of filth around cottages was due to ignorance, as "cleanliness prevails generally *within* the cottages".[46] And similarly, a Mr Hassell writing of the Penrith area of Cumberland stated that "the country cottages in this district are ... kept very clean by the labourers' wives".[47]

But this conclusion about the high standard of domestic hygiene in labourers' families can only be tentative, given the paucity of work on the subject. A great deal more research needs to be done not only on this topic, but all aspects of disease and mortality in the eighteenth and nineteenth centuries. Special focus must be placed on housing conditions and their effects on health. Additionally, detailed work on occupational mortality will enable an assessment of the role of socio-economic factors in mortality. It is hoped that the present book will re-open the debate about the role of mortality in population change, as well as providing an introduction to some of the unresolved problems of early modern English demography.

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[44] *Ibid*, p. 43.

[45] According to data in the East Kent marriage licences, the parental mortality rate of women marrying labourers was one of the lowest of all occupational groups at the end of the eighteenth century. This echoes the low adult mortality rate of labourers in mid-Victorian England. See Michael Haines, "Conditions of work and mortality decline", in R. S. Schofield *et. al.* (eds.), *The Decline Of Mortality In Europe* (1991), p. 183.

[46] Chadwick, *op. cit.*, XXVII, p. 156.

[47] *Ibid*, XXVIII p. 428.

Discussion Point

Should Remaining Stocks of Smallpox Virus be Destroyed?

By PETER RAZZELL*

There has been much recent debate about the destruction of the last remaining stocks of smallpox virus preserved in specialist laboratories in Atlanta and Moscow.[1] This debate has been carried out exclusively by virologists and microbiologists, the specialists who clearly are most qualified to decide the fate of one of the world's most deadly viruses.

However, one voice not yet heard in the debate is that of the medical historian. Familiarity with the history of smallpox, cowpox and vaccinia raises a number of questions and worries, and in this paper, I want to briefly consider some of the issues raised by the medical historical evidence.

One potential problem that has already been raised is the survival of smallpox virus in corpses buried in the permafrost,[2] and the possibility that a new epidemic may be triggered by the accidental disturbance of such corpses. That this worry is not purely theoretical is indicated by an event that occurred in an English village in the eighteenth century:

> The following very singular occurrence happened in the year 1759 at Chelwood a village near Pensford (Somerset); the sexton of the place opened up the grave in which a man, who died of the smallpox, had been interred near 30 years before. The coffin was of oak, and so firm, that it might have been taken out whole; but the man forced his spade through the lid, when there issued a most nauseous stench. The person who was to be buried being of eminence, most of the inhabitants of the village attended the funeral: in a few days afterwards, 14 persons were seized with the smallpox in one day; and in three days after, all but two in the whole village, who had not had it, were seized in a like manner. It is remarkable, the disease was so favourable, that no more than two persons died of it.[3]

The ability of smallpox virus to survive underground for long periods of time is also indicated by the practice of one Scottish inoculator at the end of the eighteenth century. The inoculation of smallpox – variolation – had been practiced in Britain since at least 1721, and an amateur inoculator by the name of John Williamson had preserved smallpox virus in the following fashion:

> He ... keeps it a long time before he puts it to use – sometimes seven or eight years; and, in order to lessen its virulence, he first dries it in peat smoke, and then puts it underground, covered with camphor.[4]

This suggests that smallpox virus survived for long periods underground, and along with the above evidence, indicates that the possibility of accidentally

* 30 Ingram Road, East Finchley, London N2 9QA, UK.

[1] *Nature,* 366, (23/30 December I993), 71I.
[2] Peter Lewin, 'Mummified, Frozen Smallpox: is it a Threat?', JAMA, *253,* (7 June 1985), 3095.
[3] Peter Razzell, 'Smallpox Extinction – a Note of Caution', *New Scientist* (I July 1976), 35.
[4] *Ibid.*

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triggering a smallpox epidemic through the disturbance of a corpse dying from the disease is not just a theoretical one.

But there is perhaps a greater problem arising from another source. In order to understand this danger, it is necessary to briefly consider the history of vaccination, and the source of the early vaccines used by Jenner and his supporters. After a few trial experiments with the cowpox discovered in Gloucestershire, Jenner was forced.to resort to cowpox discovered in a dairy in Gray's Inn Road, London. This had been discovered by one of his supporters, Dr William Woodville, Director of the London Smallpox Hospital. Woodville had conducted trials with this strain of cowpox in the hospital, and in the process contaminated it with smallpox virus.

The vaccine that Woodville sent to Jenner 'was taken from the arm of Anne Bumpus, who had had an eruption of three hundred and ten pustules resembling those of smallpox.'[5] It is highly likely that this vaccine was derived from smallpox and not cowpox virus, and was subsequently attenuated through arm to arm passage, using sites of previous inoculations as a source of virus.

As a result of this and other experiences, Jenner came to believe that cowpox was nothing but smallpox, modified by passage through the cow. He had great difficulty in successfully inoculating cowpox into human beings, which is one of the reasons he relied on the 'vaccine' supplied by Woodville. Some historians have suggested that the cowpox that Jenner discovered amongst the Gloucestershire dairymaids was in fact originally derived from smallpox. Variolation was practiced in a large scale in Gloucestershire for three decades or more before Jenner discovered vaccination. (Jenner himself had been variolated as a boy and had practiced variolation as a country surgeon.)[6] It is possible that milkmaids who had *been* variolated, had transmitted smallpox virus to the cows they were milking, by scratching the itches on their arms and infecting the udders of the cows.[7]

The belief that cowpox was derived from smallpox was shared by a number of Jenner's contemporaries. When vaccines became too attenuated by arm-to-arm passage, it was thought necessary to find fresh stocks of cowpox to create new supplies of vaccine. Cows were successfully inoculated with smallpox by Ceely, Badcock, Thiele and Copeman in the nineteenth century, although Copeman believed it was necessary to attenuate the disease first by inoculating monkeys as an intermediary host.[8] Attempts to inoculate cows with smallpox have been less successful in the twentieth century, and even to this day. The exact relationship between smallpox, cowpox and vaccinia is not known.

Even the origins of current strains of vaccinia are unknown. Some current stocks of vaccine may have originated from smallpox virus; for example, the

[5] Peter Razzdl, *Edward Jenner's Cowpox Vaccine: The History of a Medical Myth* (Firle, 1977), 22.

[6] Peter Razzell, *The Conquest of Smallpox* (Firle, 1977), 69.

[7] Peter Razzell, *Essays in English Population History* (London, 1994), 48.

[8] *Ibid.*

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strain of vaccinia preserved at the Lister Institute in London is reputed to have derived from a Prussian soldier with smallpox in 1870.[9]

Virologists have shown that smallpox, cowpox and vaccinia are similar in their basic structure, although they have slight genetic and other variations, making it impossible to determine their exact relationship. The similar structure of the three viruses is what we would expect, on the basis of the known historical relationship between them.

It is probable that some strains of cowpox and vaccinia were derived from smallpox, and therefore it is possible that a slight mutation in either of these two viruses could lead to the re-emergence of smallpox.[10] There is evidence chat cowpox has become more prevalent in Europe since the cessation of vaccination, and that cowpox – or closely related poxviruses – are to be found in monkeys, rodents and cats, and possibly other animal species. There is recent evidence that human beings have been infected by cowpox probably caught from contact with rodents and cats.[11]

The historical and virological evidence suggests great uncertainty about the future of pox viruses, and how they might affect man in the future. Given that some strains of cowpox and vaccinia have been derived from smallpox virus, it would in my opinion be premature to destroy the remaining stocks of smallpox. They may be required in future as a research aid to combat the resurgence of a disease chat has proved to be so fatal to man in the past.

[9] Razzell, *Edward Jenner,* 3.
[10] *Nature,* 366, 711.
[11] J. L. Burton, 'Of Mice and Milkmaids, Cats and Cowpox', *The Lancet*, 343, (8January 1994).

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DISCUSSION POINT

The Origins of Vaccinia Virus—A Brief Rejoinder

By PETER RAZZELL*

Derrick Baxby's discussion of my article on the destruction of smallpox virus[1] raises one major issue that warrants further comment. He writes with the authority of a leading medical microbiologist familiar with the latest research on smallpox, cowpox and vaccinia, and his conclusion about the immunological and molecular relationship between the three viruses is not at issue. As Baxby writes, the three viruses are immunologically related but have stable differences in their DNA and other characteristics.

However, what is at issue is the historical origin of the vaccinia virus, and given the ambiguity of the microbiological evidence, it is a subject that must be largely settled by medical historical evidence. Fortunately there are certain points of agreement which should allow further clarification of the subject.

Discussing the nature of cowpox, Baxby writes: 'Despite its name cowpox virus does not circulate in cows—probably why nineteenth-century vaccinators had difficulty obtaining material.' The historical evidence more than bears out this conclusion: not only did Jenner find it almost impossible to inoculate what he believed to be cowpox (taken from the udders of cows), but so did his contemporaries. It was for this reason that he turned to Woodville for a supply of 'cowpox lymph'.

Woodville claimed to have discovered a source of cowpox in a dairy at Gray's Inn Lane, and it was this strain which reputedly formed the origin of the vaccine supplied to Jenner and other medical practitioners. Baxby argues that this was only one source of vaccine, but it became so important as to acquire the name of the 'world's lymph', used not only in England, but in France, Germany, America, and elsewhere.

But if cowpox is not to be found in cows, what was the source of the vaccine used by Woodville, Jenner, and the others? Baxby writes that cowpox 'is maintained in small wild rodents', but there is no evidence that Jenner or any of his contemporaries obtained cowpox virus from this source, and they unanimously believed that cowpox was a disease of cows. (Jenner experimented in trying to obtain the virus from the grease in horses' hooves, but with ambiguous results.)

If vaccinia was not derived from cowpox taken from cows, what was its origin and source? It is possible that the virus Jenner and Woodville found on cows had originated from milkmaids. Many of these milkmaids had been inoculated with smallpox and it is likely that some of them had scratched the itchy pustules on their

* Peter Razzell, Faculty of Social Science, Open University, Walton Hall, Milton Keynes MK7 6AA, UK.

[1] See Peter Razzell, 'Should Remaining Stocks of Smallpox Virus be Destroyed?', *Social History of Medicine*, 8 (1995), 305–7, and Derrick Baxby 'Should Smallpox Virus be Destroyed? The Relevance of the Origins of Vaccinia Virus', *Social History of Medicine*, 9 (1996), 117–19.

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arms and inadvertently transferred smallpox virus to the cows. Baxby has written that vaccinia is probably a hybrid originating from both cowpox and smallpox,[2] but to sustain this view, he must answer the question, from what source did Jenner, Woodville, and others obtain their cowpox virus?

I agree with Baxby that the chances of vaccinia or cowpox mutating into smallpox are likely to be very slight, but given what I believe to be the origin of vaccinia—that it was derived from smallpox—it is not beyond the bounds of historical possibility. Baxby's tone is one of reassurance, adopting the mantle of impartial scientific authority, but the truth is that no one knows for sure the origin and nature of the vaccines which were used on many millions of people. Perhaps the lesson to arise out of this particular episode in medical history is that scepticism of medical certainty is the true legacy of Jenner's discovery of vaccination.

[2] D. Baxby, *Jenner's Smallpox Vaccine; the Riddle of Vaccine Virus and its Origins* (London, 1981).

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Did smallpox reduce height?

By PETER RAZZELL

Voth and Leunig's recent article in this journal presents detailed evidence for a correlation between smallpox and height, concluding that on average 'smallpox reduced height by at least 1 inch'.[1] The authors discuss the serious and destructive nature of smallpox in the eighteenth and nineteenth centuries, arguing that many young children were permanently stunted as a result of the impact of the disease in early life. Voth and Leunig have rightly pointed out the serious consequences of smallpox and it is probable that there were a number of secondary illnesses—including tuberculosis and bronco-pneumonia—which resulted from the disease.[2] It also led to physical deformity and permanent disfigurement, and in the late nineteenth century up to two-thirds of unvaccinated children attacked by smallpox were left significantly pock-marked.[3]

The authors are therefore undoubtedly correct in highlighting the possible significance of smallpox for average height. However, there are major problems with the quality of the data that they have used in their article, and these are so fundamental that a re-examination of their central conclusions is necessary.

Voth and Leunig have used the computerized dataset prepared by Floud, Wachter, and Gregory in their study of height for the period 1750-1980.[4] Part of this is based on boys recruited into the Marine Society for the period 1770-1873, and Voth and Leunig have used this for their analysis of smallpox and height. The data on height are derived from all boys recruited into the society, whereas those on smallpox are based on a sample of cases.[5]

Voth and Leunig have presented a diagram (their figure 6) summarizing the incidence of smallpox among recruits, plotting the proportion of boys 'who had experienced smallpox' against their 'year of birth', computed from stated age.[6] This diagram shows that just over 40 per cent of boys born in the middle of the eighteenth century had had smallpox, a proportion rising to nearly 100 per cent by 1760, and staying at that level (with one or two minor fluctuations) until about 1820, dropping dramatically to zero by the end of the 1820s, and staying at that level until 1859, the end point of the diagram.

[1] Voth and Leunig, 'Did smallpox reduce height?', p. 542.
[2] See Razzell, *Conquest of smallpox*, pp. 107, 108.
[3] See, e.g., Collins, *St Pancras*.
[4] The dataset is deposited in the ESRC archive, ESRC SN 2134. For the origins of the dataset, see Floud, Wachter, and Gregory, *Height, health and history*.
[5] See Floud, Wachter, and Gregory, *Height, health and history*, p. 133.
[6] Voth and Leunig, 'Did smallpox reduce height?', p. 547.

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The boys recruited into the Marine Society came predominantly from London, the proportions varying, according to Floud, Wachter, and Gregory, between 71.7 per cent and 88.9 per cent.[7] The incidence of smallpox given by Voth and Leunig for the eighteenth and nineteenth centuries thus essentially describes the experiences of boys living in London.

I

There are two major problems with the data as presented by Voth and Leunig. First, smallpox is known to have been endemic in London since at least the sixteenth century, and was probably a disease of childhood from that period onwards.[8] Lettsom, who had treated over 6,000 smallpox cases in London between the years 1773 and 1776,[9] stated that 'most born in London have smallpox before they are seven',[10] and this is consistent with the endemic nature of the disease. We would therefore expect virtually all boys to have experienced smallpox before they were recruited into the Marine Society at the average age of about 14 years—casting doubt on the authors' conclusion that only about half of boys born in the 1750s and recruited in the 1770s had experienced the disease.[11]

Even more unexpected is Voth and Leunig's depiction of a more-or-less constant, nearly 100 per cent, level of smallpox incidence in boys born between 1760 and 1820, followed by a dramatic decline for those born in the 1820s, and culminating in a zero level for the 1829 to 1859 cohorts. This pattern of smallpox incidence is not consistent with what is known about smallpox mortality in London. The authors themselves cite Landers's figures of smallpox deaths as a proportion of total burials, falling from 10.5 per cent in the 1760s to 7.3 per cent in the 1800s, and 3.5 per cent in the 1820s, and this pattern of mortality is confirmed by detailed statistics which I have published elsewhere.[12]

Voth and Leunig explain this significant decline of smallpox mortality by citing Kunitz's argument that the 'growth of population and increasing integration of national economies led to a change in the human crowd diseases, notably measles and smallpox, transforming them into more benign childhood diseases'.[13] This thesis is flawed on a number of counts:

[7] Floud, Wachter, and Gregory, *Height, health and history*, p. 105, n. 9. The original registers often distinguish between parish of origin and current parish, but the majority of boys appear to have both originated and lived in London.

[8] See Razzell, *Conquest of smallpox*, p. 113.

[9] Ibid., p. 106.

[10] Creighton, *Epidemics in Britain*, p. 554.

[11] It is possible that some of the boys born outside London escaped the disease before entering the Marine Society, but an examination of the original registers indicates that most of these boys came from seaports such as Chatham, Portsmouth, and Plymouth, where smallpox was probably endemic and therefore a disease of young children. (See Razzell, *Conquest of smallpox*, p. 114.) In any event, the majority of boys came from London itself, where most children had contracted the disease before the age of seven.

[12] Voth and Leunig, 'Did smallpox reduce height?', p. 557, n. 74. See also Razzell, *Conquest of smallpox*, p. 148.

[13] Voth and Leunig, 'Did smallpox reduce height?', p. 557.

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smallpox was probably always a childhood disease in London; the weight of evidence is that smallpox in childhood was just as fatal as it was in adulthood; and smallpox probably increased sharply in virulence during the eighteenth and nineteenth centuries.

To support their argument that smallpox was less fatal in childhood than in adulthood, the authors quote figures from my work on case fatality by age for Aynho in Northamptonshire,[14] based on a sample of 132 cases. I present two other tables on the same and an adjacent page, one for Berlin for the period 1865-74 and the other for London for 1870-83, the Berlin table covering 6,123 cases, and the London one 2,159.[15] Both these tables show that smallpox fatality was higher for children under the age of 10 than for any other age group, with the London figures indicating that smallpox fatality was particularly high for children under the age of three—a case fatality rate of 66.0 per cent, compared with 43.0 per cent for adults over the age of 40.[16] This higher level of smallpox fatality is consistent with a general pattern of greater vulnerability of infants and young children to infectious diseases.

There is also no evidence for the reduction in the virulence of smallpox during the period under discussion. On the contrary, the average case fatality of smallpox increased steadily throughout the eighteenth and nineteenth centuries, probably peaking at the end of the nineteenth century. When the Royal Society conducted censuses of smallpox epidemics in the 1720s, it found that the average case fatality rate of 32 epidemics in different parts of the country was 16.5 per cent.[17] According to a series of local censuses, this rate climbed steadily to over 40 per cent by the early 1890s,[18] providing evidence for McVail's conclusion that 'natural smallpox gradually became throughout the eighteenth century, and up to the epidemic of 1870-73, a more virulent and fatal disease, its maximum fatality being on a large basis of facts 45 per cent'.[19]

Average case-fatality rates do not, of course, simply reflect levels of virulence. They are also strongly influenced by age and, probably to some extent, by environmental conditions. However, the scale of change in average case fatality—nearly trebling in 150 years—indicates that there was a marked increase in virulence. Literary evidence also supports this conclusion; for example, Lettsom, writing in 1805, stated that 'the malignity [of smallpox] even in London is augmenting. When I practised here, 35 years ago, one in ten was the calculation, but I think one in six is now a fair proportion.'[20] This increase in the virulence of smallpox is consistent with what is known about the nature of the virus: the more virulent the strain the more infectious the disease,[21] and with the develop-

[14] Ibid., p. 556; Razzell, *Conquest of smallpox*, p. 126.
[15] Razzell, *Conquest of smallpox*, pp. 126, 127.
[16] Ibid., p. 127.
[17] Ibid., p. 131.
[18] Ibid., p. 133.
[19] Ibid., p. 127.
[20] Ibid., p. 135.
[21] Ibid., pp. 34, 35.

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ment of world trade, more virulent strains were probably imported from India, China, and elsewhere.

There is therefore a fundamental inconsistency between the pattern of smallpox incidence as presented by Voth and Leunig, and the known facts about changing smallpox mortality and case-fatality rates. The latter two factors indicate that in London smallpox was a universal disease of childhood increasing in virulence, not consistent with a sharp rise in incidence in the 1750s and a sudden disappearance in the 1820s. That pattern is also not consistent with what is known about the impact of inoculation and vaccination on smallpox mortality, but this will be discussed later.

II

In the light of this major contradiction, the original registers of the Marine Society deposited in the National Maritime Museum were re-examined. Only limited details of these registers are provided by Floud, Wachter and Gregory, and Voth and Leunig do not give any additional informtion making exact identification possible. However, they state that the dataset refers to the period 1770-1873, and there are two types of register with information on height and smallpox for this period, the 'Registers of boys entered as servants in the king's navy' and the 'Register of apprentices sent to merchant ships'. The first type starts in 1770 and ends in 1873, whereas the second begins in 1772 and finishes in 1835.[22] From these dates it is likely that the dataset analysed by Voth and Leunig is based on the first series—the royal navy registers—although there is some information in the merchant navy registers which might also have been used.

The royal navy registers give information on height and smallpox between 1770 and 1844, although, as we will see later, the quality of registration deteriorates sharply in the early 1840s. The merchant navy registers ostensibly give information on height and smallpox between 1772 and 1831. However, the pattern of registration is complex, and it is necessary to describe in detail the information recorded and how it changed over time.

The royal navy register starts on 25 September 1770, and initially there is no information recorded on smallpox. Then on 31 October (case number 170) a letter 'P' (= pox) appears in the column headed 'Reads Or Writes', which from 14 December includes the word 'Spox'. This composite heading of 'Reads Or Writes/Spox' is included from this date (14 December 1770) until 27 November 1844, which is the end of register no. 14. For the final register (no. 15), which runs from 1844 to 1873, the composite heading is replaced by the printed heading 'Reads Or Writes', and smallpox is no longer mentioned either in the heading or in the text of the register itself. It should be emphasized that 'no smallpox'

[22] See National Maritime Museum Library, Greenwich, documents MSY/O/1-15 and MSY/Q/1-6.

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is a residual category, in that it is the absence of a marking for smallpox (the letter 'P') that is the basis of the coding for this category.

In the first royal navy register a total of 638 boys are entered between 31 October 1770 and 17 April 1772, of whom 610 are listed as having had smallpox, i.e. have the letter 'P' entered against their name, giving a total of 95.6 per cent. Most of the 28 boys not listed as having had smallpox are to be found right at the beginning of this sequence (25 in the first 100 cases), and it is possible that this was due to poor registration before the system became fully established.

The second register in the royal navy series runs from 15 August 1772 to 2 February 1778, although there are no cases recorded during the year 1774, probably a result of an interruption in the operations of the Marine Society. This second register covers a total of 1,578 boys, of whom 1,414 (89.6 per cent) are listed with the letter 'P' against their names. But again, it is likely that the absence of smallpox was the result of poor registration, since most missing cases occur on blank column pages with no information on reading, writing, or smallpox. This suggests that the registration clerk simply omitted information on these pages, presumably out of negligence.

From 1778 through to 1824 (11 further registers in the royal navy series) virtually all boys (98 per cent and above) are marked as having had smallpox and the small minority of cases without smallpox are frequently on blank column pages. In register 14, which begins in 1824, there are only 18 boys without smallpox in the first 1,351 cases, but then there is a sharp deterioration in the quality of registration, with many blank column pages, and 80 of 341 cases between 29 August 1838 and 28 March 1841 are listed as without smallpox. From 28 August 1841, registration almost completely collapses, with only 3 cases out of a total of 324 listed as having had smallpox. The register at this time mirrors registration in the initial period in 1772: only information on reading and writing is recorded, and the registration of smallpox is abandoned. The final register (no. 15), starting on 27 November 1844, no longer has a column for smallpox, and no further information is given about the disease from that date onwards.

The merchant navy register begins on 3 July 1772, and includes the following two headings: 1. 'When appeared before the Committee—If he has had the Small Pox . . . P—If only supposed to have had it . . . S.' 2. 'When indentured—If has been inoculated by order of the Committee . . . I.' These headings are included in the first five registers in the series running from 1772 to 1831, and then disappear in the sixth and final register, beginning in 1832, although, as we will see, most of these registers in fact contain no information on smallpox.

The first merchant navy register runs from 3 July 1772 to 3 July 1778, and although it has headings for smallpox and inoculation on every page, no information on these topics is recorded in the body of the register. Likewise with the second register in this series, starting on 3 July 1778: the columns headed 'If has had the smallpox' and 'if has been inoculated by order of the committee' are completely blank until 16 November 1780.

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The number of blank cases between the start of the first register in 1772 and the first recorded case of smallpox in November 1780 is 799.

Information is recorded on smallpox in the second merchant navy register running from 16 November 1780 to 21 December 1787, with a total of 404 cases, of which 51 are listed as without smallpox. Nearly all these are on pages with complete blank columns and there is no mention of inoculation in the adjacent column—suggesting that many of those without smallpox are the result of under-registration. From 21 December 1787 there is no further information on smallpox, and although the headings on smallpox and inoculation are retained in the fifth register in the series, ending on 6 June 1831, there are no entries on smallpox in the text of the registers. Finally, the headings on smallpox and inoculation are dropped from the sixth register which starts on 23 February 1832, and there is no further mention of smallpox in the text of the register.

Voth and Leunig place a special emphasis in their analysis of smallpox and height on the two recruitment periods 1770-5 and 1820-40, and this is because these are the only two periods in their data where there are 'those with smallpox, and those without'.[23] In all other periods nearly 100 per cent of boys had had smallpox, and were therefore not suitable for analysis. However, when we compare the figures for smallpox incidence supplied by Voth and Leunig with those revealed by the original registers there is a fundamental inconsistency. Although they do not quote exact figures, from their figure 6 it would appear that approximately 60 per cent of boys recruited in both 1770-5 and 1820-40 had had smallpox. According to the first register in the royal navy series 95.6 per cent of boys recruited in 1770-2 had experienced smallpox—and this is a minimum figure because of the under-registration discussed earlier. The second register in this series covers the period 15 August 1774 to 2 February 1778, largely outside the 1770-5 period, and this indicates a figure of 89.6 per cent, but with an even greater degree of under-registration. The first merchant navy register starts in 1772 and runs through to 1778, but although there is a heading for smallpox, no cases are recorded in that register.

In the second period, 1820-40, there is again a basic contradiction between Voth and Leunig's figures and those revealed by the original registers. The royal navy series indicates a minimum smallpox incidence of 98 per cent up to 1824. From 29 March 1824 to 29 August 1838, there were 1,351 boys registered, of whom 1,333 (98.7 per cent) had experienced smallpox. From 29 August 1838 to 28 March 1841, 261 of 341 boys (76.5 per cent) had had smallpox—near to the estimated 60 per cent figure from Voth and Leunig's figure. However, most of the cases without smallpox are on blank column pages and therefore are almost certainly the result of under-registration.

[23] Voth and Leunig, 'Did smallpox reduce height?', p. 556.

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III

How do we account for the discrepancies between Voth and Leunig's data and information revealed by a re-analysis of the original registers? First, the former material is based on samples and therefore not strictly comparable to data from the complete set of registers, but the discrepancies are so great as not to be consistent with this explanation. A complete explanation will only be possible by comparing the ESRC computerized dataset with the original registers, but the most likely reason for the discrepancy lies in coding procedures. Many of the blank column page entries—probably all of the first 169 cases in the royal navy register for 1770 and possibly the whole of the first merchant navy register for 1772-8—may have been coded as 'no smallpox', simply because there was an absence of positively coded smallpox entries. The great majority of Voth and Leunig's 'no smallpox' cases in the 1770s consist of entries on blank column pages, with an absence of all information on reading, writing, and smallpox. In the later period of the 1840s, most of the 'no smallpox' cases are probably the result of the abandonment of smallpox registration in 1841. This is indicated by the zero level of smallpox incidence in Voth and Leunig's birth period 1830-59—a period when the incidence of the disease was no longer being registered by the Marine Society. Voth and Leunig make no reference to the original registers and it is likely that they worked only with the computerized dataset; this would explain why they were unfamiliar with the registration problems of the original source material.

The question arises as to whether there is any reliable information in the Marine Society registers which could be used for the analysis of smallpox and height. In the royal navy register before 1841, genuine cases of 'no smallpox' can possibly be recognized by their occurrence in individual entries with information on reading or writing. However, of 24,057 cases registered between 25 September 1770 and 31 August 1841, only 29 fell into this category. To analyse this small sample, these 29 cases were matched with ones immediately following which had identical information on reading and writing, but mentioned smallpox. The total height of the 29 boys in both groups was almost exactly equal—132.9 feet (average 54.99 inches) in the 'no smallpox' sample and 132.5 feet (average 54.82 inches) in the smallpox one. As the mean age of the two groups was almost identical, this would tentatively suggest that smallpox had no impact on average height.

There is one remaining major problem yet to be considered. Given the decline of smallpox mortality in London charted by Landers and by Razzell, why did the incidence of smallpox continue at such a high (almost 100 per cent) level until the late 1830s? The probable explanation is one referred to by Voth and Leunig in their footnote 30. In the eighteenth and nineteenth centuries, contemporaries viewed inoculation as a type of smallpox, believing it was just another form of the disease. The Marine Society was interested in the practical question of whether boys were vulnerable to smallpox when it sent them to royal navy or

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merchant navy ships, which is why it asked them whether they had previously had smallpox,[24] either in its natural or inoculated form—both preventatives against future attacks of the disease. It is also presumably for this reason that the society was concerned about inoculating boys who had not previously had smallpox, either naturally or by inoculation. Therefore the category 'has had the smallpox' would include boys who had smallpox both in its natural and inoculated forms. This is indirectly confirmed by the royal navy registers from 1822 onwards: a capital letter 'V' is sometimes marked in the smallpox column, presumably referring to vaccination as a surrogate for inoculation.

Inoculation became popular in London from the middle of the 1770s and after the beginning of the nineteenth century was practised widely, along with vaccination.[25] This explains why mortality from smallpox fell during this period, in spite of a severe increase in virulence. This is a further reason why Voth and Leunig's statistics of smallpox incidence are so implausible: they are not consistent with the history of inoculation and vaccination, both known to be effective in preventing smallpox.

IV

If the arguments of this comment are correct, what are the lessons to be learnt from the misinterpretation of the Marine Society's registers? If the basic problem derives from Voth and Leunig's exclusive reliance on the ESRC computerized dataset, it will provide a salutary lesson for economic history. No amount of sophisticated statistical analysis will supply a substitute for careful study of original sources. Because the new economic history is able to analyse data at a very sophisticated and abstract level, including computer modelling, there is a danger that insufficient attention will be paid to the reliability of the raw material on which such studies are based. The neglect of detailed empirical research on source material will inevitably lead to the problems associated with the study of the Marine Society registers. An example of this is the use of parish registers, which have formed the basis of much complex and sophisticated historical demographic work—registers which have not been properly evaluated through detailed empirical research.[26] Perhaps Voth and Leunig have provided the new economic history with an invaluable lesson—there is no substitute for the scrupulous study of original source material.

The Open University

[24] Hanway, writing an account of the Marine Society in 1770, stated that 'if any have not had the small pox, with their consent they are ordered to be inoculated': Hanway, *Marine Society*, p. 26.

[25] Razzell, *Conquest of smallpox*, pp. 71-3.

[26] See *idem*, *English population history*, pp. 173-216. Parish registers can be evaluated through the empirical method of 'triangulation', involving the comparison of registers with wills, poor law records, local censuses, apprentice indentures, newspaper reports, and other relevant documentary material. This process of nominal record linkage is a much more reliable method of assessing the quality of parish registers than abstract and general statistical analysis of parish register data.

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Social Class, Infant Development And Adult Disease Mortality.

Since at least the beginning of the twentieth century there has been an interest in the link between infant development and adult disease mortality. A number of clinical studies in the 1940s and 1950s indicated a relationship between infant health and mortality from a variety of adult diseases, including coronary heart disease, stroke and stomach cancer. Most of these of these studies were flawed by poor design methodologies, but in 1964 Geoffrey Rose published the results of carefully designed research which indicated that the mortality patterns amongst families of heart disease patients were significantly higher than amongst the families of control patients suffering from other diseases.[1] The most important findings were as follows:

Table 1: Mortality Amongst Parents And Siblings Of Male Heart Disease Patients And Controls[2]

	Heart Disease Patients	*Controls*
No. Of Families	65	65
No. Of Fathers Dead Before Age 45	11	3
No. Of Mothers Dead Before Age 45	8	2
No. Of Siblings Born	319	367
Percentage Dying In First Year	22%	10%
Percentage Dying After One Year	28%	21%

The families of heart disease patients suffered both from higher parental mortality and infant & child mortality, but death in the first year of life was particular high amongst siblings.

Rose's paper had little influence on subsequent research but the link between infant mortality and heart disease was confirmed by an ecological study of Norway carried out by Forsdahl.[3] He found a significant correlation between county infant mortality rates and mortality from arteriosclerotic heart disease in people aged between 40 and 69 years, and put forward the hypothesis that 'great poverty in childhood and adolescence followed by prosperity, is a risk factor for arteriosclerotic heart disease.'[4] Although the emphasis on adult prosperity has not been widely accepted,[5] the link between infant mortality and heart disease has been supported by subsequent research. For example, Williams et. al. established a correlation between infant mortality rates during 1885-1948 and adult ischaemic heart disease mortality for the counties of England & Wales in 1969-73 .[6]

The link between infant mortality and heart disease mortality was partially confirmed by Buck and Simpson in 1982. They found that in the 1917-21 birth cohorts of 17 United States Registration States, infant mortality from was significantly related to arteriosclerotic heart disease at ages 40-44 and 50-54 in both sexes, after controlling for contemporary infant mortality.[7] However, there was no association between infant mortality and heart disease for a later birth cohort 1927-31, which led them to re-analyse their data. They discovered that it was infant mortality due to diarrhoea and enteritis that was mainly responsible for the link between infant mortality and heart disease in the 1917-21 cohort. Diarrhoea and enteritis largely disappeared between 1917-21 and 1927-31, which Buck and Simpson believed was the reason for a lack of correlation between infant mortality and heart disease mortality in this later period.[8] This finding illustrates the importance of historical change in shaping patterns of infant and adult disease mortality.

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[1] Geoffrey Rose, 'Familial patterns in ischaemic heart disease', *British Journal Of Preventative Social Medicine*, 18 (1964), 75-80.

[2] *Ibid*, p.76.

[3] A. Forsdahl, 'Are poor living conditions in childhood and adolescence an important risk factor for arteriosclerotic heart disease?', *British Journal Of Preventative And Social Medicine*, 31 (1977), 91-95.

[4] *Ibid*, p. 91.

[5] George Davey Smith, 'Adult factors contributing to socio-economic differentials in cardiovascular disease', in Dinah Kuh and Yoav Ben-Shlomo (Eds), *A Lifecourse Approach To Chronic Disease Epidemiology* (Oxford 1997), p. 246.

[6] D.R.R. Williams, Sarah J. Roberts, and T.W. Davies, 'Deaths from ischaemic heart disease and infant mortality in England & Wales', *Journal Of Epidemiology And Community Health*, 33 (1979), 199-202.

[7] Carol Buck and Helen Simpson, 'Infant diarrhoea and subsequent mortality from heart disease and cancer', *Journal Of Epidemiology And Community Health*, 36 (1982), 27-30.

[8] *Ibid*, p.27.

In 1986, Barker and Osmond produced evidence confirming the association between infant mortality and ischaemic heart disease in local authority areas in England & Wales.[9] They found a correlation between both neo-natal and post neo-natal mortality in 1921-25 and heart disease mortality in 1968-78, with strong associations between all infant causes of death and heart disease. They also established correlations between infant and other adult disease mortality: bronchitis, stomach cancer, stroke and lung cancer. Barker and colleagues concluded that 'poor nutrition in early life increases susceptibility to the effects of an affluent diet' for the development of heart disease.[10]

Leon and Davey Smith in a recent study confirmed the link between infant mortality in 1921-23 and adult disease mortality in 1991-93 for twenty-seven countries.[11] Controlling for current levels of infant mortality, they found significant correlations between infant mortality and mortality from respiratory tuberculosis, stomach cancer and stroke, but only weak or non-existence correlations between infant mortality and lung cancer and coronary heart disease.[12] This lack of a correlation between infant and coronary heart disease mortality may be due to the historical shift in patterns of infant mortality discussed by Buck and Simpson. Leon and Davey Smith speculate that infections associated with high infant mortality - such as *H pylori* - might be responsible for the higher rates of mortality from respiratory tuberculosis, stomach cancer and stroke.

Bengtsson and Lindstrom have recently produced evidence on the link between infant mortality and a range of adult diseases in a number of parishes in southern Sweden during the period 1760-1894. The authors found no correlation between maternal or infant nutrition and adult disease, but did find a strong association between infant and adult mortality, particularly with respect to airborne infectious diseases.[13] Infectious diseases were more important in the nineteenth than the twentieth century, and most of the work on the link between infant mortality and coronary heart disease has been for the twentieth century. It is probable that the 'epidemiological transition' led to the replacement of infectious diseases by degenerative ones in shaping patterns of mortality during the twentieth century.[14]

Forsdahl, Barker and some other epidemiologists have assumed that infant mortality, birthweight and weight at one year are primarily shaped by infant and maternal nutrition. The basic assumption is that poverty has influenced foetal and infant health and development through inadequate nutrition. Recently for example, Dorling and colleagues have argued that the pattern of poverty mapped by Booth in his classic study of London at the end of the nineteenth century was reflected in subsequent adult mortality from the middle of the twentieth century onwards.[15]

Research at the Open University however on infant mortality has found that there was little or no correlation between poverty and infant mortality in England before the twentieth century. A study of infant mortality in Fulham, one of the areas covered by Booth, found the following pattern of mortality, based on the poverty colour-coding of the streets in which children were born.

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[9]D.J.P. Barker and C. Osmond, 'Infant mortality, childhood nutrition, and ischaemic heart disease in England and Wales', *The Lancet*, (May 10, 1986), 1077-1081.

[10] *Ibid*, p. 1077.

[11] David A. Leon and George Davey Smith, 'Infant mortality, stomach cancer, stroke, and coronary heart disease: Ecological Analysis', *British Medical Journal*, Volume 320 (24 June 2000), 1705-1706.

[12] *Ibid*, p.1705.

[13] Tommy Bengtsson and Martin Lindstrom, 'Childhood misery and disease in later life: the effects on mortality in old age of hazards experienced in early life, Southern Sweden, 1760-1894.' *Population Studies*, 54 (2000), 263-277.

[14] A.S. Osram, 'The epidemiologic transition theory. A preliminary update', *Journal Of Tropical Pediatrics*, 29 (1983), pp. 305-316..

[15] Danny Dorling, Richard Mitchell, Mary Shaw, Scott Orfod and George Davey Smith, 'The ghost of Christmas past: health effects of poverty in London in 1896 and 1991', *British Medical Journal*, Volume 321 (December 2000), pp. 1547-1551. It should be pointed out however, that George Davy Smith and colleagues have emphasised a life-course approach to the development of adult disease, which looks at poverty and social class across a range of age groups.

Table 2: Infant Mortality in Fulham, 1876-1888.[16]

Colour Code of Birthplace Street	*Number of Births*	*Number of Infant Deaths*	*IMR*
Light Blue/ Dark, Blue/ Black	651	100	154
Light Blue/Purple, Purple + Purple/ Pink	865	133	154
Pink + Purple/ Pink/ Red	829	124	150
Pink/Red + Red + Red/ Orange	385	53	138

There was little or no difference in the first three colour-coded streets and a slight reduction in mortality amongst the fourth middle class colour classification. Provisional research also indicates little or no social class/ infant mortality gradient was found during the period 1871-1885 in the registration districts of Fulham, Ipswich, Bungay, Felixstowe, Loughborough and Hollingbourne.[17]

This lack of a social class gradient in infant mortality in the 1870s requires special comment. A clue was provided by Newsholme in a special report on infant mortality at the beginning of the twentieth century: "probably 80 per cent of the mothers of infants in wage-earning classes suckle their infants partially or entirely ... [whereas] the proportion of mothers in the well-to-do classes who are able or willing to continue to give their infants this immenely important start in life is believed, I think rightly to be much smaller. There must be some reasons of great potency, enabling infants of the well-to-do to survive in much higher proportions to the end of the first year of life, notwithstanding the heavy handicap against them."[18]

In a special cohort study of infant mortality in Ipswich, it was found that mortality was much higher amongst the professional classes in the first two months of life than it was amongst labourers' families - 41 per 1000 as against 24 per 1000 births respectively.[19] There was no mortality gradient by the age of four months, suggesting that any disadvantage amongst professional families due to lack of breastfeeding was compensated at a later age by superior nutrition and hygiene.

Early evidence suggests that the social class gradient for infant mortality increased sharply between the 1870s and 1920s. For example, a special study of cohort mortality in Ipswich matching labourers with professionals for the period 1871-1895 revealed the following pattern:

Table 3: Cohort Analysis Of Mortality By Age Four Months In a Matched Sample of Professionals and Labourers, Ipswich 1871-1895. (Mortality Rate Per 1000 At Risk, Numbers At Risk In Brackets)[20]

Period	*Labourers*	*Professionals*
1871-78	50.9 (179)	50.4 (177)
1884-95	100.5 (155)	41.1 (174)

16 The figures are based on births and infant deaths in the years 1876, 1877, 1881, 1882 1887 and 1888. I am grateful to Sue Smith for allowing me to quote these figures from her post-graduate research at the Open University.

[17]These studies were carried out on vaccination birth and infant death registers lodged in local record offices. These registers were copies of the civil registers made for purposes of compulsory vaccination. See Michael Drake and Peter Razzell, *The Decline Of Infant Mortality In England And Wales 1871-1948: A Medical Conundrum* (Interim Report to the Wellcome Trust, available from the Faculty of Social Science, The Open University).

[18] There is some independent evidence to confirm Newsholme's statement that the middle classes breastfed much less frequently than the working classes at the end of the nineteenth and beginning of the twentieth century. See Valerie Fildes, 'Infant feeding practices and infant mortality in England, 1900-1919', *Continuity And Change,* 13 (1998), p. 258; Valerie Fildes,'Breastfeeding in London, 1905-19', *Journal Of Biosocial Science*, 24 (1992),.p. 59.

[19] The numbers of children at risk in the two groups were 236 amongst professional families, and 240 amongst labourers' families.

[20] These figures are based on the analysis of vaccination birth registers deposited in the Ipswich Record Office which are copies of the civil birth registers, with additional information on the dates of vaccination and death before vaccination. Cohort mortality is calculated on the number of births and deaths before vaccination.

These figures indicate that infant mortality doubled amongst labourers between 1871-78 and 1884-95, a period when when early infant mortality was falling slightly amongst professional families. The social class gradient of overall infant mortality further sharpened between 1887 and 1911, as indicated by the following figures for occupational mortality in Ipswich:

Table 4: Infant Mortality Rates By Occupation Group In Ipswich, 1887-1911. (Number Of Births In brackets)[21]

Period	*Labourers*	*Carpenters*	*Clerks &Professionals*
1887-1895	175 (2404)	144 (694)	106 (743))
1896-1905	185 (3366)	148 (722)	105 (882)
1906-1911	144 (2111)	105 (343)	52 (533)

This increasing social class gradient is reflected in the national figures of infant mortality derived retrospectively from the 1911 Fertility Census and subsequent Registrar-General's figures. Haines has published this material by occupational group as follows:

Table 5: Infant Mortality Rates by Occupation England & Wales, 1896-1950.[22]

	Infant Mortality Rate							
	1896	1899	1905	1911	1921	1930-32	1939	1949-50
Father's Occupation								
Professional	101.5	100.6	76.4	55.1	38.4	32.7	26.8	18.4
Farmers	97.3	97.1	80.2	74.2	51.2	46.3	40.5	24.3
Teachers	104.7	106.0	80.0	57.5	41.6	37.1	28.9	16.6
Clerical	124.3	119.1	95.9	81.2	51.2	42.0	35.1	21.8
Miners	178.8	183.7	158.8	160.0	104.7	81.9	65.5	43.4
Farm Workers	115.5	116.9	101.4	95.9	67.6	58.2	47.2	29.9
Building Labourers	177.0	180.9	149.5	138.5	93.0	64.8	58.0	38.1

Infant mortality nearly halved amongst professionals and teachers between 1896 and 1911, whereas the fall in mortality amongst miners, farm workers and building labourers was only of the order of 10 to 20 per cent. The social class gradient sharpened significantly during this period, confirming the trend revealed by the figures for the local communities quoted above. Although infant mortality continued to fall amongst all groups during the first half of the twentieth century, there was still a two to one gradient in 1949-50. Infant mortality fell to a very low level after that date with most deaths occurring in the first month of life for mainly "endogenous" biological reasons, but in the 1970s children born to unskilled parents were four times more likely to die between one month and one year than those born to professional families.[23]

Given the link between infant and adult disease mortality, it is possible that the changes in the social class gradient of infant mortality were reflected in later mortality rates for heart and other adult disease mortality. The social class gradient in male heart disease mortality in England & Wales was as follows:

[21] Mortality rates are calculated be expressing the number of infant deaths in the first year of life as a ratio of the number of births in each occupational group. The raw data was collected from copies of the civil birth and death registers lodged in the Ipswich Record Office.

[22] Michael R. Haines, 'Socio-economic differentials in infant and child mortality during mortality decline England & Wales, 1890-1911', *Population Studies*, 49 (1995), p.313.

23 Power, Manor and Fox, *Health And Class*, p.1.

Table 7: Heart Disease Mortality Amongst Males By Social Class: Standardised Mortality Ratios[24]

Period	*Social Class I*	*Social Class V*
1921	92	105
1931	98	109
1951	123	101
1961	94	116
1971	86	115
1981	69	141

The gradient continued to widen in the 1980s, but heart disease mortality began to fall amongst Social Class V between 1979-83 and 1991-93.[25] Much of the change in the social class gradient in heart disease mortality is probably due to variations in adult life style risk factors, including diet, smoking and physical exercise. However, as the *Health of the Nation* Working Group found, risk factors such as smoking, drinking and diet only explain "around a third of the socio-economic gradient in coronary heart disease mortality and morbidity".[26]

Considering the figures in Table 7, there is also the difficulty that definitions of heart disease mortality have changed over time, and were probably associated with changing medical assumptions about the relationship between social class and heart disease.[27] Given uncertainty about the reliability of the figures, it is impossible to reach firm conclusions about the timing of changes in heart disease and other forms of adult disease mortality. In general terms, it appears that the social class gradient for heart disease mortality increased significantly from about 1951 onwards, fifty years or so after the gradient in infant mortality began to establish itself. Whether this link is a causal one must await further research, and the reasons for the association must at this stage be largely speculative.

The work of Buck and Simpson linking death from diarrhoea in infancy with later mortality from heart disease may provide a clue to explaining the link. The middle classes appear to have become aware at the end of the nineteenth century of the important role of breastfeeding and good personal and domestic hygiene in preventing infant death, particularly from diarrhoea. The fall in infant mortality amongst middle class families was particularly strong with respect to diarrhoea occurring within the first two months of life,[28] and this may be a central key to explaining the association between infant mortality and coronary heart disease. Barker and colleagues have produced evidence to show that "mortality from coronary heart disease has similar but separate trends with neonatal and postneonatal mortality."[29] Reductions in neo-natal mortality among the middle class could therefore be important in explaining subsequent declines in mortality from heart disease in this social group, but only detailed cohort studies of individual families will enable clarification of these complex issues on the relationship between social class, early infant development and subsequent adult disease mortality.

The above data indicates that infant mortality fell initially and most significantly amongst the middle classes, suggesting that the elimination of poverty and increases in per capita consumption of food were not the primary reasons for the reduction of infant mortality. There is some confirmation of this conclusion in McGonigle's classic study of poverty and mortality in Stockton-on-Tees during the late 1920s and early 1930s. This research focused on the effects of re-housing nearly a half of a local slum population, ninety per cent of which were unemployed. These families were relocated to new council housing with modern sanitary facilities, yet overall mortality increased by about fifty per cent amongst this rehoused population, whereas mortality declined slightly amongst the group left in the slum area. McGonigle attributed this paradoxical finding to the growth of poverty and a decline in the consumption of food amongst the rehoused population resulting from a large increase in rent paid for new housing. However, infant mortality fell significantly amongst the rehoused population whereas the reduction was less

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[24] George Davey Smith, 'Adult factors contributing to socio-economic differentials in cardiovascular disease', in Dinah Kuh and Yoav Ben-Shlomo (Eds), *A Lifecourse Approach To Chronic Disease Epidemiology* (Oxford 1997), p. 251.

[25] Frances Drever and Margaret Whitehead (Eds), *Health Inequalities* (HMSO, London 1997), p. 106.

[26] *Ibid*, p.199.

[27] *Ibid*, pp. 248-250.

[28] This appears to have been the case in Ipswich, but has yet to be confirmed in other studies.

[29] D.J.P. Barker, *Mothers, Babies And Disease In Later Life* (BMJ 1994),p. 8. There were probably other links between social class, infant mortality - such as that due to *Heliocabacter pylori* infection - and adult disease mortality as evidenced in the work of Davey Smith, Leon and others, but these links are likely to be highly complex, and involve a number of additional variables.

significant amongst those left in the slum area, suggesting that the fall in infant mortality was not the result of better nutrition but was due to improved domestic hygiene associated with new bathrooms, running hot water and other improved facilities.[30]

There is a range of evidence that housing conditions shape levels of infection and infant mortality. Systematic reviews of evidence in third world countries show that better water and sanitation is associated with decreased diarrheal morbidity, improved nutritional status and lower childhood mortality.[31] A study of appendicitis in Anglesey concluded that the provision of hot water systems and fixed baths were important in reducing the incidence of the disease.[32] In England, poor quality housing has been found to be linked to respiratory illnesses,[33] and damp housing to general ill-health amongst adults.[34] There is also evidence that housing conditions are associated with levels of childhood infection which influences the later development of adult disease. Infection with *Helicobacter pylori* is linked to poor housing conditions, overcrowding, and sharing a bed with parents,[35] and the lack of private indoor tapped water supply in childhood was found to be associated with increased mortality from coronary heart disease in later life.[36] An ecological study of found a correlation between overcrowding levels in 1936 and mortality from stomach cancer in 1968-78, but this finding was not confirmed in a cohort study linking housing conditions to later mortaluity levels.[37] Recent unpublished research by the MRC Environmental Epidemiology Unit at Southampton University on Finnish population and medical registers has found that failure of growth in height and weight between birth and one year, in association with domestic overcrowding, is linked with the long-term development of coronary heart disease.[38]

Barker and colleagues have produced a series of seminal papers establishing an association between foetal/ infant development, as measured by birthweight and weight at one year, and a range of adult diseases.[39] This research was based initially on the Hertfordshire Health Visitors' Register, and the pattern of correlation between birthweight/ weight at one year and a number of these adult diseases has been confirmed by a number of studies for other areas and in other countries.[40] It has been widely assumed that birthweight and weight at one year are the result of maternal and infant nutrition, both before birth and during the first year of life. The role of maternal nutrition in

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[30] G.C.M. M'Gonigle and J. Kirby, *Poverty And Public Health* (London 1936), pp. 112-115.

[31] Susan E. Burger and Steven A. Esrey, 'Water and sanitation: health and nutrition benefits to children', in Per Pinstrup-Anderson, David Pelletier and Harod Alderman (Eds), *Child Growth And Nutrition In Developing Countries* (Cornell University Press, 1995), pp. 156, 157.

[32] D.J.P. Barker, *Mothers, Babies, And Disease In Later Life* (BMJ London 1984), p. 112.

[33] P. McCarthy, D. Byrne, S. Harrison, and J. Keithley, 'Respiratory conditions: effect of housing and other factors', *Journal of Epidemiology and Community Housing*, 39 (1985), pp. 15-19.

[34] Claire N. Packer, Sarah Stewart-Brown, Sarah E. Fowle, 'Damp housing and adult health: results from a lifestyle study in Worcester, England', *Journal of Epidemiology and Community Health*, 48 (1994), pp. 555-559.

[35] David Leon and Yoav Ben-Shlomo, 'Preadult influences on cardiovascular disease and cancer', in Dinah Kuh and Yoav Ben-Shlomo (Eds), *A Lifecourse Approach To Chronic Disease Epidemiology* (Oxford 1997), p. 61.

[36] D.J. Dedman, D. Gunnell, G. Davey Smith, S. Frankel, 'Childhood housing conditions and later mortality in the Boyd Orr cohort' (Unpublished paper).

[37] D.J. P. Barker, D. Coggan, C. Osmond and C. Wickham, 'Poor housing in childhood and high rates of stomach cancer in England and Wales, *British Journal of Cancer*, 61 (1990), pp. 575-578; D. Coggan, D.J.P. Barker, H. Inskip, G. Wield, 'Housing in early life and later mortality', *Journal of Epidemiology and Community Health*, 47 (1993), pp 345-348.

[38] Personal communication from Professor David Barker.

[39] See D.J.P. Barker, *Mothers, Babies And Disease In Later Life* (BMJ 1994); D.J.P. Barker (Ed), Fetal And Infant Origins Of Adult Disease (BMJ London 1992).

[40] David Leon and Yoav Ben-Shlomo, 'Preadult influences on cardiovascular disease and cancer', in Dinah Kuh and Yoav Ben-Shlomo (Eds), *A Lifecourse Approach To Chronic Disease Epidemiology* (Oxford 1997), pp. 54, 55.

influencing birthweight is however a matter of controversy, and it is likely that nutrition is only important in determining birthweight above a certain minimal threshold of malnutrition.[41]

There are reasons to doubt the role of poverty in shaping patterns of birthweight in the Hertfordshire sample used by Barker and his colleagues. They produced the following figures on social class and birthweight/ weight at one year:

Table 8: Birthweight And Weight At One Year In Men According To Social Class At Birth[42]

Social Class At Birth (Based On Father's Occupation)	*Number*	*Birthweight (Pounds)*	*Weight At One Year (Pounds)*
I	96	7.7	22.2
II	372	7.9	22.5
IIINM	279	7.8	22.4
IIIM	920	7.9	22.2
IV	512	7.7	22.1
V	228	7.8	21.7

The was no significant relationship between social class and birthweight, but a slight correlation between social class and weight at one year in this Hertfordshire sample. The information on father's occupation was retrospectively gathered and the number of cases in Social Class I was very small, and it was possible to only trace less than half of the cases selected from the Hertfordshire Health Visitors' Register.[43] More reliable evidence on socio-economic status and birthweight/ weight at one year is available from research on the rateable values of houses in which children were born in the 1920s and 1930s. This is contemporary information and is available on about three-quarters of all cases in the Hertfordshire Health Visitors' Register.

The following tables summarises findings of research on the relationship between the rateable value of houses in which children were born and stillbirths, premaurity, birthweight, weight at one year, infant and child mortality in two Hertfordshire towns, Berhampstead and Hoddesdon, during the 1920s and 1930s.

Table 9: Stillbirths, Prematurity, Birthweight, Weight At One Year, Infant & Child Mortality, By Rateable Value In Hoddesdon, 1915-39 (Number Of Cases In Brackets)

Rateable Value	*Stillbirths (% Births)*	*Premature (% Births)*	*Birth Weight (pounds)*	*Weight 1 Year (pounds)*	*Infant Mortality (per 1000 births)*	*Child Mortality 1-4 Years (per 1000 cases))*
£4-£6	5.0% (120)	10.0% (120)	7.6 (74)	20.7 (79)	96 (114)	80 (50)
£7	1.0% (299)	5.4% (299)	7.8 (170)	21.2 (215)	54 (295)	36 (111)
£8	1.8% (226)	10.2% (226)	7.6 (156)	21.5 (157)	54 (222)	26 (117)
£9	2.9% (209)	6.7% (209)	7.6 (142)	22.0 (161)	45 (202)	16 (127)
£10	1.3% (239)	8.8% (239)	7.5 (162)	21.5 (167)	39 (236)	7 (135)
£11-£14	3.2% (436)	9.0% (436)	7.7 (289)	21.9 (296)	31 (421)	4 (247)
£15+	3.2% (338)	9.8% (338)	7.6 (184)	22.3 (185)	8 (331)	0 (168)

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[41] Ivan J. Perry, 'Fetal growth and development: the role of nutrition and other factors', in Dinah Kuh and Yoav Ben-Shlomo (Eds), *A Lifecourse Approach To Chronic Disease Epidemiology* (Oxford 1997), p.158; D.J.P. Barker, *Mothers, Babies And Disease In Later Life* (BMJ 1994), p. 124.

[42] C. Osmond, D.J.P. Barker, P.D. Winter, C.H.D. Fall, S.J. Simmonds, 'Early growth and death from cardiovascular disease in women', *British Medical Journal* (11th December 1993), Volume 307, pp. 1522.

[43] There is some evidence for a correlation between social class and birthweight In England & Wales, but this is for the period after the Second World War and the scale of the correlation is very modest. See M.E.J. Wadsworth, *The Imprint Of Time* (Oxford 1991), p.25; O.G. Brooks, 'Effects on birthweight of smoking, alcohol, caffeine, socioeconomic factors, and psychosocial stress', *British Medical Journal*, 298 (25 March 1989), pp. 795-801; T. Coms-Orme et.al., 'Predicting birth weight: relative importance of socio-demographic, medical and prenatal care variables', *Social Science Review*, 67 , 4 (December 1993), pp. 617-30.

Table 10: Stillbirths, Prematurity, Birthweight, Weight At One Year, Infant & Child Mortality, By Rateable Value In Berkhampstead, 1923-39 (Number Of Cases In Brackets)

Rateable Value	*Stillbirths (% Births)*	*Premature (% Births)*	*Birth Weight (pounds)*	*Weight 1 Year (pounds)*	*Infant Mortality (per 1000 births)*	*Child Mortality 1-4 Years (per 1000 cases)*
£3-£6	1.7% (174)	10.3% (174)	7.4 (146)	20.1 (141)	51 (156)	12 (136)
£7	1.1% (190)	8.4% (190)	7.5 (157)	20.5 (153)	66 (182)	20 (154)
£8-£9	1.9% (160)	12.5% (160)	7.5 (125)	21.6 (124)	47 (149)	31 (129)
£10-£11	1.9% (157)	12.1% (157)	7.5 (123)	21.5 (123)	48 (145)	8 (125)
£12-£15	3.1% (321)	12.5% (321)	7.6 (262)	21.4 (248)	14 (281)	16 (244)
£16+	0.7% (286)	26.0% (293)	7.4 (153)	21.7 (141)	20 (249)	0 (199)

There were significant correlations between rateable value and the post-natal variables of weight at one year, infant and child mortality in both towns, but no association between rateable value and the pre-natal variables of stillbirth, prematurity and birthweight.[44] Given that rateable value was a reflection of the market value of housing, this suggests that poverty and housing conditions had a very powerful effect on post-natal child development, but little or no impact on pre-natal development.

Rateable value can be seen as a measure of socio-economic status as well as an indication of housing conditions, and it is known that there was a close link between poverty/ wealth and the consumption of food. Michael Nelson has summarized the association between soci0-economic status and the consumption of food after 1900 as follows:

"Lower-income diets continued to be based mainly on bread and potatoes, and compared with higher-income diets contained much less meat and fish, eggs and milk, separated fats and sugar, fruit, and vegatables other than potatoes. They also provided less energy and other nutrients per family. Income rather than physiological requirements was the major determinant of food choice and quantity ... it was not until the Second World War, with the imposition of a rigorous rationing system, changes in the laws on the provision of welfare foods, and a marked reduction in income differentials, that permanet changes in the social class distribution of food took place."[45]

In 1936/37 families in the top five per cent income group spent an average of about 17 shillings per head on food, compared to the 6 shillings per head spent by the bottom fifteen per cent.[46] It was estimated that none of the top five per cent income group were below the BMA food expenditure minimum, compared to 48 per cent in the bottom fifteen per cent income group.[47] Given that rateable value is a measure of socio-economic status, the lack of a correlation between rateable value and birthweight and a strong association between rateable value and weight at one year in Hertfordshire, suggests that nutritional levels had little or no imact on birthweight, but a significant influence on weight at one year.

There were major increases in the per capita consumption of food during the first half of the twentieth century, associated with growing real incomes and improvements in the standard of living. Total expenditure on food per head rose by 47 per cent between 1900 and 1955, and increases occurred in nearly every category of food covered by the food expenditure surveys.[48] If nutrition had different impacts on birthweight and weight at one year, it is hypothesized that there would be no changes in average birthweight during the twentieth century, but significant increases in mean weight at one year due to the growth of per capita consumption of food. The following figures are based on an analysis of changes in birthweight and weight at one year over time in th two Hertfordshire towns,

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[44] Ref to pattern of correlation re data.

[45] Michael Nelson, 'Social class trends in British diet, 1860-1980', in Catherine Geisler and Derek Oddy (Eds), *Food, Diet And Economic Change Past And Present* (Leicester, ?), p.104.

[46] Richard Stone, *The Measurement Of Consumers' Expenditure And Behaviour In The United Kingdom, 1920-38*, Vol. 1 (Cambridge 1954), p.167.

[47] John Charlton and Mike Murphey (Eds), *The Health Of Adult Britain*, Vol. 1 (HMSO London, 1997), p.102.

[48] Richard Stone, *The Measurement Of Consumers' Expenditure And Behaviour In The United Kingdom, 1920-38*, Vol. 2 (Cambridge 1954), p. 130.

Hoddesdon and Berkhampstead, and in a later period average birthweights in the two Health Authority areas in which they are located:

Table 11: Changes In Mean Birthweight And Weight At One Year In Hoddesdon And Berkhampstead, 1911-39, And East & North Hertfordshire And West Hertfordshire, 1986-98. (Number Of Cases In Brackets)[49]

Period	*Hoddesdon*		*Period*	*Berkhampstead*	
	Birthweight (Pounds)	Wt At 1 Year (Pounds)		Birthweight (Pounds)	Wt At 1 Year (Pounds)
1911-19	7.7 (94)	21.2 (245)	1923-28	7.4 (471)	21.1 (447)
1920-29	7.4 (747)	21.5 (723)	1929-34	7.4 (514)	21.3 (496)
1930-39	7.4 (1116)	21.7 (1109)	1935-39	7.3 (365)	21.5 (356)
	East & North Hertfordshire			*West Hertfordshire*	
1986-98	7.4 (64932)		1995-98	7.4 (27167)	

There was no significant change in birthweight between 1911 and 1998, whereas there was a modest increase in weight at one year in both Hoddesdon and Berkhampstead in the 1920s and 1930s.[50] These figures suggest that nutrition did not play a significant role in changing birthweight, whereas it may have been partly responsible, along with improvements in housing conditions, for increasing weight at one year.

The determinants of birthweight are highly complex, and it is possible that parental characteristics play a role in shaping birthweight, both through genetic and other influences. For example, Davey Smith and others found that mothers of heavier babies were taller, had higher body mass index and died from cardiovascular disease a third less frequently than mothers of small babies,[51] suggesting that genetic or congental factors may play a role in shaping patterns of birthweight.[52] It is also possible that demographic factors - such as maternal age and parity - play a large part in determining birthweight.[53]

The evidence from the study of rateable value and birthweight and weight at one year suggests that birthweight was not influenced by socio-economic environmental variables, whereas weight at one year and other post-natal factors were strongly affected by such environmental factors. Birthweight might be a biological marker for genetic and other biological variables which are not directly influenced by the socio-econopmic or nutritional environment, and it is possible that pre-natal factors measured by birthweight affect later adult disease patterns in a different way from post-natal variables shaped by the soci-economic environment.

Infant development in the first year of life has not attracted the same attention as foetal development in epidemiological research, although historically infant growth was recognised as very important outcome of envuiromental poverty. In 1910, Dr John Robertson , Medical Officer of Health for Birminhagm, published an important paper on the effect of female employment and povery on infant development and health. He showed that family income had a strong impact on weight at one year, as demonstarted in the following table:

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[49] The figures for Hoddesdon and Berkhampstead were calculated from the Hertfordshire Health Visitors' Register, copies of which were made available in computerised form by the MRC Environmental Epidemiology Unit at Southampton; the figures for East & North Hertfordshire and West Hertfordshire were made available by the Hertfordshire Health District Authority.

[50] The figures for birthweight and weight at one year are calculated from the Hertfordshire Health Visitors' Register. There is also some evidence that birthweight did not change significantly at a national level during the second half of the twentieth century: the proportion of babies born under 2500 grams was 8.0 per cent in 1955 and only fallen slightly to 7.2 per cent by 1986. See M.E.J. Wadsworth, *The Imprint Of Time* (Oxford 1991), p.26.

[51] George Davey Smith, Carole Hart, Catherine Ferrell, Mark Upton, David Hole, Victor Hawthorne, Graham Watt, "Birthweight of offspring and mortality in the Renfrew and Paisley study: prospective and observational study", *British Medical Journal*, Vol. 315 (8 November 1997), pp. 1189-1193.

[52] See Paul McKeigue, 'Diabetes and insulin action', in Kuh and Ben-Shlomo, *op.cit.*, pp. 92, 93; Yoav Ben-Shlomo and Dinah Kuh, 'Conclusions', in Kuh and Ben-Shlomo, *op.cit.*, p. 300.

[53] See M.L. Nordstrom, 'Effects on birthweight of maternal education, socio-economic status and work related characteristics', *Scandinavian Journal Of Social Medicine*, 24, 1 (March 1996), pp 55-61.

Table 12: Family Income And Average Weight At One Year In Birmingham, 1909.

Total Income	*Number Of Babies Weighed*	*Average Weight At One Year (Pounds)*
Under 10 Shillings Per Week	52	16.8
10 - 20 Shillings	303	17.5
20 - 30 Shillings	300	18.3
Over 30 Shillings	39	18.8

Robertson concluded that "large number of infants start life at a very great disadvantage, because during intra-uterine life or during the first six months their mothers have not been able to nourish them ... what food comes into the house is given to the children or the husband, while they themselves go on from day to day in a state of semi-starvation."[54] Robertson appended to his report six charts which tracked weight in the first year, suggesting that most of the differential weight gain between those living below and above the poverty line occurred after birth, and was the result of both poverty and infection. Robertson implied that it was the lack of good-quality breastmilk along with infection that was responsible for inadequate growth in infancy

Infant development in the first year of life has also perhaps been under-estimated in its effect on later adult disease mortality. In the studies carried out by Barker and colleagues, the correlation between weight at one year and coronary heart disease mortality amongst males was significantly stronger than the correlation between birthweight and coronary heart disease mortality. [55] A special study carried out by Fall and colleagues found no correlation between birthweight and the incidence of coronary heart disease in a Hertfordshire sample of men, but a a strong and linear correlation between weight at one year and heart disease.[56]

The research reviewed in this paper suggests that infant mortality and development in the first year of life is critical for the understanding of coronary heart and other adult diseases. The Hertfordshire Health Visitors' Register provides the basic information for the further exploration of both birthweight and weight at one year, as well as breastfeeding, infection and infant mortality. These variables need to be linked to data on rateable value, father's occupation, housing conditions, overcrowding, and the family links between siblings and parents. The former will allow the exploration the role of socio-economic and other environmental factors, and the latter the genetic links with regard to birthweight and adult disease mortality, enabling greater clarification of the infant origins of coronary heart and other adult diseases.

Peter Razzell

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[54] *Ibid,* p.19.

[55] D.J.P. Barker, *Mothers, Babies, And Disease In later Life* (BMJ London 1994), p.41.

[56] C.H.D. Fall, M. Vijayakumar, D.J.P. barker, C. Osmond, S. Duggleby, 'Weight in infancy and prevalence of coronary heart disease in adult life', *British Medical Journal*, Vol. 310 7th January 1995, p. 18.

Economic History Review, LIV, 1 (2001), pp. 108–109

Did smallpox reduce height? A final comment

By PETER RAZZELL

There are now many areas of agreement between myself and Leunig and Voth over the Marine Society Register evidence on smallpox and height. However, the tone and content of their latest reply is misleading, as it seeks to reassure readers that the overall quality of their data is reliable, and that these data support the argument that smallpox reduced average height by up to 1 inch. What they do not bring out clearly is that by examining the original registers they have been forced to concede that the evidence cited in their article and first reply was almost entirely flawed.[1]

They now acknowledge that 'in November 1844 the Marine Society ceased to register whether boys had the smallpox' and 'that for much of 1770 the Marine Society did not ask whether boys had suffered from smallpox.[2] The number of 'no smallpox' cases resulting from this non-registration of smallpox during 1770-5 and 1844-73 was 2,149 out of a total of 3,177 'no smallpox' cases in the whole sample for 1770-1873, i.e. 67.6 per cent of the total. Data for the two periods 1770-1873 and 1770-5 formed the sole basis of their tables and figures in their articles and first reply,[3] and as the great majority of the 'no smallpox' cases in these two periods are now acknowledged by Leunig and Voth to be spurious, the basis of their original claims for the effect of smallpox on height is clearly invalidated.

They attempt to defend the overall quality of the data by examining them in detail and subjecting them to the 'literacy test', on the assumption that accurate recording of literacy (sharing the same column in the registers as smallpox) is an effective way of evaluating the registration accuracy of smallpox. However, the logic of Leunig and Voth's use of this test is flawed. By applying the test to all 'no smallpox cases' they are analysing both genuine and spurious cases, so that, for example, all the spurious 'no smallpox cases' in 1844-73 (1,980 cases) are included in the analysis. These represent all boys entering the Marine Society in this period, and it is therefore not surprising that the literacy characteristics of this sample are representative of the general population.

Leunig and Voth attempt to deal with the problem of the accuracy of the data by confining their analysis to the period 1777-78, when they

[1] See Voth and Leunig, 'Did smallpox reduce height?'; Leunig and Voth, 'Smallpox did reduce height'.

[2] Leunig and Voth, 'Smallpox really did reduce height: a reply to Razzell', pp. 110-114.

[3] Voth and Leunig, 'Did smallpox reduce height?' pp. 551-3; Leunig and Voth, 'Smallpox did reduce height', pp. 373, 375.

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believe the registration of smallpox was of a high quality. This means that their analysis is based on only 151 'no smallpox cases', a fraction of their original 3,177 'no smallpox' cases. But not only is their sample now very small, there are good reasons to believe that even this rump of 'no smallpox' cases is unreliable. Of the 151 'no smallpox' cases in 1777-8, only 6 (4.0 per cent) had marks in the reading/writing column, significantly below the 35.6 per cent of boys who were listed as illiterate in the whole period 1770-1873. This would indicate that the registration of smallpox was not accurate during the period 1777-8, and it is surprising that Leunig and Voth did not notice this themselves, as their own literacy test indicates an almost complete lack of reliability in the registration of smallpox in the 1777-8 period.

We may conclude from a review of the evidence that smallpox was probably never accurately recorded at any period in the existence of the Marine Society Register. The issue of the impact of smallpox on average height cannot be settled by analysis of the Marine Society dataset, as it is fundamentally flawed in its registration of smallpox. Only a dataset with reliable information on both smallpox and height will settle the question of the relationship between smallpox and height.

The Open University

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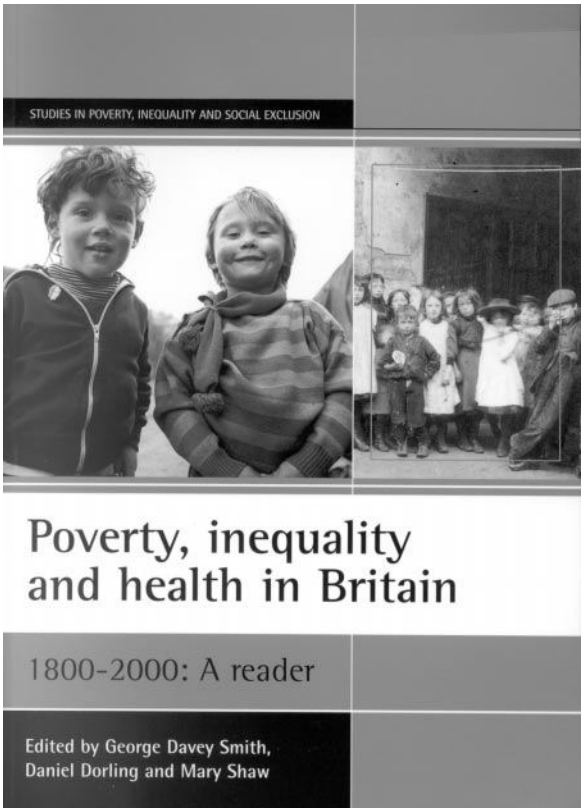
Poverty, Inequality And Health In Britain, 1800–2000: A Reader. *George Davey Smith, Daniel Dorling, Mary Shaw (eds). Bristol: The Policy Press, 2001, pp. 384, £55.00 (HB). ISBN: 1-86134-328-0; £15.99 (PB). ISBN: 1-86134-211-X.*


STUDIES IN POVERTY, INEQUALITY AND SOCIAL EXCLUSION
Poverty, inequality and health in Britain
1800-2000: A reader
Edited by George Davey Smith, Daniel Dorling and Mary Shaw

The editors of this volume are to be congratulated on the quality of the selections from classics texts on poverty, inequality and health in Britain during the nineteenth and twentieth centuries. They have ranged widely both in time and subject matter, including material from Malthus, Farr, Chadwick, Engels, Mayhew, Marx, Rowntree, Booth, Pember Reeves, Greenwood, McGonigle, Boyd-Orr, Beveridge, Titmuss, Morris, Abel-Smith, Townsend, and the recent Black and Acheson Reports.

The book has focussed both on the history of poverty and its effect on health and mortality. The authors quote widely from statistical studies as well as narrative descriptions of poverty from social surveys and other sources. For example, they cite Collis and Greenwood's influential work on the health of the industrial worker, detailing the effects of poverty and overcrowding on tuberculosis mortality during the early twentieth century. The selections on poverty often stand in their own right, and evoke an appropriate sympathy for the poor and their plight in grappling with extreme poverty. Some of the most effective sections of the book on poverty are selections from relatively unknown working class authors, such as Robert Roberts and Robert Tressell.

It is only possible to convey the flavour of this writing by quoting from the text of the book. Tressell worked as a painter and decorator in Hastings at the beginning of the twentieth century and described in his autobiographical novel the following scene:

> 'The woman did not reply at once. She was bending down over the cradle arranging the coverings which the restless movements of the child had disordered. She was crying silently, unnoticed by her husband. For months past—in fact ever since the child was born—she had been existing without sufficient food. If Easton (her husband) was unemployed they had to stint themselves so as to avoid getting further into debt than was absolutely necessary. When he was working they had to go short in order to pay what they owed; but of what there was Easton himself, without knowing it, always had the greater share. If he was at work she would pack into his dinner basket overnight the best there was in the house. When he was out of work she often pretended, as she gave him his meals, that she had hers while he was out. And all the time the baby was draining her life away and work was never done. She felt very weak and weary as she crouched there crying furtively and trying not to let him see.'

Inevitably, such poverty and maternal malnutrition led to poor health, not only for mothers but also for their children, an association which has been emphasized by Barker and others in their work on infant growth and later adult disease. This research necessarily leads to the study of historical conditions, and Davey Smith, Dorling and Shaw are pioneers in bringing the relevance of historical evidence to the attention of epidemiologists and other medical researchers, with their work on Booth's poverty map and its links to twentieth century patterns of adult disease mortality.

However, there are problems with some of the assumptions made by Davey Smith, Dorling and Shaw. At one point they write that 'the association between poverty and ill-health has been apparent across the two centuries with which we are concerned'. This was certainly true of the twentieth century, but there is increasing evidence that it was not true of the nineteenth. Historically, there was no simple relationship between poverty and mortality before the twentieth century. The editors of the present volume have quoted nineteenth century evidence which has long been discredited. For example, they quote Chadwick, Engels and Titmuss on the relationship between social class and expectation of life in the nineteenth century, based on average age at death detailed in various records. Neisson, Farr and others pointed out that this method was fundamentally flawed, as it did not allow for variations in the age structure of populations at risk.

Neisson and other Victorian actuaries concluded from insurance, friendly society and civil registration data that adult mortality was actually higher amongst middle class groups than it was amongst working class populations. For example, they found that mortality amongst clerks and schoolteachers was higher than that amongst manual workers. This difference only disappeared in the twentieth century with the emergence of the classical social class gradient.

Neisson and others believed that the 'inverse' social class adult mortality gradient was due to the healthier lives lived by manual workers, particularly those engaged in active outdoor occupations. However, it is possible that the explanation for higher middle class adult mortality was partly a function of patterns of infectious disease. There is some evidence that the middle classes managed to avoid certain diseases in childhood, and certainly they

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went to great pains to avoid plague, smallpox and other contagious diseases, frequently fleeing from areas where these diseases were rife. As a result, middle class families probably caught some of these diseases—such as smallpox—later in adolescence and adulthood, increasing their levels of adult mortality.

Farr and other writers on nineteenth century mortality were certainly aware of the importance of disease environment in shaping levels of mortality. Davey Smith and colleagues quote Farr to this effect as follows: '(Those living in low-mortality healthy districts) generally follow agricultural pursuits; and they are scattered thinly over the country, often on high ground, so that the impurities which they produce are dispersed and diluted in the air and water. They do not breathe each others' exhalations in theatres and churches. They do not drink water sullied by impurities.'

There is a consensus emerging amongst historical demographers that geographical location was probably more important than social class in influencing mortality in the nineteenth century. Generally, rural areas were much healthier than urban ones, and this only really changed at the end of the nineteenth and beginning of the twentieth century. This was probably linked to the 'epidemiological transition', with infectious diseases being replaced by degenerative ones. The historical evidence is that poverty did not significantly affect infectious disease mortality, but did have a major impact on mortality from degenerative diseases, explaining why it had so much more impact on mortality in the twentieth than in the nineteenth century.

These patterns of historical transition mean that epidemiologists have to be very careful in their use of historical data. For example, Davey Smith *et al.*'s work on the correlation between Booth's poverty map and twentieth century adult mortality assumes that late nineteenth century poverty was associated with poor health, and yet recent research has found a lack of a correlation between the poverty colour-coding of streets and levels of infant mortality in one of Booth's London districts, although there may well be an association with child mortality. This new work is based on copies of civil birth and death registers, many of which have survived and been deposited in county record offices, allowing epidemiological and demographic research for both the nineteenth and twentieth centuries.

The above reservations about the present volume should not however detract from the success that the editors have in demonstrating the relevance of historical evidence to a wider account of epidemiology. Many epidemiologists wish to create a timeless body of generalizations independent of historical variation, but the editors have alerted us to the importance of medical history for a complete understanding of epidemiological reality. The selections contained in the book abundantly and effectively illustrate a wide body of work both on poverty and its effect on health and mortality in the twentieth century.

PETER RAZZELL

Ecological Integrity: Integrating Environment, Conservation and Health. *D Pimentel, L Westra, RF Noss (eds). Washington DC: Island Press, 2000, pp. 428, £55.00 (HB). ISBN: 1-55963-8-079; £27.95 (PB). ISBN: 1-55963-8-087.*

This book brings together and synthesizes the work to date of the Global Integrity Project, which was started in 1992. The aims of the project, as stated on the back cover of the book, have been *'... to examine the combined problems of threatened and unequal human well-being, degradation of the ecosphere, and unsustainable economies'*. The biographies of the contributors to this edited volume highlight that the project has brought together specialists from the fields of ecology and related biological/environmental sciences, economics, philosophy, epidemiology, ethics and law. Between them the contributors have an equally broad experience of academia, industry, governmental and non-governmental organizations. This bodes well for a project and book that aim to take a transdisciplinary approach to the issues concerned.

I would emphasize now that this is not simply a book that describes which and how environmental factors affect human health today. The whole approach of the book is to focus on definition, measurement and effects of 'ecological integrity' and its loss, in the context of which the impacts on human health are considered.

The book has a straightforward structure, similar to that of many edited collections, and is amenable to 'dipping in' to chapters of interest. Indeed it may be quite difficult to plough through the book in its entirety. However, I would recommend against health specialists simply heading straight for the chapters that deal explicitly with human health, without some consideration of the remaining content of the book. The book tries to show that human health not only responds to the state of ecological integrity (at whatever scale), but is also an inherent part of it. Focussing solely on the health section would therefore lead to missing the key point of the book. Having said that, there is probably more detail than is needed on ecological theory and specifics such as forestry for even the broadest-minded epidemiologist, but that does not limit the utility of the book as a whole.

The introductory section does a good job of telling the story of what the book is about, while making the argument for why the following chapters are important and how they fit into the story. This is followed by the four main sections of the book: the history and philosophy behind the ecological integrity concept; the concept as applied to natural resource systems, including agriculture, landscape and fisheries; human and societal health; and economic and ethical aspects. The book ends with a final synthesis, which brings together the ideas and summarizes a prescription for action.

In contradiction to my recommendation above, but with a view to the readership of the *International Journal of Epidemiology*, a brief review of the health-relevant chapters follows. In chapter 14, Professor Tony McMichael sets out to answer the question *'In what ways do global environmental changes affect the prospects for human health?'* The focus on health *prospects* highlights that this is concerned with possible environmental effects on health in a long-term, ecological framework rather than measurement of current exposure effects. The chapter provides a neat summary of the manifold means by which public health is likely to be affected by global and regional environmental changes, which will be familiar to anyone who has read McMichael's book *Planetary Overload*.[1] In common with much of the rest of the book, McMichael argues for the need for transdisciplinary, holistic scientific assessment, since these complex and large-scale issues do not fit reductionist and classical linear analyses. He suggests that to assume that things are getting, and will continue to get, 'better' because life expectancy

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The Conquest Of Smallpox

To the memory of my mother and father,
Margaret Hannah Rosina and Edward Samuel Razzell

THE CONQUEST OF SMALLPOX:

The Impact of Inoculation on Smallpox Mortality in Eighteenth Century Britain

Peter Razzell

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Peter Razzell, *Edward Jenner: the History of a Medical Myth.*

PREFACE TO THE NEW EDITION

In addition to the new introduction written for this edition, I have re-written parts of the main text to reflect the new arguments developed in the introduction. However, the main substance of the original text has been preserved in this second edition.

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INTRODUCTION TO THE NEW EDITION

The Conquest Of Smallpox was originally written as a part of the debate on the origins and causes of population increase in eighteenth and early nineteenth century Britain. It attempted to address some of the issues raised by Thomas McKeown on the relative roles of economic and medical factors in the decline in mortality during this period.[1]

The book was written at a time when it was assumed that mortality played the key role in population growth, reflecting a long tradition of thought initiated by Rickman,[2] and culminating in the writings of Brownlee,[3] Griffiths,[4] Buer[5], George[6] and Chambers.[7] Wrigley and other members of the Cambridge Group have subsequently challenged this view, and on the basis of detailed work carried out during more than three decades have concluded that increases in fertility were more important than reductions in mortality in fostering eighteenth and early nineteenth century population growth.[8]

There is now however increasing evidence that mortality did play the major role in population change, although the debate about the relative roles of fertility and mortality has yet to be fully resolved. The publication of a new edition of *The Conquest Of Smallpox* is therefore an appropriate place in which to discuss

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[1] Thomas McKeown, *The Rise Of Modern Population* (London 1976).
[2] For a discussion of Rickman's work see "Rickman's parish register returns of 1801 and 1841" in E. A. Wrigley and R.S. Schofield, *The Population History Of England, 1541-1871* (London 1981), pp. 598-623.
[3] J. Brownlee, "The history of birth and death rates in England and Wales taken as a whole from 1570 to the present time", *Public Health,* xxix (1915-16), pp. 211-22, 228-38.
[4] G.T. Griffith, *Population Problems Of The Age Of Malthus* (Cambridge 1926).
[5] M.C. Buer, *Health, Wealth And Population* (London 1926)
[6] M.D. George, *London Life In The Eighteenth Century* (London 1930).
[7] J.D. Chambers, "Three essays on the population and economy of the Midlands", in D.V. Glass and D.E.C. Eversley (Eds), *Population In History* (London 1965).
[8] E.A. Wrigley & R.S. Schofield, *The Population History Of England, 1541-1871* (London 1981); E. A. Wrigley, R.D. Davies, J.E. Oeppen and R.S. Schofield, *English Population History From Family Reconstitution 1580-1837* (Cambridge 1997).

recent evidence on the history of fertility and mortality during the eighteenth and early nineteenth centuries. The issues are complex, and it will only be possible here to summarise some of the main features of new demographic research. This discussion of the demographic evidence will provide an appropriate prelude to a review of recent research on the history of smallpox and its impact on mortality.

The Demographic Background

One of the main features of the Cambridge Group's research on the eighteenth century was its aggregative project. This research covered 404 parishes from all parts of England & Wales and their data on changes in baptism and burial rates for England may be summarised as follows:

Table 1: English Baptism And Burial Rates Calculated From Cambridge Group Data[9]

Period	*Estimated Population*	*Baptism Rate*	*Burial Rate*
1701-40	5,350,000 (1721)	29.3/1000	27.7/1000
1741-80	6,147,000 (1761)	29.8/1000	25.5/1000
1781-1820	8,664,000 (1801)	29.4/1000	20.6/1000

According to these figures there was little or no change in the baptism rate between 1701 and 1820, whereas there was a significant fall in the burial rate during the same period, particularly at the end of the eighteenth and beginning of the nineteenth centuries. Wrigley and colleagues believed that the growth of religious non-conformity and other factors led to a decline in the quality of birth registration at the end of the eighteenth century, which led them to inflate the numbers of baptisms during the period 1781-1820. On the basis of this inflation, Wrigley et. al. argued that there was a significant increase in fertility during the late eighteenth century.[10]

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[9] Peter Razzell, "The conundrum of eighteenth-century population growth", *Social History Of Medicine*, Vol. 11, No. 3 (1998), p. 471.
[10] See Wrigley & Schofield, *op.cit.*

I have evaluated the reliability of baptism registration by comparing census with parish register data – tracing individuals listed in the 1851 Census to their stated parish of birth – and examined a total of 45 parishes from different parts of the country. This research suggests little or no change in baptism registration reliability between 1761 and 1834, indicating that between a quarter and a third of all births were under-registered during this period.[11]

There is evidence derived from the work of Gregory King that the age structure of the English population in 1695 was similar to that in 1821 based on national enumeration returns.[12] This evidence along with that in Table 1, suggests that there was no significant long-term change in fertility, and that a reduction in mortality was the major factor in bringing about population growth.

There has however been a great deal of family reconstitution research that provides additional details on the demographic history of England during the eighteenth century. Wrigley and colleagues concluded from research on 26 parishes that there were significant increases in fertility but only modest falls in mortality during the eighteenth and early nineteenth century.[13] They argued that the increases in fertility came about mainly as a result of a fall in the mean age of marriage of women during the latter half of the eighteenth century.

The Cambridge Group's reconstitution research has attracted considerable criticism, focusing mainly on assumptions about the reliability of reconstitution parish registers, as well as concerns about the effect of migration on reconstitution methodology.[14]

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[11] Peter Razzell, *Essays In Population History* (London 1994), pp. 82-149; Peter Razzell, "Evaluating the same name technique as a way of measuring burial register reliability in England", *Local Population Studies,* No. 64, Spring 2000, pp. 8-22.

[12] Glass and Eversley, *op.cit.,* p.215.

[13] Wrigley et.al., *English Population History op.cit.*

[14] See T.H. Hollingsworth, *Historical Demography* (London, 1969), pp. 181-96; Steven Ruggles, "Migration, marriage and mortality: correcting sources of bias in English family reconstitutions", *Population Studies*, 4 (1992); Razzell, "The conundrum", *op.cit.*

The strongest evidence that the Group produced to support its central argument was on mean age at marriage, but even here there are significant problems deriving from the effect of migration on "censoring" calculated ages at marriage. Evidence from marriage licences suggests that mean age at marriage was lower at the end of the seventeenth and beginning of the eighteenth century than that found by the Cambridge Group. The mean age at marriage of 12,382 spinsters marrying in 1661-1714 by licence in five counties – Yorkshire, London, Kent, Nottinghamshire and Suffolk – was 23.6 years, significantly lower than the equivalent figure in the Group's reconstitution sample for 1675-1724 – 26.1 years.[15] It is the high mean age of marriage of spinsters in the earlier period that led Wrigley et al. to conclude that there was a fall in marriage age during the eighteenth century, and this central issue remains yet to be resolved.

Similar problems exist with reconstitution data on mortality in the eighteenth century, particularly with respect to adult mortality. The major difficulty with calculation of adult mortality is in tracking individuals, and it has only been possible to trace about 10 per cent of the population from birth to adult death.[16] There is the additional problem of the reliability of the raw data drawn from parish registers. Other sources of data – such as information on paternal consent in marriage licences and apprenticeship indentures – are probably more reliable ways of measuring adult mortality.[17]

The measurement of infant and child mortality is less difficult because migration in the first five years of life was fairly minimal in most areas, and there are independent ways of measuring child burial registration reliability. It was common in England and elsewhere to give the name of a dead child to a subsequent child of the same sex, allowing measurement of the completeness of burial registration through searching for the first same-name child in the burial register.[18]

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[15] Razzell, "The conundrum", *op.cit.*, p. 485.

[16] *Ibid*, p.495.

[17] For examples of research using these sources see Razzell, *Essays In English Population History*, pp. 194-197.

[18] For a full discussion of the same-name method, see Peter Razzell, "Evaluating the same name technique as a way of measuring burial

Applying same-name correction ratios to reconstitution data for different parishes in various areas in England, yields the following results:

Table 2: Estimated Infant And Child (1-4) Mortality Rates (Per 1000) In St. Bartholomew's London, Truro, Ampthill, Nineteen Cambridge Group Reconstitution Parishes, And Nine Small Rural Parishes, 1650-1837.[19]

Place		*Period*			
		1650-99	1700-49	1750-99	1800-37
St. Barts., London	IMR	264	342	206	–
	CMR	260	274	114	–
Truro, Cornwall	IMR	218	177	145	90
	CMR	231	224	228	103
Ampthill, Bedfordshire	IMR	186	204	131	103
	CMR	121	119	102	103
19 Cambridge Group Parishes	IMR	188	193	163	122
	CMR	105	103	95	74
9 Small Rural Parishes	IMR	134	166	146	89
	CMR	118	89	87	66

Infant and child mortality fell significantly in all parishes covered by Table 2, but the timing and rate of fall varied from area to area. Infant and child mortality fell particularly sharply in St. Bartholomew's, London from the middle of the eighteenth century

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register reliability in England", *Local Population Studies,* No. 64, Spring 2000, pp. 8-22.

[19] The nineteen Cambridge Group reconstitution parishes covered by this table are Terling, Southill, Shepshed, Odiham, Morchard Bishop, Hartland, Great Oakley, Gedling, Earsdon, Dawlish, Colyton, Bridford, Botesford, Banbury, Austrey, Ash, Aldenham, Alcester and Ipplepen. For details on the calculation of the infant and child mortality rates using same name correction ratios, see Razzell, "The conundrum", *op.cit.*, pp. 488, 489. All other rates in this table are based on reconstitution research that I carried out on parish registers lodged in the library of the Society of Genealogists, using same name correction ratios. The nine small rural parishes all had populations less than 500 in 1801, and are as follows: Breamore Hampshire, Kemerton Worcestershire, Weston Colville Cambridgeshire, Cusop Herefordshire, Eaton Hastings Berkshire, Woodchurch Cheshire, Poddington Bedfordshire, Canewden Essex, Stow Maries Essex. For full details see Peter Razzell, *Essays In Historical Demography* (Forthcoming, Caliban Books.)

onwards, a pattern confirmed by other data derived from the London Bills of Mortality and reconstitution data on Quaker families living in London.[20]

Infant mortality fell steadily in Truro throughout the whole of the eighteenth and early nineteenth century, whereas child mortality did not reduce until the beginning of the nineteenth century. In Ampthill, both infant and child mortality fell from the middle of the eighteenth century onwards, although the reduction in child mortality was very modest. In the Cambridge Group's nineteen reconstitution parishes, both infant and child mortality began to fall from the middle of the eighteenth century, although much of the reduction occurred in the early nineteenth century. A similar pattern occurred in the nine small rural parishes, except that there was a fairly sharp fall in child mortality in the first part of the eighteenth century, and a particularly strong reduction in infant mortality during the early nineteenth century.

Table 2 indicates that infant and child mortality fell generally during the eighteenth and early nineteenth centuries, with mortality more than halving from the earliest to the last period in some areas.

There is some evidence that in the early period there was little or no difference in the infant and child mortality between the wealthy and the poor, and that the reduction in mortality first occurred amongst elite social groups.[21] This can be illustrated with data for the town of Truro:

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[20] See John Landers, *Death And The Metropolis: Studies In The Demographic History of London, 1670-1880* (Cambridge, 1993); R.T. Vann and D.E.C. Eversley, *Friends In Life And Death: The British And Irish Quakers In The Demographic Transition* (Cambridge 1992). There is however an inconsistency in the findings of Landers and Vann & Eversley about Quaker infant mortality which has yet to be resolved.

[21] See Razzell, *Essays In Historical Demography.*

Table 3: Socio-Economic Status And Infant/ Child Mortality (Per 1000) In Truro, 1629-1837. (Numbers At Risk In Brackets)

	Elite Families (Merchants & Professionals)			*Non-Elite Families (All Minus Elite Families)*		
Period	*IMR*	*CMR*	*IMR+CMR*	*IMR*	*CMR*	*IMR+ CMR*
1629-1699	271 (435)	237 (244)	508	201 (1183)	237 (732)	438
1700-1749	188 (259)	213 (152)	401	175 (1356)	225 (855)	400
1750-1799	162 (280)	135 (164)	297	142 (1557)	244 (978)	386
1800-1837	66 (190)	25 (100)	61	93 (1241)	116 (607)	209

Infant mortality was higher amongst merchant and professional families than it was amongst the rest of the population in the seventeenth century.[22] Infant and child mortality fell strongly and throughout the whole of the eighteenth and early nineteenth centuries amongst elite families. Although infant mortality fell steadily though less significantly amongst the rest of the population, there was little or no fall in child mortality amongst this group until the beginning of the nineteenth century, when it fell very sharply.

Table 3 indicates that infant and child mortality diminsished more strongly and at an earlier date amongst wealthy families than it did amongst the rest of the population, and provisional evidence for other parishes suggests that this was also the pattern elsewhere.[23]

Changes in adult mortality are less clear, because of the difficulty of measuring this form of mortality. The most reliable evidence is that for socially elite families because of detailed biographical information available.

[22] The higher mortality amongst the wealthy may have been a function of greater exposure to infection through trading and other activities.
[23] *Ibid.*

Table 4: Expectation of Life (Years) for Males Aged 25, 1600-1824.[24]

Period	Social Group			
	Aristocracy	*Members Of Parliament*	*Tontine Nominees*	*Scottish Advocates*
1600-49	25	-	-	29
1650-99	27	26	28	31
1700-49	32	31	35	38
1750-99	36	37	36	38
1800-34	37	38	-	-

Changes in expectation of life at aged 25 were similar amongst the aristocracy and members of parliament, with most increases occurring throughout the whole of the eighteenth century. Adult life expectancy also went up amongst tontine nominees and Scottish advocates, although this mainly occurred in the first half of the eighteenth century. Tontine nominees were a self-selected group, and were largely drawn from the London area,[25] whereas the Scottish advocates lived primarily in urban areas of Scotland.[26] The aristocracy and members of parliament resided in both London and a number of different geographical rural locations, reflecting a wide range of disease environments.

The central thesis of *The Conquest Of Smallpox* was strongly influenced by the finding that expectation of life at birth increased significantly amongst the aristocracy and members of county families during the eighteenth century.[27] The data in Table 4 confirms part of this conclusion, although the findings on infant and child life expectancy are subject to a measure of uncertainty.[28]

There is some other meaningful data available on adult mortality in the eighteenth century. The Vicar-General's marriage licences includes information on paternal mortality, particularly

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[24] Razzell, *Essays op.cit.*, p. 201.

[25] See F. Leeson, *Guide To The British State Tontines* (1964).

[26] Rab Houston, "Mortality in early modern Scotland", *Continuity And Change*, Vol. 7 (1992).

[27] See page 3.

[28] The main source of data for the aristocracy is T. H. Hollingsworth, "The demography of the English Peerage", *Population Studies*, Vol. 18, No. 2 (Supplement, 1964). Because of uncertainty about the reliability of the raw data, Hollingsworth inflated the number of infant deaths by a factor of three before the middle of the eighteenth century.

for people marrying under the age of twenty-one, who were required by law to provide evidence of parental consent, frequently in the form of written affidavits from parents or guardians. I have analysed samples of licences for the period between 1600 and 1849, summarised in the following table.

Table 5: Proportions Of Fathers Of Brides Marrying Under 21 Listed As Dead, Vicar General's Marriage Licences, 1600-1849[29]

Place		*Period*				
		1600-51	1661-99	1700-49	1750-99	1840-49
London	Total	407	1342	1904	1359	276
	Dead	185	634	918	495	86
	% Dead	45.5%	47.3%	48.2%	36.4%	31.2%
Home Counties And The Environs Of London	Total	289	608	596	578	224
	Dead	118	267	253	199	57
	% Dead	40.8%	43.9%	42.4%	34.4%	25.4%
Total Sample	Total	696	1950	2500	1937	500
	Dead	303	901	1171	694	43
	% Dead	43.5%	46.2%	46.8%	35.8%	28.6%

This table indicates that paternal mortality began to fall significantly in London and the Home Counties during the middle of the eighteenth century, a fall that continued through the first half of the nineteenth century.[30] Male adult mortality appears to have fallen by about 40 per cent between the middle of the

[29] The material for the period 1600-41 is taken from George J. Armytage, *Allegations For Marriage Licences Issued By The Bishop Of London, 1520-1610* (Harlaian Society, Vol. 25, London 1887). The data for 1661-1849 is derived from the Vicar-Generals Marriage Allegations lodged in the Society of Genealogists Library.

[30] However, Table 5 does not allow for possible changes in the ages of fathers, a topic to be discussed in Razzell, *Essays In Historical Methodology, op.cit.*

eighteenth and nineteenth centuries, a similar order to the overall reduction in mortality found amongst infants and children.

Recent Research On The History Of Smallpox.

In order to evaluate the demographic consequences of the reduction in smallpox mortality, it is necessary assess its extent, age incidence and varying case-fatality rates. It has sometimes been assumed that smallpox was mainly a disease of childhood in Britain,[31] but all the evidence is that in some areas it affected more adults than children. In the first edition of the present book this was not a topic covered in any detail. Data for the age incidence of smallpox in towns indicated that it was a disease of childhood,[32] but no attempt was made to systematically assess age incidence in the countryside. There was a brief discussion indicating that smallpox did affect many adults in some areas, such as Godalming, in Surrey, but the only detailed data cited was that for Aynho, Northamptonshire, which showed that 43 per cent of cases and 68 per cent of smallpox deaths were of adults.[33]

The age incidence of smallpox is important for three reasons: 1. It is an indication of whether the disease was endemic in a particular area. 2. Case fatality varied very significantly by age. 3. Age incidence had a marked effect on the up-take of inoculation and vaccination.

During the eighteenth century smallpox is known to have been a disease of childhood in Sweden and many other European countries.[34] In Britain it was also a disease of childhood in some areas, particularly in cities and large towns. Monro indicated "the inhabitants of Scotland generally have the smallpox in their infancy or childhood; very few adults being seen here in this

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[31] See for example, S.R. Duncan, Susan Scott and C.J. Duncan, "The dynamics of smallpox epidemics in Britain, 155-1800", *Demography,* Vol. 30, No. 3 (1993), p. 407.

[32] See page 150 for some evidence on this subject.

[33] See pages 153 and 166.

[34] See page 151 and Peter Skold, *The Two Faces Of Smallpox* (Umea 1996), p.105; K.J. Pitkanen, J.H. Mielke and L.B. Jorde, "Smallpox and its eradication in Finland: implications for disease control", *Population Studies,* Vol. 43 (1989), p.99.

disease."[35] Haygarth also implied that smallpox was mainly a disease of childhood in Cheshire and Lancashire, quoting evidence that ninety-five per cent of the militia of these counties had contracted smallpox before their entry into the militia.[36]

Evidence from parish registers suggests that there was a north/south divide in the age incidence of smallpox. The data for 39 parishes reveals the following pattern:

Table 6: Smallpox Deaths Amongst Children And Adults.[37]

Place	*Date*	*Number Of Child Smallpox Deaths*	*Number Of Adult Smallpox Deaths*	*Proportion Of Child Smallpox Deaths*
Northern Parishes				
Penrith, Cumberland	1656-61	60	1	98%
Adel, Yorkshire	1685-1702	16	0	100%
Skipton-In-Craven, Yorkshire	1716-36	110	4	96%
Newton Reigny, Cumberland	1727	9	0	100%

[35] See page 127.

[36] See page 163.

[37] The data for Manchester, Carlisle, Chester, and Kilmarnock is derived from Charles Creighton, *A History Of Epidemics In Britain,* Volume 2 (Cambridge 1894)., pp. 527, 536, 538, 554. The figures for Thorton Lansdale and Newton Reigny are taken from Susan Scott and Christopher J. Duncan, *Human Demography And Disease* (Cambridge 1998), pp. 285, 293. The figures for Whitehaven for 1751-81 are taken from Jean E. Ward, 'Death in eighteenth century Whitehaven: the mortality records from Holy Trinity Church', *Transactions Of The Cumberland & Westmorland Antiquarian & Archaelogical Society,* Volume 98 (1998), pp. 256, 257. The information on smallpox in Birstall, Yorkshire was kindly provided by Michael Drake. All other data is based on the analysis of parish registers in the Society of Genealogists' library. Parishes were selected mainly on the basis of references to smallpox in secondary literature. Where there was specific information on age at death, children were defined as being under twenty-one; otherwise they were categorised as children where they were referred to as "son/ daughter/ child of". The age incidence of cases of smallpox would be different from the figures in this table because of variations in case-fatality by age.

Kilmarnock, Scotland	1728-63	622	0	100%
Ackworth, Yorkshire	1745-1812	84	1	99%
Thorton-in-Lansdale, Yorkshire	1750-56	24	5	83%
Whitehaven, Cumberland	1751-81, 1785-86	664	4	99%
Manchester, Lancashire	1769-74	588	1	99%
Chester, Cheshire	1772-77	369	0	100%
Hickleton, Yorkshire	1776-88	2	0	100%
Braithwell, Yorkshire	1777-1812	17	0	100%
Carlton-Juxta-Snaith, Yorkshire	1777-1812	6	0	100%
Addingham, Yorkshire	1777-1812	41	0	100%
Burhwalis, Yorkshire	1778-1803	6	0	100%
Hindley, Lancashire	1779-1814	160	0	100%
Carlisle, Cumberland	1779-1787	241	0	100%
Heslington, Yorkshire	1782-1804	5	0	100%
Askham Bryan, Yorkshire	1783-1812	6	0	100%
Skipton-In-Craven, Yorkshire	1783-1812	196	2	99%
Birstall, Yorkshire	1784	41	41	100%
South-Western Parishes				
Truro, Cornwall	1767	53	2	96%
Whittington, Shropshire	1774-76	14	0	100%
Southern Parishes				
Basingstoke, Hampshire	1675-1803	147	188	44%
Riseley, Bedfordshire	1690-1742	15	12	56%
Godalming, Surrey	1701-23	78	79	50%
Calne, Wiltshire	1704-58	211	137	61%
Tenterden, Kent	1712-41	10	36	22%
Banbury, Oxfordshire	1718-19	61	41	60%
Breamore, Hampshire	1720-1803	2	10	17%
Aynho, Northamptonshire	1723-24	8	18	31%
Great Shefford, Berkshire	1751-67	2	1	66%
Rayleigh, Essex	1753	7	18	28%
St. Mary's, Southampton, Hampshire	1753-61	22	26	46%

St. Mary's, Bury St.Edmunds, Suffolk	1756-57	93	66	58%
Burford, Oxfordshire	1758	93	78	54%
Cuxham, Oxfordshire	1772	2	6	25%
Horton Kerbie, Kent	1772-1801	0	8	0%
St. Lawrence, Thanet, Kent	1774-89	57	1	98%
Sutton Courtenay, Berkshire	1782-1811	3	6	33%

This Table must be interpreted with caution. The categorisation of regions is somewhat arbitrary and further research is required to clarify the precise geographical distribution of the age incidence of smallpox. Some of the data refers to the late eighteenth century when inoculation was being practised, and this may have reduced the age at which people caught smallpox. Some of the parishes were towns with fairly substantial populations – such as Manchester, Carlisle and Chester – and this would have provided the conditions for endemic childhood disease.[38] However, overall the table suggests that there was a north/south divide, with smallpox being a childhood disease in most northern parishes, and affecting both adults and children in southern ones. The two south-western parishes – Truro and Whittington – appear to have fallen into the northern rather than southern pattern.

There is more precise information on age of death in some parishes. In the southern area, 15 per cent of all smallpox deaths in Tenterden during 1712-42 were under the age of ten,[39] compared to 23 per cent in Aynho, Northamptonshire in 1723/24.[40] Likewise, a reconstitution study of Burford in Oxfordshire indicates that 38 per cent of all smallpox deaths in 1758 were in this under-ten age category.[41] By comparison, the great majority of smallpox deaths were children under ten in the northern parishes – 88 per cent in Adel, 86 per cent in Ackworth,

[38] London which is not covered by the table had the vast majority of its smallpox cases amongst young children.

[39] This figure is derived from the analysis of *Dr. Cliff's Diary* (Kent Archives Office Maidstone, P364/28/4), which lists the causes and ages of death in Tenterden between 1712 and 1742.

[40] For the raw figures for Aynho, see Creighton, *op.cit.*, p.520.

[41] These figures were derived from Joan Moody, *The Great Smallpox Outbreak Of 1758* (Burford 1998).

94 per cent in Braithwell, 83 per cent in Burhwalis, 83 per cent in Carlton-Juxta-Snaith, 98 per cent in Addingham, 95 per cent in Skipton-in-Craven, 100 per cent in Heslington, Manchester, Chester and Carlisle. These high northern figures are similar to the proportion of smallpox deaths under the age of ten in Sweden during 1756-60 – 94 per cent.[42]

All this data suggests that southern England was quite distinctive in its age structure of smallpox. It may have been partly due to the fact that many of these southern parishes were inland, and that England's island position gave it some protection against the importation of infection. However, in the seaport town of Southampton the majority of smallpox deaths appeared to have occurred amongst adults,[43] and many northern inland districts suffered from smallpox as an endemic disease.

Evidence on inoculation also suggests that smallpox was mainly a disease of children in the north of England. For example, 83 per cent of the people inoculated in the Halifax area by Nettleton in 1723 were children under the age of seven.[44] By contrast, the general inoculations that took place in the south of England involved all age groups, as in Brighton "from one day to Near Fourscore Years".[45]

Not only the age incidence, but also the small number of smallpox deaths in some southern parishes suggests that it was possible avoid the disease for very long periods of time.[46] There were just twelve smallpox deaths in Breamore, Hampshire in period of more than eight decades in the eighteenth century, and ten of these were adults. In Horton Kerbie, Kent, there were just eight deaths from smallpox in 1772-1801, and this low mortality was probably not the result of inoculation, for the descriptions of

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[42] The figures for Sweden are from Skold, *op.cit.*, p.166.

[43] It is possible that many of the adult smallpox deaths in Southampton were due to people migrating from the surrounding countryside, and this issue can only be settled by a reconstitution study of one of the parishes in the town.

[44] See page 175. For other evidence of inoculation of children in the north see pages 98-102.

[45] Page 122. See pages 111-122 for a discussion of general inoculations and the age groups involved.

[46] Haygarth pointed to the small number of smallpox deaths in some southern rural parishes: in three Kent parishes there were only 10 smallpox deaths in the twenty-year period 1762-82. See page 195.

people dying from the disease were as follows: "a young woman", "married", "aged 61", "aged 54", "wife", "aged 61", "wife", and "aged 55".

I have described in the present book how people went to extreme lengths to avoid smallpox in the south of England.[47] A further example is provided by an advertisement placed in the *Chelmsford Chronicle* in 1766:

"A lad between thirteen and fourteen years of age, to be a postillion or an assistant under an older servant. He has not had the smallpox, so would rather chuse a place detached from any town."[48]

Likewise, when Joseph King of Colne Engaine, Essex was called for jury service in 1779, he wrote:

"I am warn'd to appear this day at the Sessions to be one of the Petty Jury, and I should have readily attended but am inform'd that the Small Pox is very much about Chelmsford and its neighbourhood and neither my Selfe Wife nor any of my children have had it, it strikes such a Dread and Horror upon me that I dare not venture to attend so I humbly beg of your Worship for this time to excuse me . . ."[49]

This fear of smallpox can be contrasted with the attitude of the general population in the north of England. Writing of Chester, Haygarth noted that "the lower class of people have no fear of the casual [natural] smallpox. Many more examples occurred of their wishes and endeavour to catch the infection, than to avoid it."[50] Monro observed of Scotland in 1765 that "in the villages the peasants are generally assistant to their neighbours of whose family any is sick ... and [do not] fly from the place where it [smallpox] is."[51]

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[47] See page 151 and the various references to the avoidance of market towns when smallpox was present.
[48] J.R. Smith, *The Speckled Monster* (Essex Record Office, Chelmsford 1987).
[49] *Ibid,* p. 24.
[50] See page 72.
[51] See page 127.

It is possible therefore that variations in the age structure of smallpox were due to regional differences in attitude towards the disease. However, the more plausible hypothesis is the reverse: that a fatalistic attitude arose where smallpox was endemic and affected mainly children, whereas in southern rural areas where the disease took an epidemic form and affected children and adults alike, individuals were much more fearful of it.

The question arises as to why smallpox was endemic in northern England, the Scottish mainland and Sweden, characterised generally by dispersed populations of a rural character. In the case of the north of England it was probably partly the result of industrialisation, particularly where industrial villages existed in large numbers and where there were extensive pack-horse routes and regular communication between villages and towns. However, this would be less true of Scotland and Sweden, and perhaps the nearest to an explanation of the endemic nature of smallpox in these countries, has been put forward by Deborah Brunton. Noting that the disease was not endemic in the Scottish islands, Brunton observed:

"The epidemiological pattern of smallpox on the islands was not dissimilar to that found on the English mainland, where discrete, densely populated village communities were periodically visited by the disease. In mainland Scotland, however, smallpox showed a quite difference incidence. Much of the Scottish rural population was scattered thinly over the countryside in small settlements, called 'farm touns' consisting of a few families. As a result, infectious diseases travelled through areas very slowly and were present for long periods. In some parishes, smallpox deaths were recorded in five, or even eight, of ten years, though more typically it was present for around one-third of the time."[52]

This suggests that smallpox was difficult to avoid in these areas, which presumably explains why it was a disease of childhood. In the south of England, the smallpox epidemics tended to strike at distinct periodical intervals and were therefore highly visible, enabling avoidance of the disease.

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[52] Deborah Brunton, "Smallpox inoculation and demographic trends in eighteenth-century Scotland", *Medical History*, 36 (1992), p.409.

Although it may have been possible for many people to escape smallpox altogether in some southern villages, there could be a penalty to be paid by avoiding the disease in childhood. This is illustrated in a smallpox census carried out on August 1772 in the Oxfordshire village of Cuxham. Twenty-nine children were attacked by the disease, of which only two died – 7 per cent – compared to six of twenty adults – 30 per cent.[53] There is not a great deal of evidence on the case-fatality rates of smallpox by age during the eighteenth century, but one of the most detailed surveys was that carried out in Aynho during 1723-24:

Table 7: Age Incidence Of Smallpox Cases And Deaths In Aynho, Northamptonshire, 1723-24.[54]

Age	*Smallpox Cases*	*Smallpox Deaths*	*Case-Fatality*
0-4	13	3	23%
5-9	15	1	7%
10-14	33	3	9%
15-20	14	1	7%
20-24	16	3	19%
25-29	9	3	33%
30-39	12	3	25%
40+	22	9	41%

The evidence suggests that there was a U-Curve distribution of case-fatality, documented in a limited way in the present book.[55] Although based on small numbers, the evidence for Aynho suggests there was a marked difference in the fatality of smallpox depending on age – with a 7 per cent fatality for the 5-9 age group, and 41 per cent for those over the age of 40.

There is similar evidence for this U-Curve distribution from modern times. The following table summarises the data for the unvaccinated population of Madras in 1961-69:

[53] Details of this census are to be found in the Cuxham Marriage Register. What is surprising given the higher fatality amongst adults, is that only 2 adults as against 27 children were inoculated during this epidemic.

[54] Creighton, *op.cit.*, p. 520.

[55] See pages 166-68.

Table 8: Age Specific Case Fatality Rates Of Smallpox In Unvaccinated Persons In Madras, 1961-69.[56]

Age Group (Years)	*Number Of Cases*	*Case Fatality*
0-4	2091	41.7%
5-9	708	22.2%
10-14	154	11.7%
15-19	143	22.4%
20-29	260	39.2%
30-39	91	44.0%
40-44	32	37.0%
45+	55	61.5%

Neither Tables 7 or 8 brings out variations in case-fatality amongst young children under the age of ten. Data from the Whitehaven Dispensary for the period 1783-1804 reveals the following pattern:

Table 9: Age Specific Case Fatality Rates Of Smallpox In The Whitehaven Dispensary, 1783-1804[57]

Age Group (Years)	*Number Of Smallpox Cases*	*Number Of Smallpox Deaths*	*Case Fatality Rate*
0-2	378	139	37%
2-5	665	105	16%
5-10	308	32	10%
10+	36	3	8%

Mortality was highest in the 0-2 age group, and nearly four times as high as that in the 5-10 category. There were no children attacked in Aynho under the age of two, which might explain why the fatality rate in the 0-4 age group in the 1723/24 epidemic was relatively low.

The figures in Tables 7, 8 and 9 reveal the complexity of smallpox mortality, and given the variations in age incidence and age-specific fatality rates, it is difficult to draw definitive conclusions about smallpox mortality in eighteenth century Britain. Some remote rural areas in the south may have largely avoided the disease altogether, whereas others less isolated suffered very heavy mortality; for example Burford in Oxfordshire

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[56] F. Fenner, *Smallpox And Its Eradication* (World Health Organisation, Geneva, 1988), p.54. For other data on the age incidence of smallpox see *Ibid,* pp. 51, 53, 54.

[57] See *Annual Reports Of The Whitehaven Dispensary, 1783-1804.* (Cumbria Record Office, Whitehaven, Ref: YTHOS 2/60).

lost about a sixth of its population to smallpox in 1758.[58] The disease appears to have affected mainly children in the north of England and Scotland, as well as some large towns and cities in England, and fatality would have depended very much on the exact age structure of the disease in these areas.

Age incidence not only affected mortality levels but also the practice of inoculation and vaccination. Deborah Brunton has pointed out that general inoculations were largely confined to the south of England, with little evidence that they took place in the north and in Scotland, other than in remote areas like the Shetland islands.[59] This is because as we have seen endemic smallpox generated a fatalistic resignation, whereas epidemic smallpox which affected large numbers of adults created panic and a resort to mass inoculation and vaccination.

The minimal mortality associated with vaccination undoubtedly helped popularise this new form of inoculation. Many parents feared to impose an immediate hazard on their children where there was a possibility that they might avoid smallpox altogether. The risks of vaccination were sufficiently low to overcome this difficulty. Resistance to vaccination in countries and areas where smallpox was a disease of childhood soon disappeared. This was partly because inoculation had made gradual headway in these places before the introduction of vaccination. By the beginning of the nineteenth century smallpox had also become a very virulent disease, killing large numbers of children in areas where it was endemic, and vaccination became rapidly popular.[60]

The Impact Of Inoculation And Vaccination On Mortality And Fertility.

General inoculations covering all vulnerable members of the population were widely practised in the south of England, a

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[58] Moody, *op.cit.*

[59] Deborah Brunton, *Pox Britannica: Smallpox Inoculation In Great Britain, 1721-1830* (Ph.D. Thesis, University of Pennsylvania, 1990).

[60] See Alex Mercer, *Disease, Mortality and Population in Transition* (Leicester, 1990); D.R. Hopkins, *Princes and Peasants: Smallpox in History* (Chicago 1983).

conclusion confirmed by research published since the original edition of *The Conquest Of Smallpox.*[61] These mass inoculations covered both children and adults, and were practised from the mid-1760s onwards. The impact of these general inoculations would depend on the age incidence of smallpox and the virulence of individual outbreaks of smallpox, as well as any secondary diseases that resulted from smallpox, such as tuberculosis and infantile "convulsions". It is impossible to put a precise figure on this saving of life, but it must have been significant during the last three decades of the eighteenth century.

Outside the south, the decrease in mortality resulting from the practice of inoculation must have been much more modest. This was documented to some extent in the first edition of the present book, presenting evidence that inoculation was only gradually adopted in the north of England and in Scotland, and towards the end of the eighteenth century. Only much more detailed work on individual parishes will allow a full assessment of the impact of inoculation. For example, the proportion of smallpox to all deaths in Hindley, Lancashire was as follows:

Table 10: Smallpox Mortality In Hindley, Lancashire, 1779-1814.[62]

Period	*Number of Smallpox Deaths*	*Total Number Of Deaths*	*Smallpox As A Proportion Of All Deaths*
1779-89	50	277	18.1%
1790-99	59	402	14.7%
1800-09	45	532	8.5%
1810-14	6	251	2.4%

Virtually all smallpox deaths in Hindley were of children, with short-interval epidemics occurring every two years. Table 10 suggests that inoculation made only modest inroads into smallpox mortality before 1799, but significant falls took place after 1800, probably the result of the practice of vaccination and inoculation.

It is possible to trace the long-term impact of inoculation and vaccination on smallpox mortality in one northern urban

[61] See Smith, *The Speckled Monster op.cit.*; Mercer, *Disease Mortality op.cit.*; Brunton, *Pox Britannica op.cit.*

[62] These figures are based on an analysis of the Hindley parish register in the Society of Genealogists' library.

parish, the town of Whitehaven. Between 1751 and 1781 there were a total of 3,138 deaths, of which 597 – nineteen per cent – were due to smallpox, most of whom were of children.[63] In 1776 local surgeons began to offer free inoculation to the poor,[64] and in 1781 the Whitehaven Dispensary began to inoculate local people *gratis*. In the following eighteen years 1,309 children were inoculated, of whom only one died.[65] The case-fatality rate of smallpox in Whitehaven was 19 per cent at this time,[66] and therefore these 1,309 inoculations saved about 250 children, an average of about 14 children per year. Given that on average approximately 20 children died annually from smallpox between 1751 and 1781, this represents a very significant saving of life.

However, according to the dispensary's reports, some of the poor continued to resist inoculation until the very end of the eighteenth century, and it was not until the year 1804 when vaccination became universally accepted, that smallpox began to disappear as a cause of death in the annual reports.[67]

Smallpox mortality declined in Hindley and Whitehaven in a more-or-less linear fashion during the late eighteenth and early nineteenth century, but in other parishes the pattern was more complex and non-linear. For example, the parish register of Ackworth, Yorkshire gives age and cause of death for the period 1745-1812, revealing the following pattern of smallpox mortality:

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[63] See 'A general state of the Whitehaven dispensary for the year 1800', in J. Dixon (Ed), *General State Of The Whitehaven Dispensary For The Year 1795, 1801, 1803, 1804* (Whitehaven, 1795-1804)

[64] Ward, *op.cit.*, p.257.

[65] 'A general state of the Whitehaven dispensary for the year 1800', p.6, in Dixon, *op.cit.*

[66] See Dixon, *op. cit.*

[67] *Ibid.*

Table 11: Smallpox Mortality In Ackworth, Yorkshire, 1745-1812.[68]

Period	*Number of Smallpox Deaths*	*Number Of All Deaths*	*Smallpox Deaths As A Proportion Of The Total*
1745-49	3	75	4.0%
1750-59	3	125	2.4%
1760-69	46	301	15.3%
1770-79	14	168	8.3%
1780-89	15	163	9.2%
1790-99	9	148	6.2%
1800-09	6	175	3.4%
1810-12	0	47	0.0%

Smallpox mortality was very low before 1760, and only increased to more than 15 per cent in the 1760s. Thereafter mortality declined steadily, until it more-or-less disappeared in the early nineteenth century. The low mortality in the late 1740s and 1750s illustrates the variability of smallpox mortality, something that contemporaries were aware of: "it is sometimes so very Mortal, and at other Times so very mild and Favourable" and "they are fatal in one Place, favourable in another and not known in a third."[69] However, Table 11 also indicates an increase in the virulence in smallpox in the 1760s, perhaps a part of a general growth of case-fatality in the eighteenth century, a topic covered in the main text.

Many parish registers include information on smallpox, and only future detailed work on these sources will yield a full understanding on the long-term impact of inoculation and vaccination.[70]

The possible influence of smallpox on fertility is discussed briefly in the present book. Since its first publication, Willibrord Rutten has examined the topic through an analysis of Dutch municipal records. He concluded:

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[68] The table is based on an analysis of the parish register in the Society of Genealogists' library.

[69] See page 174.

[70] However, the problems of registration discussed in chapter 7 must be taken into consideration. A further example of registration problems is illustrated by an entry in the Dedham parish register for 1724: " a great Number of Persons who died in this year when ye Small Pox was very fatal, are omitted." See Smith, *Speckled Monster op.cit.*, p. 192.

"Survivors of smallpox infection apparently had similar marriage, sterility, and fecundity rates to the general population. It is argued that smallpox was of no significance as an aetiological factor in male infertility."[71]

This conclusion is somewhat at variance with the findings of Skold's work on Swedish data. He concluded that both age at marriage and their fertility were influenced by smallpox, largely through women becoming less attractive as marriage partners due to smallpox pitting.[72] There is a lack of detailed data for Britain, but the limited evidence that is available does not indicate a relationship between smallpox and age at marriage.[73]

There has been virtually no work done on the secondary mortality resulting from smallpox. Voth and Leunig claimed that smallpox reduced height – and therefore presumably health – amongst recruits to the Marine Society who had survived attacks of smallpox.[74] But their methodology and quality of data have been strongly criticised, and the issue of how smallpox may have affected height has yet to be resolved.[75]

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[71] Willibrord Rutten, "Smallpox, subfecundity, and sterility: a case study from a nineteenth-century Dutch municipality", *Social History Of Medicine*, Volume 6 (1993), p.85.

[72] Skold, *op.cit.* , pp. 204, 211, 212, 220.

[73] For example, age at marriage in London appears to have risen slightly at the end of the eighteenth century, when smallpox mortality was beginning to fall.

[74] Hans-Joachim Voth and Timothy Leunig, "Did smallpox reduce height?: stature and the standard of living in London, 1770-1873", *Economic History Review*, Volume 49, (1996), pp.541-560.

[75] Markus Heintel and Joerg Baten, "Smallpox and nutritional status in England, 1770-1873: on the difficulties of estimating historical heights", *Eonomic History Review,* Volume 51 (1998), pp. 360-71; Peter Razzell, "Did smallpox reduce height?" *Economic History Review*, Volume 51 (1998), pp. 351-359; Timothy Leuning and Hans-Joachim Voth, "Smallpox did reduce height: a reply to our critics", *Economic History Review*, Volume 51, (1998), pp. 372-81; Peter Razzell, "Did smallpox reduce height?: a final comment", *Economic History Review*, Volume 54, (2001), pp. 108-09; Timothy Leunig and Hans-Joachim Voth, "Smallpox really did reduce height: a reply to Razzell", *Economic History Review,* Volume 54 (2001), pp. 110-14.

Although inoculation and vaccination played a subsidiary part in reducing overall mortality, these prophylactic measures played a major preventative role in protecting the population against the effects of a highly virulent disease. Overall case-fatality amongst young children was of the order of 45 per cent by the 1870s. Smallpox had grown in virulence throughout the eighteenth and nineteenth centuries, and was probably increasing in prevalence with the growth of turnpike roads, canals and railways.[76] By the time civil registration was introduced in 1837, smallpox was largely a disease of young children affecting virtually the whole population.

We can conclude this section by illustrating the fatality of smallpox through quoting one of the Registrar-General's reports for the early 1870s. He illustrated the consequences of neglecting vaccination by comparing mortality in London with that in the Hague:

"It is well known that among the lower classes in Holland a very strong prejudice exists against vaccination. It may be useful to enquire what might be the result in London if the prejudice against vaccination, which is so strongly held by a few in this country, should ever become so widely spread as in Holland. If the same death rate had prevailed in London during the [first] quarter [of 1871] as existed in The Hague during January and February, the deaths from this disease within the Metropolitan Division would have been 38,828 during the three months, instead of the 2400 which actually occurred."[77]

Conclusion

Inoculation and vaccination had a significant impact on smallpox mortality, but the magnitude of that impact cannot be fully assessed without further research. The age incidence and case-fatality of the disease varied so significantly from place to place

[76] The Registrar-General pointed out the importance of foreign and domestic forms of communication in spreading smallpox; see for example, the *Thirty-Fourth Annual Report Of The Registrar-General (1873)*, p. xxxi.
[77] *Ibid.*

that only detailed work on parish registers and other local sources will further clarify the overall magnitude of reductions in smallpox mortality.

However, we can provisionally evaluate the demographic importance of smallpox by comparing the data on mortality in the first section of this introduction with the later evidence on inoculation/ vaccination and smallpox mortality. There were major falls in infant, child and adult mortality in London from the middle of the eighteenth century onwards, but the chronology and age structure of these reductions in mortality do not suggest that inoculation played a primary role in this process. Inoculation was not widely practised in London until the end of the eighteenth century, and smallpox mortality did not begin to fall until the 1770s.[78] Also, given that smallpox was mainly a disease of young children, inoculation probably made little contribution to the fall in adult mortality that took place from about the 1740s onwards.

Much of the fall in infant/ child mortality occurred in rural parishes at the end of the eighteenth and beginning of the nineteenth centuries, and this was the period when inoculation and vaccination were very widely practised. From the age incidence of smallpox, we would expect these prophylactic measures to make the greatest contribution towards reducing child mortality in northern parishes. Inoculation probably contributed to the reduction of both child and adult mortality in the south of England, but only detailed studies of individual parishes examining both the structure of smallpox and overall mortality will answer these questions about the demographic consequences of smallpox.

In one respect smallpox was an epidemiological marker for other diseases. The absence of smallpox amongst children in a southern parish like Breamore perhaps illuminates the structure of mortality more generally. Breamore was an isolated parish in the New Forest, and had one of the lowest infant and child mortality rates in any of the parishes covered by Table 2. By contrast, smallpox was almost exclusively a young child's disease in Truro, which had one of the highest infant and child mortality rates in the sample of parishes covered, which may have been a function of its general epidemiological situation as a trading town near the Cornish coast.

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[78] See page 198.

There is however some evidence that not all diseases were avoided in the way that smallpox was. The mean age of the ten people dying from smallpox in Sutton Courtenay, Berkshire in 1782-1811 was 38 years, compared to the average age of the six measles deaths – 6 years.[79] Likewise, the mean age of the forty-five smallpox deaths in Tenterden, Kent during 1712-41 was 30 years, compared with the average age of 10 years for the fourteen people dying from measles and whooping cough.[80] This suggests that families in these two southern parishes were concerned to avoid smallpox but not the more benign diseases of measles and whooping cough.[81]

The history of inoculation illustrates the increasing importance of empirical medicine in eighteenth century England. The empirical emphasis was not associated with the classical learning of the ancient universities, but with the dissenting academies and the non-conformist doctors who played such an important role in the development of inoculation practice.[82] Much of this empirical emphasis was also linked to market forces, illustrated in the letters of the Glynde bailiff Thomas Davies, discussing the cost and effectiveness of inoculation practices provided by different inoculators.[83]

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[79] The figures are calculated from the Sutton Courtenay parish register in the Society of Genealogists' library.

[80] See Hull, *op.cit.*

[81] It is probable that more serious infections were avoided, particularly by the wealthy who had the means to remove their families when threatened. Jane Austen illustrated this in *Sense And Sensibility*: "the word infection . . . gave instant alarm to Mrs Palmer on her baby's account . . . and confirming Charlotte's fears and caution, urged the necessity of her immediate removal with her infant." Jane Austen, *the Complete Novels* (Oxford 1994), p.186.

[82] See Francis M. Lobo, "John Haygarth, smallpox and religious Dissent in eighteenth-century England", in Andrew Cunningham and Roger French (Eds), *The Medical Enlightenment Of The Eighteenth Century* (Cambridge 1990).

[83] See pages 82 and 84. The importance of market forces in the practice of inoculation is illustrated somewhat humorously by a letter written to the *Chelmsford And Colchester Chronicle* on the 4th March 1768: "All the villages in our neighbourhood [in Northamptonshire] are at present under Inoculation. We have a great variety of practitioners, from the pompous Tye-Wigg down to the greasy night Cap; even boys of seven or

In summary, we may conclude that inoculation and vaccination did not play the major role in diminishing overall mortality in Britain during the eighteenth and early nineteenth century. However, these prophylactic measures did make a highly significant contribution and were a part of a general process of medical innovation and improvement that brought about the fall in mortality. The wealthy and educated classes played a pioneering role in the adoption and practice of both inoculation and vaccination.[84] This was mirrored in a number of other measures adopted first by elite groups and then by the general population – the rebuilding of houses, better personal hygiene, the use of cinchona bark, and a range of environmental and other improvements – which in combination with inoculation and vaccination, were responsible for the reduction of mortality in the eighteenth and early nineteenth centuries.[85]

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eight years perform the operation for a halfpenny a-piece, and succeed surprisingly . . . Giles Wilcox, the sowgelder, who lives near the pinfold, is by far the most in vogue. He takes pupils at 2s 6d a head and teaches 'em the true orthodox method. What the method is I cannot learn, but 'tis said to be preferable to the Suttonian or any other wholesale itinerant operator we have seen yet."

[84] Benjamin Pugh wrote in 1779: "the royal family, nobility, and people of fortune, have their children inoculated at the proper ages; the people in middle life inoculate pretty generally; and the poor (seeing so many instances of the happy success of it) are every where desirous of being inoculated as soon as the natural smallpox begins to range near them." *Gentleman's Magazine*, 20 March 1779, p.52. See also pages 72, 125 of the present volume.

[85] See Razzell, *Essays op.cit.*, pp. 160-171, 220-229.

PREFACE TO THE FIRST EDITION

In a recent edition of *The Lancet,* Professor McKeown's thesis that "medical intervention has made a relatively small contribution to the prevention of sickness and death" has been discussed as a serious part of a policy debate on the allocation of resources between conventional and environmental medicine. Dr Lever, commenting on this claim, has accepted that "improvements in nutrition and hygiene and changes in behaviour can take most of the credit" for decreasing mortality, and that the publicity in favour of environmental medicine coming out of McKeown's work, "is of a type likely to affect Ministers."[1] The present book can be seen in part as an attempt to show that historically, one classical conventional prophylactic measure – inoculation (variolation) and vaccination against smallpox – was significantly effective in reducing overall mortality.

During the first seven years after the introduction of civil registration (1838-44), only about one-and-a-half per cent of children born in England and Wales died of smallpox, in spite of it being a disease of young children – 87 per cent of all deaths occurred under the age of ten – with a case-fatality rate of approximately forty per cent. Given that smallpox is a disease which potentially attacks everyone at all ages (except for a small minority of about five per cent with natural immunity), this represents a very significant saving of life. In the absence of variolation and vaccination, between one in four and one in three more children born would have died than actually did in early Victorian England, and this figure takes no account of the effects of secondary diseases. It is probable that the more serious of these – such as broncho-pneumonia – increased overall mortality from smallpox before it was brought under control.

The main perspective from which the present book was written was a demographic one, and recent research in India has yielded findings of particular significance to this perspective. It has been established that smallpox is one of the major causes of male infertility, through the creation of focal lesions in the epidydimis. There is some historical evidence that smallpox in Britain had an effect on fertility, and that with the disappearance of the disease fertility

increased. Although it is impossible to put a precise figure to the combined effect of decreasing mortality and increasing fertility, it is clear that the gradual elimination of smallpox was of great demographic importance.

Its conquest was achieved through inoculation and vaccination; the former was a product of folk medicine, and the latter on the arguments of the present book, an unintended and accidental attenuation of this inoculation. Smallpox ranks with bubonic plague in its historical importance, and without its gradual elimination, the world's population would have suffered the kind of decimation resulting from the Black Death, and the Industrial Revolution of the late eighteenth and the early nineteenth century would not have been possible.

One of the lessons to emerge from research on the history of smallpox, is that effective medical measures do not always come from highly organized and expensive research programmes, but sometimes arise out of the traditional skills of folk medicine – the wisdom of the ordinary man. (Perhaps more money should be spent on the evaluation of the effectiveness of all forms of folk medicine). The conquest of smallpox was undoubtedly one of the greatest achievement in the history of medicine, and the numerous nameless inoculators and vaccinators practising during the past two hundred and fifty years and more, are the true heroes of this book.

INTRODUCTION TO THE FIRST EDITION

In 1803 a correspondent wrote to the *Gentleman's Magazine* about the late eighteenth century population increase in England:

"One very great cause of increasing population may be ascribed to the success of inoculation for smallpox. One in four or five, or about 200 to 250 in a thousand, usually died of this loathsome disorder in the natural way of infection ... so that this saving of lives alone would account for our increasing number, without perplexing ourselves for any other causes."[2]

Such a sweeping claim invites scepticism, but recent work in historical demography suggests that we should take it more seriously than medical historians have hitherto. T. H. Hollingsworth, in his work on the demography of the British peerage, concluded that there was a significant increase in expectation of life during the middle of the eighteenth century, which is the period when inoculation became widespread amongst the aristocracy. This change may be summarized by the following Table on the mortality experiences of aristocratic women[3]:

Cohort Born	*Expectation of life at birth (years)*
1700-24	36.3
1725-49	36.7
1750-74	45.7
1775-99	49.0
1800-24	51.7

Almost identical results emerged from a study I made of gentry families living in the counties of Northamptonshire and Hertfordshire, but with an even larger increase in life-expectancy: from 35 years in 1720-39 to 48 years in 1740-59.[4] Data from the study of life annuities and tontines for the same period lead to an almost identical conclusion,[5] and it must now be considered to be a firm finding, at least for aristocratic and gentry groups.

Most of the saving of life was concentrated in the younger age groups, and took place during the 1740s and 1750s. Explanations in terms of increases in per capita consumption of food are obviously implausible for groups like the aristocracy and gentry, and given the age structure of the saving of life, we would expect a priori to find the reduction of childhood diseases to be implicated. Against this background, I have re-examined the literature on the history of inoculation against smallpox. No attempt is made to assess the magnitude of the latter's demographic consequences, as this must wait the fruits and results of work based on the new techniques and methods of historical demography.

To guide the reader through the somewhat confusing medical terminology and conflicting views on inoculation (variolation), and its relationship to vaccination, I now summarize the conventional medical position, followed by my own view. Inoculation is the injection of smallpox virus taken from a pustule of a person suffering from smallpox, whereas vaccination is the injection of virus taken originally from a cow suffering from cowpox. The two injections are distinguished by the symptoms and results that they produce: inoculation usually produces pustular eruptions around the body, typical of a mild form of smallpox, and is consequently infectious in that it spreads the disease to an unprotected population. Vaccination produces only a local vesicle at the site of injection, which is not infectious. Both injections protect from future attacks of smallpox by eliciting antibodies, which provide a defensive pool against future attacks of smallpox. Inoculation was first used by the medical profession in England in 1721, but was replaced by vaccination which was introduced by Edward Jenner in 1796. Vaccination, unlike inoculation, is a safe injection, both for the person injected and the unprotected population exposed to him, and this was the reason why inoculation was replaced by vaccination.

I have recently challenged this conventional medical view, arguing that the vaccines used in Jenner's lifetime were in fact derived from smallpox virus, and that early vaccination was a form of smallpox inoculation.[6] This conclusion undermines the polarisation of vaccination and inoculation, with the one being viewed as safe and effective, the other as dangerous and demographically

damaging. It also raises the question as to the actual historical contribution of inoculation in reducing smallpox mortality. The prophylactic power of inoculation to protect against attacks of smallpox has never been questioned, and it is generally agreed that being severer in its effects than vaccination, it produced larger amounts of antibody and a much longer period of protection against future attacks of smallpox – usually for lifetime.

The major argument against inoculation has always been that it spread smallpox to unprotected members of the population, because of its similarity to natural smallpox. We will see however that smallpox virus appears to have been attenuated by the process of inoculation, leading to negligible secondary contagion. Also, from a demographic point of view, it will be argued that secondary contagion was irrelevant on two grounds: (i) the universality of smallpox before the introduction of inoculation, virtually everyone in the population being affected by it; (ii) the danger of secondary infection led to the practice of general inoculation – all vulnerable members of a community being inoculated at one point of time – which resulted in a radical decline in smallpox mortality.

One final point on terminology: because of the development of inoculations for other diseases, the inoculation of smallpox virus is now known by the more specialised term of variolation. In this book, however, the term inoculation will continue to be employed as this was the term used by contemporaries in the eighteenth century, whose writings I will be referring to.

CHAPTER 1
Methods Of Inoculation And Variations In The Severity Of Its Effects

The inoculation of smallpox is probably nearly as old as the disease itself, and reports of its existence appear in the literature as early as the late seventeenth century. It seems to have been a long-standing practice in China, India and parts of Arabia. In all these countries, inoculation was probably perfected over a period of hundreds of years. An English doctor living in India, J. Z. Holwell, described in 1767 a technique of inoculation which is similar to modern methods of vaccination:

"with a small instrument he [the Indian inoculator] wounds, by many slight touches, about the compass of a silver groat, just making the smallest appearance of blood, then . . . applies it [smallpox matter] to the wound . . ."[7]

According to Holwell, the result was that "of the multitudes I have seen inoculated in that country, the number of pustules have seldom been less than fifty, and hardly ever exceeded two hundred", so that not "one in a million . . . miscarries under it."[8]

The inoculation of smallpox was also a part of the traditional folk medicine of Britain, but this only came to notice after Lady Mary Wortley Montagu had her daughter inoculated in London in 1721. Reports of folk inoculation appeared from Scotland and Wales, the latter giving rise to two independent accounts; Dr Perrot Williams described the history of the practice in Pembrokeshire:

"it has been commonly practised by the Inhabitants of this Part of Wales time out of mind, though by another Name, viz that of buying the Disease . . . In order to procure the Distemper to themselves, they either rub the Matter taken from the Pustules when ripe, on several Parts of the Skin of the Arms, etc or prick those Parts with Pins, or the like, first infected with the same Matter."[9]

This was confirmed by a Mr Richard Wright, a surgeon living in Haverford West, who interviewed a number of very old people in the

area, who said it had "been a common Practice with them time out of mind."[10] One woman, a seventy-year midwife, stated that to her personal knowledge it had been practised "above fifty Years", and that she knew "but one dying" after the operation, in spite of "hundreds" having undergone it.[11]

"Buying the smallpox" was perhaps a more vivid way of describing the operation than the gardening metaphor "inoculation" - although the latter's meaning (transplantation), indicates the same contemporary belief that a favourable form of the disease was merely being transferred from one person to another. We will see later that this was a mistaken belief, and that the process of inoculation probably involved an attenuation of the virus.

The first medical account of inoculation to appear in England was that written by Dr Emanueli Timoni, an abstract of which was published in the *Philosophical Transactions* in 1714. Timoni, who practised medicine at Constantinople, claimed that of the "thousands" of people who had been inoculated during the previous eight years, "none have been found to die" of the operation.[12] He admitted however that occasionally symptoms were very severe, and in one year when smallpox was very fatal, four out of fifty cases had inoculated smallpox "near the confluent sort."[13] However, the suddeness of onset of the symptoms led to a suspicion that "these four had caught the common Small-Pox before the Incision was made"[14] – a problem which affected all forms of smallpox prophylaxsis, both inoculation and vaccination, in subsequent experience. Other than this complication, Timoni stated that the pustules resulting from inoculation were "distinct, few and scatter'd; commonly 10 or 20 break out; here and there one has but two or three, few have 100."[15] These very mild results from inoculation were almost certainly achieved through the technique of injection employed. Timoni described this as follows:

"the Operator is to make several little Wounds with a Needle, in one, two or more Places of the Skin, till some Drops of Blood follow, and immediately drop out some Drops of the Matter in the Glass, and mix it well with the Blood issuing out; one Drop of the Matter is sufficient for each Place pricked. These Punctures are made

indifferently in any of the fleshy Parts, but succeed best in the Muscles of the Arm or Radius. The Needle is to be a three edged Surgeon's Needle; It may likewise be perform'd with a Lancet: The Custom is to run the Needle transverse, and rip up the skin a little, that there may be a convenient dividing of the Part, and the mixing of the Matter with the Blood more easily perform'd."[16]

The conclusions reached by Timoni about the safety and method of inoculation in Turkey, were confirmed by a number of independent witnesses, both medical and non-medical. Peter Kennedy, a Scottish surgeon who had practised at Constantinople, stated in a book written in 1715, that he had been informed by physicians and merchants living there, "that of the Number of two thousand, which had then lately undergone that Method [of inoculation], there were not any more than two who died."[17] Similarly, Dr Jacob Pylarini described in an account published in the *Philosophical Transactions* for 1716, how inoculation had been introduced into Turkey in 1701 from Thessaly, and described the method of inoculation as follows:

"the Greek woman . . . pricks the middle of the Forehead, and the Temples at the Root of the Hairs; as also the Chin and both the Cheeks, with a steel or golden Needle, not thrusting it in straight, but obliquely, and separating the Skin a little with a sharp Point from the Flesh below. Then with the same Needle she introduces the Pus into the little Orifices, and ties a Bandage over the Parts . . . The Eruption is almost always of the distinct kind, and the Pustules not numerous; but frequently twenty or thirty, rarely a hundred, and very seldom two hundred."[18]

The injection in the forehead, chin, cheeks etc, was a residue of the Christian belief that incisions made in the pattern of a cross would help to ensure their success, but the actual technique of inoculation appears to have been very sound, with very good results.

It was soon after Pylarini gave his account of inoculation, that Lady Mary Wortley Montagu wrote her famous letter to a friend in

1717, describing her observations of inoculation in Constantinople when she was the wife of the English ambassador:

"The smallpox, so fatal, and so general amongst us, is here [Constantinople] entirely harmless, by the invention of in grafting [inoculation], which is the term they give it. There is a set of old women, who make it their business to perform the operation, every autumn in the month of September, when the great heat is abated . . . the old woman comes with a nutshell full of the matter of the best sort of smallpox, and asks what vein you please to have opened. She immediately opens that you offer her, with a large needle (which gives you no more pain than a common scratch) and puts into the vein, as much matter as can lie upon the head of her needle, and after that binds up the little wound with a hollow bit of shell; and in this manner opens four or five veins . . . The children or young patients play together all the rest of the day, and are in perfect health to the eighth. Then the fever begins to seize them, and they keep their beds two days, very seldom three. They have very rarely above twenty or thirty [pustules] in their faces, which never mark, and in eight days they are as well as before their illness . . . Every year thousands undergo this operation, and the French Ambassador says pleasantly that they take the smallpox here by way of diversion, as they take waters in other countries. There is no example of anyone that has died in it . . ."[19]

Lady Montagu was probably wrong in stating that virus was inoculated into veins – no other account mentions such a practice, and there are grounds for believing it to be unlikely – and she appears to have glossed over some of the complications known to have arisen on occasions with inoculation, such as those mentioned by Timoni. Nevertheless, her overall account is compatible with the numerous other ones of Turkish inoculation, and she herself was probably an eye-witness to most of what she described.

The mildness of inoculation in Turkey was further confirmed by Charles Maitland, who was surgeon to the British Embassy at the time Lord Montagu was ambassador: "The Pustules, whether many or few (and they commonly were from 10 to 100, sometimes more)

never left any Marks or Pits behind them, except only in the Incisions, or Parts Ingrafted."[20] Maitland was present and assisted in the inoculation of Lady Mary Montagu's son at Constantinople in 1717. His account of this second known inoculation of English children – two children of an Embassy official had been previously inoculated – is not only of historical interest, but marks the beginning of a practice that had a serious and long-lasting effect on the history of inoculation in England itself:

"She [Lady Montagu] . . . sent for an old Greek Woman, who had practised this Way a great many Years . . . but so awkwardly by the shaking of her Hand, and put the Child to so much Torture with her blunt and rusty Needle, that I pitied his Cries, who had ever been of such Spirit and Courage, that hardly any Thing of Pain could make him cry before; and therefore Inoculated the other Arm with my own Instrument [lancet], and with so little Pain to him, that he did not in the least complain of it."[20]

From the very beginning of the practice of inoculation by the English medical profession, a lancet rather than a needle was used, and this affected the depth with which incisions were made, at least until Robert Sutton restored the practice of slight incisions in the early 1760s. It was only in this century, that the advantages of a needle over a lancet for vaccination were realised by the medical profession, even though the more astute practitioners of folk medicine had realised this at a very early stage. When Dr. P. Russel described the method of inoculation to a group of Turkish women at the end of the 1750s, his account was commended by an old Bedouin female servant who had herself inoculated a large number of people, except that he "did not seem so well to understand the way of performing the operation, which she asserted should be done not with a lancet, but with a needle."[21] One of the problems was that the medical profession found it difficult to accept that this radically effective prophylactic technique against smallpox had not been discovered by one of their own members, but by people with no pretension to the authoritative conventional medical wisdom of the day.

The defensiveness of the medical profession sometimes resulted in dismissive arrogance, so that one of the earliest opponents of inoculation could say that the practice of a "few ignorant women . . . so far obtains in one of the politest nations in the world, as to be received in the Royal Palace."[22] In fact most of the innovations in the technique of inoculation came from obscure surgeons and what contemporaries sometimes contemptuously referred to as "empirics". This is probably just one example in the history of medicine of how important practical medical innovations have occurred through empirical observation or chance, rather than theoretical understanding. To this day, we do not fully understand the immunology of either inoculation or vaccination, in spite of their enormous historical practical importance.

The success of the practice of inoculation in Turkey was further confirmed in 1722 by Dr James Jurin, secretary to the Royal Society. He brought out the role of a specific inoculator for the history of inoculation in that country:

"out of many thousands that in the space of about forty years past have been inoculated in and about Constantinople by one Greek woman, who still continues that Practice notwithstanding her extreme old age, not so much as one Person has miscarried, as I am assured by the ingenious Dr Le Duc, a Native of Constantinople who was himself inoculated there under the Care of his Father, an eminent Physician in that City."[23]

According. to Porter in 1755, this woman came from Morea, and was succeeded by a woman from Bosnia. Apparently inoculation was only practiced on a very limited scale in Turkey and thus the importance of these individual inoculators.[24]

There were other inoculators in Constantinople, but it appears that at least one of them was very much less successful than the women from Morea and Bosnia. Details of this unsuccessful technique are of some importance for understanding the history of inoculation, and were revealed in an extract from a letter written by Timoni published in 1734:

"at the beginning of the practice of inoculation of the smallpox at Constantinople, there was a person who used to make an incision through the skin, and then introduce into the wound the scab of a dried pustule, tying a bandage over it . . . this mode of operating was objectionable, not only on account of the pain attending it, but also because it sometimes excited the small-pox in its worst form, while at other times it failed to communicate the disease, though even then it produced very bad sores in the places where the incisions had been made. Moreover in some instances this mode of inoculation terminated fatally."[25]

The reasons for the relative failure of this type of inoculation will be discussed later. Here it is sufficient to note that the depth of the injection was probably a factor.

The first inoculation of a known individual in England was of Lady Montagu's daughter in April 1721, which was performed by Maitland in London. This was successful and the patient had less than one hundred pustules as a result of her inoculation.[26] Maitland was very important in the introduction of inoculation into England and he was responsible for the experimental inoculation of the six Newgate prisoners in the autumn of 1721, the success of which helped to persuade the royal family and the aristocracy to have their own children inoculated. Unfortunately, Maitland left no account of his technique but it is almost certain that he made deep incisions with a lancet. He described how typically the incisions he made led to "a vast Discharge" of matter,[27] a symptom known to be associated with deep injections.

This conclusion is confirmed by an eye-witness account of Maitland's inoculations of the Newgate prisoners: "The Incisions were long and large."[28] The result of this method was much severer than that practised by the Greek women in Turkey; several children had 300 pustules and more, while one had "above two Thousand".[29] This was the beginning of a fairly severe form of inoculation, which was used by all practitioners in Britain until the innovation of Robert Sutton in the late 1750s. It seriously checked the practice of inoculation, as people feared that either they or their children would die from the operation.

There were two main reasons why the early British inoculators adopted the deep injection technique: an anxiety about whether inoculation would actually produce a form of smallpox – some contemporaries questioned whether the light Turkish form would achieve this[30] – and the belief that the "poison" of smallpox had to be discharged through an "issue" for a successful outcome. The latter was a part of a long-held humoral theory of smallpox, which assumed that everyone inherited the "seeds" of the disease, which had to be expressed through the eruption of smallpox before true health could be achieved. Some of the early inoculators claimed however, that the deeper injections were also more successful because of the copious discharge at the site of the incision, than were the lighter forms of inoculation. For example, Nettleton who was one of the first to practice inoculation on any scale wrote in 1722:

"I generally found, that in those who discharged most this way, the Fever was more slight, and the Small Pox fewer, tho' I have known some do very well when these places have only appeared very red, but have scarce run any thing at all, as it usually happens, when the Incision is made so superficial as not to cut thro' the Skin."[31]

This conclusion was based however on only forty cases, and the lack of any systematic experimental evidence meant that Nettleton could in effect assert opposite propositions, without feeling a need to further clarify the overall position. Contemporaries were predisposed to accept the conclusion about the benefits of deep incisions because of their theoretical beliefs. Rogers summarized the consensus of opinion on this subject in his book on epidemic diseases published in 1734:

"Tis observed, the more these Incisions discharge, during the Course of the Disease, the more gentle all the Symptoms are; and the longer they continue open, the more perfect Health the Patient enjoys afterwards . . . Part of the morbid Virus, must be supposed to be thrown out from the circulating Juices . . ."[32]

Again this conclusion was based on a very limited number of cases – there were only 897 inoculations performed in the whole of Great

Britain, the American colonies and Hanover in the period 1721-28[33] – and later experience was to show how false it was.

The actual depth of the incisions made in this early period was indicated by a number of authors during the 1720s. Henry Newman described how in New England, "we make usually a Couple of Incisions in the Arms where we make our Issues, but somewhat larger than for them, sometimes in one Arm, and one leg."[34] Boylston confirmed this, and gave advice on how inoculation should be performed:

"Let the Incisions be made with a good Lancet thro' the true Skin [dermis], (by pinching of it up between your Fingers) across the Fibres, and about a quarter of an Inch long, such as would receive a common Pea in case you were to make an Issue . . ."[35]

An even more detailed description of technique was given by Claudius Amyand, the King's surgeon, in a letter written at the beginning of 1724:

"all my operations have been performed with a Lancet on the brawny parts of the two Arms into the Cutis [dermis] and sometimes beyond it. The Incisions have been sometimes an Inch long and sometimes only the length of a Barley-Corn and so superficial as not to penetrate the Cutis, and the Consequences have been much the same."[36]

Unfortunately, Amyand did not realise that the crucial point concerned not only the depth of the injection into the dermis and beyond, but also whether the injection penetrated fully into the dermis itself. It was only later with the innovation in technique made by Robert Sutton, that the importance of not going beyond the boundary between the epidermis and dermis was realised.

Lady Mary Wortley Montagu was aware of the dangers of the departure from the Turkish method of inoculation and complained that "the miserable gashes that they [the English inoculators] give people in the arms may endanger the loss of them."[37] The English medical profession itself realised some of the problems that very deep

injections created, for in 1736 Sir Hans Sloane warned that "great care should be had in making the incision, not to go thro' the skin; for in that case I have seen it attended with very troublesome consequences afterwards."[38] Burges later claimed that the dangers of really deep injections – penetrating through the dermis so as to wound the "cellular membrane" – were discovered by Ranby. He described the symptomatic advantages of the new technique as follows:

"In the infancy of the practice, it was the custom to cut the incision through the skin into the cellular membrane, from a prejudice then generally established, that one of the advantages of inoculation was securing a drain for the humours by the wound, which in that case generally continued its discharge for a considerable time after the distemper was over. But it was found that the incision which was at first only considered as an issue, was too often attended with several very troublesome symptoms, such as an inflammation and swelling of the whole arm, which was reduced with many difficulties, the wound continuing a troublesome sore to the surgeon, and a painful one to the patient a long time . . . Besides, it was no unusual thing at the same time for the person to be seized with other inflammatory disorders, that seemed to point out the cause and seat of the evil."[39]

The advantage claimed for the new lighter method of inoculation was that it led to less soreness at the site of injection, and fewer inflammatory complications. Nowhere in the literature is it stated that the severity of the inoculated smallpox – the number of pustules etc – was affected. Burges gives the somewhat misleading impression that the newer lighter method was universally adopted by all inoculators, but this was not the case. A Dr Henry Barnes who practised at Carlisle, gave the following account of his method of inoculation in 1755:

"I always chuse to make the Incision so deep as to pierce the Skin quite thro' to the Membrana Adiposa, allowing the Incision to

bleed for a few minutes, then the running of the Blood may not wash away the infected matter."[40]

Barnes claimed that his method was highly successful, and that of the "near four hundred" that had been inoculated in the Carlisle area by him, not one had died.[41] This claim was probably correct, but undoubtedly the symptoms resulting from his inoculations were at least as severe as those of Sloane, Ranby and others who adopted the somewhat lighter method. The key change in technique and resulting severity of symptoms occurred with the Suttons, as we shall now see.

Robert Sutton was a surgeon practising in Suffolk and he appears to have started work as an inoculator in about 1757. He first announced his new method of inoculation in the *Ipswich Journal* on the 1st May 1762, claiming that "his new Method of inoculating for the Small-Pox, which he has used for these four Months past, has succeeded so well, that upwards of Two hundred Patients have not had, upon an Average, a hundred Pustules each."[42] Later in the same year, on the 25th September, he gave the following account of his new method:

"Robert Sutton, Surgeon, of Kenton in Suffolk, continues to carry on his new Method of Inoculation with the greatest Success; and being done without Incision, the most curious Eye cannot discern where the Operation is performed for the first forty-eight Hours . . . He has inoculated since December last, three hundred and sixty five . . ."[43]

Although Sutton used a lancet rather than a needle, his technique appears to have been almost identical to that practised in Turkey. Very little further information on Robert Sutton's practice is available in the literature, as it was his son Daniel Sutton, who publicized and was responsible for making the new method widely known. There has been some confusion about the relative roles of father and son in making the innovation of technique, but its clear from contemporary accounts of Daniel Sutton's practice that he was merely following his father's methods. Robert Houlton confirmed this in two separate publications – Daniel Sutton used a "puncture so slight, that it is scarce felt by the patient, and which in a minute

afterwards is scarce visible."[44] – "The operation is performed on most without their feeling or knowing it: and in a minute afterwards, the puncture is scarce visible."[45]

Daniel Sutton himself described his method as follows: "The lancet being charged with the smallest perceivable quantity (and the smaller the better) of unripe, crude or watery matter, immediately introduce it by puncture, obliquely, between the scarf [epidermis] and true skin [dermis], barely sufficient to draw blood."[46]

Although the Sutton family had attempted to keep their method a secret, contemporaries had soon discovered its essence through questioning patients and others who had witnessed the practice. This was made all the easier through the large number of partnerships with outsiders that the Suttons formed. Thomas Dimsdale was perhaps one of the most important inoculators to publish details on the new method. His book, *The Present Method of Inoculating for the Smallpox,* written at the end of 1766, went into several editions and described two variations on the new technique:

"an incision is made in that part of the arm where issues are usually placed, deep enough to pass through the scarf skin [epidermis], and just to touch the skin itself [dermis], and in length as short as possible, not more than one eighth of an inch . . . I have also tried the following method, with the same success as that above described . . . A lancet being moistened with the variolous fluid in the same manner as in the other, is gently introduced in an oblique manner between the scarf and true skin . . ."[47]

The essence of the difference between this new method and that practised by British inoculators since the 1730s, was brought out by William Bromfield, an opponent of the Suttonian Innovation:

"I will not insist on it that matter introduced between the cuticle [epidermis] and cutis [dermis] is not equally capable of producing the disease as where a scratch is made thro' the same integument sufficient to slightly wound the true skin [dermis] . . . [which is the method] that of late years has been practiced . . ."[48]

Given that it became widely accepted that the Suttonian method was the key innovation in technique, it appears that much lighter effects were produced by injecting virus into the epidermis or, at most, the boundary between the epidermis and dermis, rather than fully into the dermis itself. I will discuss possible explanations of this conclusion in a later chapter.

Although contemporaries generally acknowledged the success of the Suttonian method, few realised the critical importance of the depth of the injection. There were even some inoculators who continued to operate in the old way; for example, the Reverend James Woodforde noted in his diary for the 22nd November, 1776, how an amateur inoculator by the name of Drake made "a deep incision in both arms."[49] This was almost certainly untypical at this time, and Woodforde describes what was a more orthodox method in giving an account of the inoculation of two of his servants by Dr Thorne of Mattishall in the same year:

"the Dr. taking a small bit of Cotton Thread saturated with Matter between his Left hand Finger and Thumb with the Launcett in his other hand, he then dipt the Point of the Launcett in a Tea Cup of warm water, then rubbed the Launcett in the Cotton Thread and with the Point of the Launcett made two dotts like this: about two Inches apart in each of their arms . . . scarce to be felt or draw blood, they then stood with their arms exposed to the cold air for about three minutes, till almost dried up: the Matter took effect almost instantaneously, and plain to be seen, the Place where the Dots were made was a little above the other Flesh, like a small sting of a Nettle – No Plaister or anything else whatever put to their arms afterwards . . ."[50]

Up until the Suttonian innovation, it had been standard practice to apply a plaister because of the depth of injections, but this was abandoned with the lighter technique.

The person who came nearest to realising the key importance of the depth of injection in the Suttonian method was, ironically, Edward Jenner. In his first publication on his new cowpox

inoculation in 1798, he reached the following conclusion, based on many years experience of smallpox inoculation:

"I have the strongest reason for supposing that if either the punctures or incisions be made so deep as to go through it [the skin], and wound the adipose membrane, that the risk of bringing on a violent disease is greatly increased. I have known an inoculator, whose practice was 'to cut deep enough (to use his own expression) to see a bit of fat', and there to lodge the matter. The great number of bad Cases, independent of inflammations and abscesses on the arms, and the fatality which attended this practice was almost inconceivable . . . It was the practice of another, whom I well remember, to pinch up a small portion of the skin on the arms of his patients and to pass through it a needle, with a thread attached to it previously dipped in variolous matter. The thread was lodged in the perforated part, and consequently left in contact with the cellular membrane. This practice was attended with the same ill success as the former . . . A very respectable friend of mine, Dr Hardwicke, of Sodbury in this county [Gloucestershire] inoculated great numbers of patients previous to the introduction of the more moderate method by Sutton, and with such success, that a fatal instance occurred as rarely as since that method has been adopted. It was the doctor's practice to make as slight an incision as possible upon the skin, and there to lodge a thread saturated with the variolous matter."[51]

Although Jenner did not distinguish carefully enough between the effects of wounding the "adipose membrane" ("inflammations and abcesses on the arms") and the results of injecting virus into the dermis and beyond ("fatality which attended this practice"), he came nearest to any of his contemporaries in realising the relationship between depth of injection and the success of inoculation. By the end of the eighteenth century however, most inoculators did practice the Suttonian method of inoculation; this was acknowledged by Jenner when he referred to the demise of the old method: "it is very improbable that any one would now inoculate in this rude way by design."[52]

The impact of technique on the safety of inoculation is clearly critical in assessing its historical significance. However, there is one major obstacle which must be surmounted before we can discuss the evidence on the severity of inoculated smallpox: the problem of inoculated people catching natural smallpox before their inoculation. We have already noted how Timoni had experienced this difficulty when attempting to assess the effects of inoculation in Turkey, and it was a problem for anyone attempting to evaluate any form of smallpox prophylaxsis – inoculation or vaccination. British and European inoculators considerably compounded this problem by introducing a lengthy period of medical preparation before inoculation – a period that obviously left those to be inoculated vulnerable to natural infection, particularly during the time of a smallpox epidemic. Angelo Gatti – who had witnessed the practice of inoculation in Turkey and had closely observed its subsequent history in Europe – wrote in 1766 an account of how the European inoculators introduced the practice of preparation:

"Ever since inoculation has been received in Europe, the practitioners have been of the opinion that the essential advantages of artificial and natural smallpox were, 1. the preparation; 2. the discharge of the variolous matter by means of the wounds; 3. the assistance of art in a disorder which is known as soon as it appears. All inoculators have said, prepare your subjects; procure an outlet to the venom; be attentive to administer every help of art, when the disorder spews itself."[53]

The major reason for the introduction of medical preparation again appears to have been the result of a belief in humoral pathology, although Lady Montagu strongly implied that it was in the medical profession's economic interest to bring about this complication of what originally had been a simple operation.[54] There was no single systematic and consistent body of beliefs on the humoral pathology of smallpox which can be quoted from the literature, but James Burges came nearest to justifying preparation in these terms, in his treatise published in 1764:

"and what makes it [smallpox] still more dreadful is, that the poison lies concealed in the blood, while perhaps the unhappy subject, ignorant of the approaching calamity, is urging the latent venom into action, and rendering his constitution unequal to the attack . . . when the skin is . . . obstructed, that the matter cannot find a passage through its pores, and nature wants force to bring on a proper suppuration, the infectious particles being reabsorbed by the blood, occasion those obstructions in the smaller vessels, that generally end in mortification . . . preparation [ensures] . . . that It [the patient's body] is neither too low to support the attack of the infection, or so loaded and overcharged as to obstruct the expulsion of it, or so heated as to conspire with the malady in raising the flame to too great a heighth."[55]

There are inconsistent elements in this passage, but what emerges centrally is a notion of "constitutions" which are too "high" – inflaming the virulence of the disease by energising it – or too "low", so as to be incapable of expelling the illness. In practice, Burges, like his contemporaries, appears to have been most concerned with constitutions which were too high and he noted that "in very lax habits, such as children, and delicate young women, the hazard is less, as such constitutions are in some degree in a natural state of preparation."[56] Constitutions which were thought to be too high were robust and active ones, and the disease was believed to be inflamed by animal foods – but reduced by purging, bleeding and a vegetarian diet.

Preparation therefore took the form of purging, bleeding and restriction to a low diet, and these are measures which were used by the medical profession in the treatment of natural smallpox before the advent of inoculation. Maitland very quickly resorted to dietary measures in the treatment of his inoculated cases,[57] and Nettleton, who published an account of his practice of inoculation soon afterwards, "employed a preparatory treatment of emetics, purgatives, and sometimes bleeding" and required patients to "abstain from animal foods and strong liquors."[58] This period of preparation soon became a lengthy affair, and Rogers writing in 1734, referred to the "necessary Preparations for about three Weeks Diet and Medicine."[59] There was a tendency for this period of preparation to lengthen, and when

Edward Jenner was inoculated as a boy in 1756, he underwent, according to one of his early biographers, the following experience:

"This preparation lasted six weeks. He was bled, to ascertain whether his blood was fine; was purged repeatedly, till he became emaciated and feeble; was kept on a very low diet, small in quantity, and dosed with a diet-drink to sweeten the blood."[60]

We must allow for some exaggeration in this account, as it was written by someone concerned to discredit inoculation, but it probably contains the substance of truth about the length and nature of preparation. The medical profession soon appears however to have realised the irrelevance of much of the preparatory measures, and Monro writing in 1765 about the history of inoculation in Scotland stated:

"When inoculation was first introduced into this country, those who were to undergo it were prepared for the operation by blood letting, purgatives, aperients and low diet: but the gentlemen of practice observing that the eruption did not proceed so well in children thus weakened, as in those who had undergone evacuations, they are now generally omitted; and a mild cooling diet to the patient, or its nurse, with a genteel laxative to empty the intestines, are the principal preparations."[61]

This simplification of preparation does not seem however to have led to a shortening of the time involved, for it would appear that before Daniel Sutton reduced the period of preparation in his practice, this still took "from a month, which then was the usual time."[62]

Such a long period of preparation obviously exposed patients to great potential dangers, particularly during smallpox epidemics. This was unintentionally revealed in an account of a very malignant smallpox epidemic in Blandford, Dorset in 1766, when "a perfect rage for inoculation seized the town."[63] This mass inoculation was widely publicised because of the relatively high proportion of people dying from smallpox after inoculation. A local doctor in describing the "ill success of inoculation" noted that

"out of 384, who lately inoculated at Blandford, 150 were poor people, for whom the parish paid the operations. Not one of these had the confluent smallpox; not one died. Of the rest a great number were in danger from confluent smallpox; and thirteen died . . . a preparatory course was despised . . . the impatience of some was so great, that they left their accustomed apothecaries for the sake of being inoculated a day or two sooner."[64]

The writer of this account tried to blame the high proportion of severe cases on the negligence of preparation, but the very opposite appears to have been the case. The impatient people were those with accustomed apothecaries, and it would have been this group that would have received conventional preparation. No mention is made of the preparation of the 150 poor people, and this is probably because the parish was unwilling to pay the relatively high cost of full preparation. Ironically, it was the richer parishioners, who could afford the cost of full preparation, who were most vulnerable to catching natural smallpox before the effects of inoculation had time to take effetct. The point must not be exaggerated as smallpox spreads relatively slowly from person to person – it was often present in market towns for up to two years before it had exhausted the available supply of victims.

The first inoculator to completely dispense with preparation was a surgeon by the name of Williams. At the end of 1768 he placed the following advertisement in the *Northampton Mercury:*

"INOCULATION WITHOUT PREPARATION (Established by a five years successful Experience, commonly called the Williams Short Method). Mr Williams . . . and a Number of Partners, have inoculated and lightly carried through many thousand persons without the usual tedious and too often injurious preparative Treatment by very strict Diet and strong Mercurial Purges . . ."[65]

Two years after this advertisement Williams died, for at Kibworth, a large village about ten miles from Leicester, on the western wall of the Church is the following inscription:

"In Memory of Mr Lewis Paul Williams, Surgeon. He departed this life January 9th 1771, in his 40th year. He was the first that introduced into practice Inoculation without preparation into this Kingdom."[66]

According to Williams' own advertisement, he first dispensed with preparation in 1763, at about the same time as Daniel Sutton began to shorten the period of preparation in his practice. Williams had much less direct influence than the Suttons, for his name never appeared in any contemporary medical works on inoculation or smallpox. However, as his monument implied, others soon followed him and these others may have been influenced by Williams or his partners. His innovation, like that of the Suttons, was merely a return to the Turkish practice and in one sense an inevitable logical development towards the complete simplification of inoculation.

According to Woodville, Daniel Sutton broke away from his father partly on grounds of disagreement about the period of necessary preparation, Daniel proposing to "reduce the process preparatory to inoculation, from a month, which then was the usual time, to eight or ten days."[67] Because of the eventual popularity of the Suttonlan method, this innovation had great practical importance in reducing preparation, although it is clear that a number of other inoculators quite independently came to the conclusion that preparation was entirely irrelevant to the success of inoculation. Andrew noted in 1765 that

"the little Necessity there is for Preparation is confirmed by the Account I received lately from Dr Swan, of Newcastle, who informs me that in his Neighbourhood 70 or 80 Persons were Inoculated without the least Preparation, and all recovered of the Small-Pox."[68]

This type of empirical observation led to the inevitable conclusion, and Dimsdale in his highly popular book on inoculation of 1767 also came near to reaching it:

"That [improvement] which appears most likely to be made, is in shortening the time of preparation; for as I have often been obliged to inoculate without any, and have always had the same success, it has inclined me to think, that much, if not the whole of this process may be dispensed with (except in very full habits, or where other particular circumstances require it)."[69]

And in the same year, Watts came to a similar conclusion, based on the experience of a friend who was a surgeon who "began to inoculate without any previous preparation at all."[70]

To a very large extent, inoculators were forced to abandon preparation because of the reactions of their patients. We have already seen how people in Blandford had become impatient at the delay in becoming inoculated, and something similar occurred at Witham, Essex in 1779 when "the great mortality which attended the natural smallpox, induced many of the inhabitants to be inoculated . . . In less than a week, upwards of one thousand persons were inoculated without any previous preparation whatever."[71] Similarly, many of the 928 poor people inoculated at Luton in 1788, refused to take their preparatory medicines, in spite of having promised to do so.[72] By 1796, Daniel Sutton could note that "it has been a practice of late, to give up preparation, medicinal and dietetic entirely."[73] However, some more conventional practitioners were reluctant to entirely abandon preparation, and as late as 1797 Woodville lamented "the ignorant and foolish parents who were unwilling to subject their children to the necessary preparation."[74] He linked this to the decline of the use of special isolation houses in which patients underwent preparation. In fact, these houses could be a considerable source of danger to patients during an epidemic, as was shown by the following experience in Hastings:

"In this year [1796-97] the disease [smallpox] was prevalent in all districts round Hastings, and inoculation was general amongst all

classes; houses being specially set up for reception of the patients . . . In these three months [December 1796 - March 1797] 11 persons are stated to have 'died of smallpox in the natural way' and 61 'in consequence of inoculation'."[75]

This high mortality amongst inoculated people was virtually unknown in a normal situation, and it is probable that most of the inoculated people dying from smallpox in Hastings caught the disease during the period of preparation while living in the isolation houses. All this is yet a further example of how the medical profession had difficulty in seeing what was obvious to many of their contemporaries. Woodville's arrogant dismissal of "the ignorant and foolish parents" who ignored preparatory measures, was echoed by a conventional surgeon who complained in 1800 that

"whenever the inoculating rage once takes place, whole parishes are doomed, without the least attention to age, sex, or temperament – no previous preparation, no after-treatment or concern . . . Are not scores and hundreds seized upon at once, for the insidious scratchings, puncturings and threadings, without ever a possibility of their being attended to?"[76]

Much inoculation at this time was beginning to be undertaken by amateur inoculators and even parents themselves, and the outrage experienced by a member of the medical profession whose conventional medical skills were being made redundant, can be felt across the intervening centuries.

Given the variations in technique and methods of preparation discussed so far in this chapter, the evaluation of the severity of the symptoms resulting from inoculation is difficult. A review of the literature nevertheless does lead to certain provisional conclusions, but these must always set in the context of the various complicating factors already discussed.

There are two major ways of evaluating the severity of inoculation: (i) the number of pustules, amount of fever and other symptoms of smallpox; (ii) the proportion of people dying after inoculation. I shall discuss these in sequence, but under the separate

headings. From the very beginning, European inoculators noted the more severe symptoms of their inoculations compared to that of the Turkish experience. Newman, an English lawyer who had witnessed the practice of variolation in New England, wrote in 1722:

"The Number of the Pustules is not alike in all, in some they are a very few, in others they amount to an Hundred, in many they amount unto several Hundreds; frequently unto more than what the Accounts from the Levant say is usual there."[77]

This conclusion is confirmed by the more detailed literature on the number of pustules resulting from inoculation. Robert Waller, an apothecary who practised inoculation in Gosport in Hampshire in 1722 and 1723, gave a list of the number of pustules in his inoculated cases as follows:

"1. Thirty. 2. Thirty. 3. About Twenty. 4. Some Thousands. 5. Four Or Five Hundred, Not To Be Numbered. 6. Fifty. 7. Six Or Seven Hundred. 8. Two Thousand. 9. Four Or Five Hundred. 10. About A Hundred. 11. Fifteen Hundred. 12. Fifteen Hundred. 13. Two Hundred. 14. Fifteen Or Twenty And But Small Ones. 15. Fifteen Or Twenty Little Ones. 16. The Confluent Kind All Over Her, With Many Purple Spots (And Died On The Tenth Day After She Was Inoculated). 17. Two Thousand."[78]

The variation in the number of pustules seems to have been partly a function of differences in technique used by Waller – the cases with the smallest number of pustules appearing to be those where the lightest incisions were made – but the important overall conclusion to emerge from this list, is the much greater severity of symptoms than reported from Turkey, and later found with the Suttonian method. Claudius Amyand, who was the royal surgeon at this time, gave a very detailed account of both his technique and the resulting symptoms in his patients. His deep incisions resulted in the following numbers of pustules amongst his inoculated cases: 150, about 300, some thousands, 12, less than 20, 200, about 200, above 500, "many more than could be numbered".[79]

The greater severity of inoculation in Britain after its introduction in 1721, and before the innovation of technique made by Robert Sutton, is confirmed by a number of authors writing of the contrast between the pre- and post-Suttonian period. For example, Dr Giles Watts writing in 1767 stated:

"A few years ago, he had two of his sons inoculated by a very judicious and experienced practitioner, in the old way. One of them had the confluent small-pox, and hardly, very hardly, escaped with life; and the other too had the distemper very severely. He has often visited patients under inoculation in the old way. And he does not remember, that he ever knew a company of ten or a dozen inoculated together in that way, but one, or more of the company has had the distemper in a pretty severe manner. Very lately he has had four of his family inoculated in the new way, and all of them together have not had so many as eighty pustules."[80]

Similarly, Dimsdale writing in the same year, concluded:

"A considerable share of employment in this branch of my profession has for upwards of twenty years occurred to me; and altho' I have been fortunate as not to lose a patient under inoculation, except one person, about fourteen years ago, who after the eruption of a few distinct pustules died of a fever, which I esteemed wholly independent of the small-pox, yet I must acknowledge that in some cases the symptoms have cost me not a little anxiety for the event . . . such who were treated in . . . [the new] way, passed thro' the distemper in a more favourable manner, than my own patients, or those of the most able practitioners in the old method of inoculation."[81]

We are fortunate to have a very exact account of the number of pustules resulting from the Suttonian method of inoculation from the report of a series of experiments published by Dr William Watson in 1768. Watson was responsible for medical treatment of children in the Foundling Hospital, and decided in 1767 to conduct a trial experiment using the Suttonian method on the children in his care. Although we find this willingness to experiment on children somewhat shocking –

and this was common practice amongst Watson's contemporaries – it has the advantage for the medical historian of providing information under more controlled conditions than is usual with most of the literature considered. As these children were not inoculated as a result of a threat of an epidemic, it removed the risk of prior infection with natural smallpox, and provides an exact account of the effects of Suttonian inoculation. Altogether, 74 children were inoculated, the lancet being "obliquely directed, that the matter might be inserted between the cuticle and the skin" – and as was standard Suttonian practice, no plaister was used to cover the punctures made in each arm.[82]

These 74 children had a total of 2,364 pustules, an average of 32 each. Three cases had a significantly greater number of pustules than average – 440, 260 and 200 – and excluding these three cases gives an average of just over 20 pustules per case.[83] It will be noted by comparing these figures with those quoted in connection with Waller's and Amyand's early practice, that the Suttonian method had dramatically reduced the severity of symptoms, even amongst those with the greatest number of pustules. In fact the Suttons were probably achieving even milder results than the average in Watson's experiment, as there were significant variations depending upon the type of smallpox matter used for inoculation. Daniel Sutton advocated the use of "unripe, crude or watery matter"[84] i.e. material taken at an early stage of the development of a smallpox pustule – and Watson found that this was the only factor of all that he examined that made any difference to the outcome of severity. The 31 cases inoculated with watery matter had a total of 428 pustules – an average of just under fourteen each, the most of any single case being "near two hundred" – whereas as the remaining 43 cases inoculated with purulent or concocted matter, had a total of 1,936 pustules, an average of about forty-five each.[85]

As we have seen earlier, not all inoculators adopted the Suttonian innovation of technique after it had been introduced. This meant that some of them were still producing fairly severe results well after the late 1760s. For example, the Reverend Woodforde noted in his diary on the 8th May, that of the four Custance children inoculated by Dr William Downe at Norwich, two of them had the smallpox "pretty full"[86] This was probably fairly exceptional by

this time, as the Suttons had forced their rivals to adopt their methods through the competitive process of the market [87] Of course the Suttons were in the main only returning to the original simple method of inoculation practised in Turkey, India and elsewhere – in effect undoing the damage done by the medical profession – but they must be credited for having paid very close attention to their experience, and working within the best English empirical tradition.

The reduction in severity of symptoms from inoculation was mirrored in the decline of mortality rates from the operation. The inquiry sponsored by the Royal Society into the relative safety of inoculation had yielded a figure of seventeen people dying out of the 827 people inoculated in 1721-28, i.e. nearly two per cent.[88] Dr James Jurin, who was mainly responsible for compiling the statistics, noted the complication referred to earlier, of people catching smallpox before they were inoculated, and quoted in particular the experience of the New England inoculators, who had inoculated as a result of an epidemic:

"The Reverend Mr Mather, in a Letter dated March 10 1721 from Boston in New England, gives an Account, That of near 300 inoculated there, 5 or 6 died upon it or after it, but from other Diseases and Accidents, chiefly from having taken the Infection in the common way, before it could be given them in this way of Transplantation."[89]

Jurin included five of these deaths in the list of those who died from inoculation in order "to avoid all occasion of dispute",[90] and so it is clear that these early figures on the effects of inoculation overstated its dangers. Some of the deaths however would almost certainly have been due to the inoculation itself, and this comes out through a consideration of specially vulnerable categories of people. Infants and pregnant women were generally recognized as being especially vulnerable to inoculation. When 2,000 people were inoculated in the Hampshire/Sussex/Surrey area in 1740, two of them died – both pregnant women[91] – and of 1,215 people inoculated at Luton in 1788, five died all under the age of four months.[92] These two

examples illustrate that inoculation could kill directly, particularly these vulnerable types of people.

The mortality rate amongst the 2,000 people inoculated in the Hampshire/Sussex/Surrey region – one per cent – seems to have been fairly typical of the pre-Suttonian era. Of 5,554 people inoculated in Scotland up until 1765, 72 died, giving a mortality rate of 1.3 per cent.[93] The inoculation mortality rate in Boston, New England can be traced in some detail: 2.0 per cent in 1721, 3.0 per cent in 1730, 1.4 per cent in 1752, and 0.9 per cent in 1764.[94] Although some of these rates are based on several thousands of cases, they are subject to a degree of uncertainty, as most mass inoculations in Boston occurred as a result of an outbreak of an epidemic, resulting in people catching natural smallpox before resorting to panic inoculations. Individual English practitioners claimed much greater success in their private practice, and this may have been a genuine function of not having to inoculate people as a panic measure during an epidemic. Andrew claimed in 1765 that he had inoculated more than three hundred people in the Exeter area during the previous twenty-three years, "not one of whom has miscarried; and in my whole Practice I have only lost one."[95]

As seen previously, Dimsdale gave an account of an almost identical degree of success during the twenty years and more practice of inoculation.[96] Yet he had to admit that under the pre-Suttonian method, "some of the inoculated have died under this process, even under the care of very able and experienced practitioners."[97] He qualified this admission however, by stating that "this number is so small, that, when compared with the mortality attending, the natural smallpox, it is reduced almost to a cypher."[98] Given the lengthy period of preparation at this time, it is surprising that more people did not die from infections caught previous to their inoculation, and as we have seen, even a practitioner like Barnes who was using very deep injections, was able to claim as early as 1755 that he had not lost one of the four hundred people inoculated by him in the Carlisle area.[99]

It was universally acknowledged however that the Suttonian innovation significantly reduced the risks of dying from inoculation. Robert Sutton is reported to have inoculated 2,500 people

between 1757 (when he first started his practice of inoculation) and 1768, without a single death,[100] and the Suttons claimed in 1767 that they and their partners had inoculated fifty-five thousand people between 1760 and 1767, "of which number six only died."[101] Although it is impossible to assess this claim directly, even those with a vested interest in questioning the success of inoculation did not deny the negligible mortality of the Suttonian method; for example, Jenner's statement that "a fatal instance occurred as rarely as since that [Suttonian] method was introduced."[102] This conclusion was also confirmed by independent practitioners who used the Suttonian method such as Dimsdale and Watts, who stated in 1767 that he had "been concerned in the inoculation of many hundred persons himself, and that without the misfortune of losing a single patient."[103]

Daniel Sutton spread the fame of his father's technique through his spectacularly successfully mass inoculations, which received wide publicity, all the more impressive because it came from unsolicited independent sources. James Hallifax, vicar to the parish of Ewell in Surrey, sent the following item to the *Gentleman's Magazine* in 1766, and had it counter-signed by the local justice of the peace, a churchwarden, and two overseers of the poor:

"On the 1st July, 156 persons, chiefly inhabitants of Ewell, and of various ages, from six months to about sixty years, began to prepare themselves for inoculation, under the care of Mr Sutton. On the 8th of the same month they were all inoculated, most of them from a woman and her daughter in the neighbourhood . . . the eruption . . . seldom amounted to more than fifty pustules, and often fell greatly short of that number . . . Many others, animated with their success, began at different periods, to prepare themselves; insomuch, that the whole number of persons under inoculation, from the 8th of July to the 12th August, amounts to 249 persons, and Mr Sutton pronounces them all entirely out of danger from the small-pox . . . I can declare, upon my own knowledge, that from the 2nd May last (which was before Mr Sutton was known in the parish of Ewell) to this 22nd day of August, 1766, not a single person, either infant or adult, hath died, or been buried in the parish of Ewell."[104]

There are other examples of public announcements of successful mass inoculations after the Suttonian innovation – and a systematic examination of local newspapers for the period would probably reveal a considerable number of these – an example being the advertisement placed by the Churchwardens and Overseers of the village of Irthlingborough, Northants in the *Northampton Mercury:*

"February 14, 1778. INOCULATED in the aforesaid Parish, by Mr Wm, Peaceful, of Twywell, in the County aforesaid, upwards of Five Hundred People; and there is not one in so large a Number, through a divine Blessing, but who has perfectly recovered."[105]

A further example was provided by Dr George Pearson, one of the first of Jenner's supporters in favouring vaccination, and a person who therefore had every reason to point up the disadvantages of inoculation where they existed:

"in the month of October (1798), 800 poor persons were inoculated for the smallpox (at Hungerford, Berkshire) without a single case of death. No exclusion was made on account of age, health, or any other circumstance, but pregnancy; one patient was eighty years of age; and many were at the breast, and in a state of toothing."[106]

These examples do not mean of course that people ceased to die from inoculation; we have seen that some inoculators still used the deep incision method late in the eighteenth century, and a number of instances of mortality from inoculation have been cited, for example, the five infants dying at Luton in 1788. But at the end of the eighteenth century, death due to inoculation was obviously becoming a rare event, and even an institution like the London Smallpox Hospital, which is known to have received cases with prior infection of smallpox, had very low inoculation mortality rates: of 5,694 people inoculated there during the years 1797-99, only nine died (0.16 per cent).[107]

Because of their willingness to act on purely empirical grounds, some of the most effective inoculators were amateurs. One of

the most successful was John Williamson, who was known by his neighbours in the Shetland Islands because of his inventiveness as Johnny Notions. He had invented his own method of inoculation – although he may have been influenced by the Suttons – during the very severe smallpox epidemic in the Shetland Isles in 1769. His method was described in some detail by the vicar of Mid and South Yell:

"He is careful in providing the best matter, and keeps it a long time before he puts it to use – sometimes seven or eight years; and, in order to lessen its virulence, he first dries it in peat smoke, and then puts it underground, covered with camphor. Though many physicians recommend fresh matter, this self-taught practitioner finds from experience, that it always proves milder to the patient when it has lost a considerable degree of its strength. He uses no lancet in performing the operation; but, by a very small knife made by his own hands, he gently raises a very little of the outer skin of the area, so that no blood flows, then puts in a very small quantity of matter, which he immediately covers with the skin that has been thus raised. The only plaster that he uses for healing the wound is a bit of cabbage leaf. It is particularly remarkable, that there is not a single instance in his practice where the injection has not taken place, and made its appearance at the usual time. He administers no medicine during the progress of the disease, nor does he use any previous preparation . . . several thousands have been inoculated by him and he has not lost a single patient."[108]

Williamson had come near to returning the practice to its original folk simplicity, although the burial of the virus underground to lessen its effects, was quite unique to him. English amateur inoculators were just as successful as the Scottish ones, and Dr J. Forbes, although an ardent supporter of vaccination, and opponent of inoculation, had to admit that none of the many people inoculated by the amateur inoculators in the Chichester region in 1821 died. One particular amateur inoculator by the name of Pearce was especially active; he claimed "that of 10,000 persons inoculated by his father, not one died, and that his own success has been as great."[109]

Forbes accepted that none of the 1,000 people inoculated by Pearce in the winter of 1821 had died.[110]

It might be thought that some of the above evidence suffers from being merely historical, and that an element of exaggeration has crept into some of the accounts of the success of inoculation. Fortunately there are recent observations on the practice of variolation (inoculation) that have been made by doctors trained and qualified in modern medical practice. Dr C. D. Rosenwald, who was a medical officer in Tanganika, gave the following account of variolation as it was practised in the southern province of that country in 1951:

"The material for the operation is obtained by inserting a sliver of wood into the smallpox vesicle on the skin of a person suffering from a very mild attack of smallpox. The variolous fluid is then rubbed into a superficial skin wound on the anterior or lateral aspect of the left forearm of the person whom it is wished to infect. This wound may be a cut made with a knife, or scratch or puncture made with a needle or thorn, with or without bleeding . . . There is no denying that the vast majority of cases resulting are mild. I have handled several children, examining their variolation pustules, when it has been pointed out to me that they were then actually in the active stage of smallpox. More careful examination has indeed brought to light a very small number of vesicles."[111]

A similar set of observations were made by Dr P. J. Imperato as a part of his work for the World Health Organisation among the Songhai in Mali, although he suggests that the effects of variolation are even milder than those found in Tanganika:

"the variolation technique used consisted of the application of vesicular fluid with either a thorn or a bird feather to a small round area of 5mm diameter on the deltoid area of the arm or the lateral aspect of the leg just below the knee. There was very little tissue destruction associated with this technique and the inoculuum was small . . . According to one infirmier who had rendered medical care to both villages during the epidemic, the sequence of events of the

variolation reaction was not unlike that of a normal primary vaccinal reaction. He was aware of only two instances in which satellite lesions appeared around the edge of the variolation site . . . observations were made on 120 variolated individuals in eastern Mali. Twenty-two (18.3%) of these people subsequently developed clinical smallpox. The disease in all of these cases was mild, characterized by a rash composed of discrete lesions. There was no mortality associated with the illness."[112]

The surprising aspect of Imperato's account, is the reference to only 18.3 per cent of the cases developing any clinical form of smallpox. Imperato seems to have been unaware of the usual symptoms of variolation – very mild forms of smallpox with a small number of pustules – and it is likely that the pustular eruptions were so mild, that like Rosenwald's first observations, Imperato missed seeing the secondary lesions. This is very similar to the relative invisibility of the Suttonian form of inoculation. May in his account of Sutton's method for example, noted how many of the children inoculated and carried out into the streets "would escape our noticing them as under the Small-Pox, their indispositions being so very slight, and eruptions so few."[113]

Like the Suttonian technique of inoculation, that used in Mali involved very superficial tissue destruction, and therefore would have achieved some of the lightest results possible. Imperato notes the existence of much severer techniques of inoculation elsewhere in Mali, involving very substantial tissue destruction, and these, like their historical counterparts, produced much severer results.[114] The wheel of this chapter has turned full circle: starting with a form of inoculation in India in 1767, remarkably similar in its technique to modern vaccination, and finishing with forms of variolation in modern Africa which are also very much like vaccination in technique and results.

CHAPTER 2
The Contagiousness Of Inoculation And The Process Of Attenuation

As a part of his survey of variolation in Mali, Imperato interviewed a large sample of the local population about their beliefs on the contagiousness of variolation, either as witnesses or as people who had been inoculated themselves. The following Table gives a summary of the findings of the survey.[115]

Age Years	*Total Interviewed*	*Yes*		*No*		*No Opinion*	
	No.	No.	%	No.	%	No	%
0-14	23	0	0	2	8.6	21	91.4
15-29	77	24	31.2	26	33.7	27	35.1
30-44	138	8	5.7	97	70.2	33	24.1
45+	209	0	0	180	86.1	29	23.9
Total	447	32	7.1	305	68.2	110	24.7

A large majority of the total sample rejected the notion that variolation could be the source of secondary contagion and spread smallpox to unprotected people, and this was particularly so among those aged thirty and above. Imperato interprets this to mean that the younger age groups have acquired a greater understanding of modern scientific medicine – that variolation is a significant source of contagion – and that this is a function of their greater education. An alternative view is possible: that the younger generation has had less experience of smallpox inoculation, and that they have been persuaded to accept the assumptions of conventional medical orthodoxy. However, it should be noted that even among this younger age group, only a minority accept the contagiousness of inoculation.

People above thirty would have had much greater experience of the actual effects of variolation, and I will argue that their view of inoculation is very substantially correct.

When inoculation was first introduced amongst medical practitioners in England in 1721, it was not thought to be contagious to those who came into contact with inoculated people. Maitland had from his experience in Turkey concluded that inoculation was not infectious in the way that natural smallpox was, but was soon led to revise his opinions from events in England. At the beginning of October in 1721, Maitland inoculated a two-year-old girl by the name of Mary Batt, a member of a Quaker family living in Hertford. Maitland described the ensuing events as follows:

"what happen'd afterwards was, I must own, not a little surprizing to me, not having seen or observ'd any Thing like it before. The Case was in short this; Six of Mr Batt's Domestick Servants, viz four Man and two Maids, who all, in their Turns, were wont to hug and carress this Child whilst under the Operation, and the Pustules were out upon her, never suspecting them to be catching, nor indeed did I, were all seiz'd at once with the right natural Small Pox . . ."[116]

As there was a smallpox epidemic in Hertford at this time, it is possible that the servants had caught the natural form of the disease, particularly as Maitland was preparing his patients before inoculation – and he may even have infected the servants himself with respiratory virus carried from natural smallpox cases in the area. However, there is no doubt that secondary contagion did occasionally arise in England, but as we shall see later, this was probably a function of the severe technique of inoculation practised by Maitland and his contemporaries.

As a result of this experience of the contagiousness of inoculation, it became a universal consensus of opinion that inoculated smallpox was merely a variant of the natural form, and as we saw in the last chapter, that the success of inoculation was due to the possibility of "managing" the disease as well as selecting a milder form of the virus with which to inoculate people. We shall see later

that this was fallacious and that the severity of the smallpox case from which the virus was taken had little or no bearing on the outcome of inoculation itself. The contagiousness of inoculation was first questioned by Holwell in his treatise on inoculation in Bengal, India in 1767:

"The general state of this distemper [smallpox] in the Provinces of Bengali (to which these observations are limited) is such, that for five and sometimes six years together, it passes in a manner unnoticed, from the few that are attacked with it; for the complexion of it in these years is generally so benign as to cause very little alarm; and notwithstanding the multitudes that are every year inoculated, in the usual season, it adds no malignity to the disease taken in the natural way, nor spreads the infection, as is commonly imagined in Europe."[117]

The lack of contagiousness of inoculation in India was probably partly due to the light technique of injection, and the very mild results achieved. Some inoculators began to notice a similar lack of contagiousness with the Suttonian form of inoculation, and this became a point of issue in the popular practice of the new method. Daniel Sutton believed that the "cold treatment" – exposing patients to cool air as much as possible – was an important part of the success of his method. Although he restricted his private patients to the grounds of his special inoculation house, poorer patients were returned home immediately after inoculation. May in his pamphlet on Sutton's method of inoculation, described how "we often meet with particularly children, who, for the benefit of the open air, are carried into the streets and ways, under all the different stages of Inoculation."[118]

In 1765 Sutton was put on trial at Chelmsford assizes for spreading smallpox in the community at large. The Grand Jury threw out the bill, mainly on the grounds that the type of infection he produced was so light, that his patients could not become a source of secondary contagion to anyone else.[119] The difficulty was, of course, that the case was difficult to prove either way, and contemporaries

continued to strongly disagree about the extent of the danger of secondary infection.

This was a very important practical issue, as it affected whether patients had to be isolated from other members of the community or not. Up until the Suttonian innovation, nearly all inoculated cases were isolated in special inoculation houses, and this both significantly put up the cost and restricted the number of people that could be inoculated at any one time. Daniel Sutton broke through both these constraints and was reputed to have inoculated over 100 poor people in one day, immediately returning them to their usual place of residence.[120] We shall see later that his example was generally copied in the country at large, but for a number of reasons was not followed in the very large towns, in which about a fifth of the total population lived during the eighteenth century. Lettsom and Watkinson became concerned about the neglect of the poor in London, and attempted to remedy this neglect by setting up a popular charitable institution for inoculating the poor in their own homes. This project was opposed by Dimsdale on the grounds that inoculation would spread smallpox to the unprotected population.

This objection was invalid – virtually all children living in London caught smallpox by the age of seven in this period – but Watkinson attempted to refute Dimsdale arguments directly on the question of secondary contagion:

"I have paid particular attention to the point in question, since the establishment of the dispensary for general inoculation; and can with truth affirm, that a single instance has not yet occurred in that charity, in which the contagion has been spread by an inoculated patient. Where the chance of spreading it has been apparently great, I have been very strict in my inquiries. In many cases the circumstances have been such, that if the apprehensions of a celebrated inoculator [Dimsdale] were well founded, the distemper must inevitably have been communicated. Some have been inoculated in narrow streets, in the midst of those who were obnoxious [vulnerable] to the smallpox, and others in little courts, where, according to the common opinion, the danger of communicating the disease was still greater. In the latter case, the

patient has sometimes been kept in a little room on the ground floor, the door of which opened directly into the court, and in the day time was seldom shut. Before this door, and within a few yards of the person inoculated, a number of children have continued to play during the whole course of the disorder, and, as has been already affirmed, without receiving the infection."[121]

In addition to their own personal experience in London, Lettsom and Watkinson noted that inoculation did not appear to spread smallpox in other places. A mass inoculation took place in Ware, Hertfordshire in 1777, "and a few families in the town did not choose to submit to Inoculation with the rest of their neighbours; not one of them, however, caught the infection, although Inoculation was otherwise general ['about one hundred were inoculated']."[122] Dimsdale was sufficiently puzzled by these experiences to write to various foreign inoculators about the subject. In 1777 Professor M. W. Schwenke wrote to him from the Hague:

"I believe in England, as well as other provinces, there are some who are enemies to Inoculation, from prejudice, obstinacy and ignorance, while there are others who are deprived of its benefits by want of opportunities, or through their inability to bear the expense of it. But this does not prevent us from inoculating every year at the proper season, whether the epidemical Small Pox reigns or not; and it may be affirmed that no epidemic has ever been occasioned by this practice. The epidemical Small Pox discovers itself among us, almost regularly at certain periods, just as it did before the practice of Inoculation was introduced . . . This is certain, last year, when the epidemic which reigned with violence in our neighbourhood was expected here, I myself inoculated forty-eight persons, and a like number underwent the operation in the hands of other physicians. The inoculated persons walked, or rode out in carriages, every day (except two that were very ill) without anything like an epidemic ensuing."[123]

This letter did not change Dimsdale's views about the degree of danger of inoculation spreading smallpox. What is surprising is

that Dimsdale's own experience did not lead him to modify his opinion. One writer noted the effects of inoculation in Dimsdale's county of Hertfordshire when the popular practice of the Suttonian method was introduced:

"At the introduction of that method, the subjects obnoxious to the disease were more numerous in proportion to the exempts, than they could possibly be in London at any period. Baron Dimsdale under whose direction a principle share of the practice was conducted, was not deficient in imposing such restrictions [of movement in public] on his patients as he thought necessary for public safety; but I believe these restrictions were not very scrupulously regarded. There were practitioners, whose practice was by no means inconsiderable and whose restrictions were less strenuously imposed and more frequently broken, yet few instances of infection from inoculation were heard of . . ."[124]

Some observers even pointed out that inoculation prevented the spread of smallpox; for example, Haygarth in 1781 noted that in Chester,

"Inoculation did not, as some might apprehend, spread the contagion, but appeared to produce a quite contrary effect. For in the districts, where most patients were inoculated, there remained the fewest in the natural small-pox; and in the districts where the smallest number were inoculated, the distemper was afterwards most general."[125]

This result can only be explained by assuming that the inoculated cases were rarely a source of contagion, and actually reduced the number of potential carriers of the natural disease. A similar phenomena occurred in Boston, U.S.A. in 1792 during a general inoculation: 9,152 people were inoculated, yet there were only 232 cases of natural smallpox in the town, while 221 people escaped the disease altogether.[126] Inoculation in this situation checked the spread of natural smallpox, and this was not only possible through the inoculation of virtually all the vulnerable population, but also because it did not spread the disease itself – otherwise the 221 people escaping smallpox would have been infected from the 9,152 inoculated cases.

Ideally, in order to evaluate the risk of inoculation spreading smallpox, experimentation would be necessary. The only experimental evidence to come to light is that accidentally supplied by Dr O'Ryan, Professor of Medicine at the College of Lyons, France, who conducted the following experiment during the latter period of the eighteenth century:

"I placed a person in the eruptive fever of the smallpox by inoculation at the distance of about half a yard from four children properly prepared; each exposure continued one hour, and was repeated daily for a fortnight, reckoning from the commencement of the fever till the pustules were become sufficiently dry: not one of the four received the infection. Two months afterwards, I inoculated three of these children, they had the distemper in a very mild manner and recovered without difficulty."[127]

O'Ryan was unaware of the difference between inoculated and natural smallpox in terms of their effect in spreading the disease, and concluded "that there is no risk of contracting it [smallpox], provided the person who is liable to the infection, keeps himself at a very little distance from patients in the smallpox, or from things which they have touched."[128] This is now known to be incorrect, for a major route of natural smallpox infection is via the respiratory tract, partly because the virus is expelled over a sufficient distance to form a significant source of contagion. The period in which the smallpox patient is infectious usually commences after the termination of the incubation period, which on average is about twelve days after the smallpox patient catches the disease.[129] In O'Ryan's experiment, the children were exposed to the inoculated patient at the time of the eruptive fever, which occurs at the end of the incubation period and therefore would be the beginning of the period of infectivity. It is therefore probable that if the inoculated patient in the experiment was highly infectious, the children would have caught the disease.

In 1791 Haygarth published a letter that he had received from the Council of Geneva, giving yet a further example of the non-contagiousness of inoculation:

"An epidemic of smallpox is of almost regular occurrence every five years, and between the epidemics it frequently happens that we have no natural smallpox whatever, little in the City or its vicinity. Inoculation began to be practised here in 1751, since when we have inoculated a very large number of children annually, and with such marked success that the deaths have not exceeded 1 in 300. Although we have often had to inoculate with pus brought from a distance at times when there was no smallpox to be found in the City, and although children so inoculated have gone freely into the streets, walks, and other public places, before, during, and after the eruption, we have never observed that they were sources of contagion, nor that they produced any intermediate epidemic, nor that they accelerated the return of the periodical epidemic."[130]

This is strong evidence for the rarity of secondary contagion from inoculation, and is very similar to that already quoted for Bengal and the Hague. Of course, in all these cases the number of susceptibles between epidemics would not be high, and would therefore reduce the risk of infection. Nevertheless, it is clear that in a place like Geneva, inoculation must have been of minimal infectivity, even during the pre-Suttonian era of the 1750s and 1760s. This evidence is the more important because it refers to a place where smallpox was not present in its natural form to complicate the interpretation of events. In the absence of natural smallpox, inoculation appears to have very rarely led to secondary contagion.

However, it is almost certain that inoculation did on occasions give rise to secondary infection. Ironically, the best evidence for this comes out of the history of early vaccination. After the initial experimental period when cowpox was used as the source of the vaccine, the main stock used by Jenner and his contemporaries became contaminated with smallpox. This is an ideal situation with which to evaluate the contagiousness of inoculation, as the early vaccinators were not expecting any secondary contagion from their inoculations – vaccination was defined by Jenner as non-contagious – and the noticing of secondary contagion would be all the more impressive for not being expected. The person primarily

responsible for the development of the main stock of vaccine (the "world's lymph") was Dr William Woodville of the London Smallpox Hospital, where the contamination of vaccine with smallpox had taken place. In his first report on the new vaccination he wrote:

"One important advantage which the Cow Pox is supposed to have over the Small Pox is that the former is not a contagious disease, and not to be propagated by effluvia of persons infected with it. This is certainly true when the disorder is confined to the inoculated part, but where it produces numerous pustules on the body, the exhalations they send forth are capable of infecting others in the same manner as the Small Pox. Two instances of casual infection in this way have lately fallen under my observation . . ."[131]

Although most of Woodville's inoculations had led to relatively mild symptoms – the first 459 people to be vaccinated had an average of 78 pustules each – there were a very small minority who suffered severely, with 700 pustules or more. It is almost certain that it was these cases that gave rise to the secondary infection discussed by Woodville. The vast majority of the cases of early vaccination did not lead to secondary contagion in spite of numbers of secondary pustules, and this was partly a function of the increasingly attenuated smallpox virus being used. Only two clear examples of secondary infection from vaccinated cases have emerged to date from a study of the literature: the minor smallpox epidemic at Petworth in Sussex at the end of 1799 deriving from vaccine supplied by Dr George Pearson, and the more serious epidemic started at Marblehead near Boston in the United States arising probably out of vaccine sent by Jenner to Benjamin Waterhouse.

The vaccinated cases which started both these outbreaks of smallpox had very severe symptoms of smallpox, and it appears that there had been a spontaneous resurgence in the virulence of the virus used in the vaccinations. However, the rarity of secondary contagion from this form of smallpox inoculation is indicated by the absence of other documented examples other than the Petworth and Marblehead incidents. Undoubtedly, the more orthodox Suttonian form

of inoculation did on occasions lead to secondary contagion, but the minimal degree of the nature of this contagiousness was probably accurately summarized by Haygarth when he wrote that "the danger of infection is much (perhaps thirty or fifty) less in the inoculated than the casual smallpox."[132]

Given this conclusion about the relative non-contagious nature of smallpox inoculation, we must raise and attempt to deal with the difficult question as to how these attenuated effects were achieved by inoculation. There is no virological or medical consensus as to how the variolators were able to achieve such successful results, and therefore the following discussion will necessarily be speculative. The first point to be noted is that there was no one-to-one relationship between the type of virus inoculated and the severity of the results of inoculations. Initially, European inoculators believed that the success of variolation was partly due to the mild form of virus selected for inoculation (i.e. virus was taken from mild clinical cases of natural smallpox), but this view was soon discredited through empirical observation. In 1749, Frewen published the following summary conclusion:

"Experience has convinced me, that it is in reality of no consequence from what kind of Smallpox it is procured. I knew one and twenty persons inoculated, the same day, with matter taken from one who had a confluent Small-pox and died of it; yet these, notwithstanding, all had it in as favourable a way as could be wished for. And I have inoculated many more with matter of the malignant kind, without any manner of ill effect." [133]

Daniel Sutton even claimed that the results of inoculation were severer when virus was taken from a "benign" case of smallpox than when it was taken from a "malignant" one, although he produced no detailed evidence for this conclusion.[134] The irrelevancy of the severity of the disease in the person from whom the virus was taken for inoculation was further confirmed by Mudge in his dissertation on inoculation published in 1777:

"Several patients have been inoculated from a confluent smallpox, which have proved mortal to its own subject, and yet have had the disorder in a very favourable way. Others have been inoculated from malignant sorts with equal success; nay, which is still more, we are told by Chandler in his essay, that in inoculating hospitals, persons have been safely infected with matter which has been taken off after the death of the patient. These, and other instances which must have occurred to men of business in this way, plainly shew that the benignity of the infecting matter has very little share in the wonderful effects of inoculation."[135]

This conclusion had become generally accepted by the end of the eighteenth century and Woodville summarized the consensus of opinion when he wrote in 1797 that it does not "signify whether the matter is taken from a mild kind or from the more virulent sort."[136]

This conclusion may have come to affect the medical view about the relationship between intrinsic virulence and clinical severity of natural smallpox. Generally, it has been the view of microbiologists and virologists until very recently, that the clinical severity of smallpox was in the main not a function of its intrinsic virulence. The only exception to this view was the distinction between variola major and variola minor, the former being much more virulent than the latter. In the last few years however, evidence has begun to accumulate to suggest that this view is mistaken. Marennikova and Shafikova have carried out research involving the comparative study of the properties of various variola virus strains taken from patients with varying clinical severity of the disease. They found that "the virus strains isolated from patients suffering from haemorrhagic forms of smallpox were usually more pathogenic for chick embryos than those isolated from other forms of the disease."[137]

The degree of statistical significance of these findings is not however very great and they go against the mainstream conclusions of modern research, which tend to show little correlation between clinical severity of individual cases of smallpox and laboratory measures of intrinsic virulence. On the other hand, it has now been

established that the severity of particular strains of smallpox virus within specific geographical areas are significantly correlated with laboratory measures of virulence. Shafikova and Marennikova found a relationship between fourteen strains isolated from patients with different severities of smallpox, and pathogenicity for suckling and irradiated adult white mice inoculated intra-cerebrally and intra-nasally.[138] Also, work carried out in conjunction with the W.H.O. smallpox eradication campaign involving the laboratory study of 200 strains of virus from all parts of the world, has tended to show quite distinct geographical patterns of pathogenicity, suggesting a number of specific regional viruses.

Dumbell and Huq have questioned the validity of the distinction between variola major and variola minor and have concluded that "recent observations during the smallpox eradication campaign fit in better with the idea of a spectrum of variola viruses of differing pathogenicity, ranging from a minimum in Brazil to a maximum in Bangladesh."[139] This conclusion is consistent with the fact that the pathogenicity of smallpox is known to have varied enormously within a particular region over long periods of time; for example, as we shall see later, the case-fatality of smallpox was of the order of five per cent in England at the end of the sixteenth century, and rose to over forty per cent by the middle of the nineteenth century.

A related finding of recent research that has a direct bearing on the explanation of the very mild results achieved by variolation, has come out of the work of Sarkar and his colleagues in India. Sarker et. al. studied the relationship between the clinical severity of smallpox and the excretion of virus in the throat and urine and found that "clinically more severe (haemorrhagic and confluent) cases excrete more virus than less severe (discrete) cases and the period of excretion is longer in the first two groups than in the last."[140] This conclusion applies to variations of clinical severity within one particular strain of virus, and again is not highly statistically significant and would tend to go against the mainstream of virological research. The correlation between clinical severity and period of infectivity does however probably apply to different strains of smallpox virus in

specific geographical regions, for as Dixon has noted, with milder forms of smallpox

"the period of infectivity is exceedingly short, only lasting a few hours, and the quantity of virus small, and if this occurs at night this patient is quite likely to miss infecting any contacts, even those living in the same house. This has been noticed particularly in outbreaks of variola minor, where . . . the low degree of infectivity has been frequently commented upon."[141]

However, there is obviously no simple one-to-one relationship between clinical severity and infectiousness. The historical literature provides abundant examples of a single strain being introduced into a community with a complete spectrum of resulting severity. This means that an apparently mild case of smallpox may in fact be the manifestation of a virologically virulent strain, with a highly infectious nature. If however inoculation produced a fundamental attenuation of the smallpox virus – as would appear to be the case – this would lead to a significantly diminished power of infectivity.

We have now reached the point where we must consider the central question as to how inoculation brought about such a radical attenuation of symptomology. The first possibility is the route by which the virus is introduced into the body. In natural smallpox infection, the virus enters via the respiratory tract, while in inoculation as practised in Europe, it was always introduced via the skin. There is one major insurmountable objection to this hypothesis, at least in its simplest form. In China, Persia and elsewhere, the virus was introduced by inoculation through the nasal passage, presumably entering the respiratory tract in the usual way; yet the results appear to have been as successful with this method of inoculation as with the more usual mode via the skin. Although no scholarly study of Chinese inoculation has ever been published, there is sufficient evidence to come to certain tentative conclusions. One of the most detailed accounts of Chinese variolation was published by Dr W. W. Peter at the end of the nineteenth century:

"One (method of inoculating) is . . . plugging the nostrils with cotton previously saturated with a mixture of water and pustular-crustaceous matter taken from the eruption of a smallpox patient. Another is to blow finely crushed, fresh scabs into the nose through a bamboo pipe. It may also be done by introducing the smallpox matter through a puncture, an incision or an abraided surface of the skin . . . The crop is less profuse than in ordinary smallpox and limited to about two hundred points . . . About one in five hundred die."[142]

Most of the other descriptions of Chinese inoculation available in English confirm Peter's account,[143] although at least one report indicated that the operation was not always as predictably safe.[144] The most frequent method of inoculation seems to have been the blowing of dried scab powder up the patient's nose, which appears to have been as successful as the more usual method of injection via the skin. Given the great variety of routes of inoculation employed by the variolators, it would seem that the route of inoculation is not crucial in the explanation of the success of inoculation.

Wheelock has recently put forward an ingenious hypothesis for the relative benignity of inoculated smallpox: that when variolators took matter from a smallpox pustule or crust, they were also taking interferon, which is known to both appear in dermal crusts of vaccinated cases, and be an effective antiviral agent.[145] There are however a number of problems with this hypothesis: interferon was only found in four out of five crusts, which would lead to a much higher failure rate than experienced in inoculation, and it probably is not as stable as would be required by some of the historical evidence, i.e. it is unlikely to have been able to survive the seven to eight years burial underground as practised in John Williamson's highly successful technique of inoculation. The most important objection however to Wheelock's hypothesis is that it cannot account for a number of observations made on the process of attenuation through arm-to-arm inoculation, or the role of the depth of the injection in bringing about milder symptomology as discussed in the first chapter.

In my book on Jenner's vaccine, I have cited evidence to indicate that the smallpox virus used in Woodville's lymph was

gradually attenuated through arm-to-arm inoculation, always selecting virus from a previous site of injection. I argued that this process of attenuation was achieved through the natural selection of "cold variants" that were particularly adaptable to the cooler areas of the skin surface than the more virulent strains of virus. There are good reasons to believe that a similar argument can be applied to the explanation of the benignity of more conventional forms of variolation. We have seen in the previous chapter how the depth of the injection was important to the outcome of inoculation, and it can be hypothesized that the temperature gradient between the skin's surface and the inner body areas is the critical variable in explaining this fact. More specifically, the lighter injections of the sort used by the Suttons would implant the virus in the epidermis, whereas the heavier inoculations practised by the early European inoculators would push the virus through the dermis into – in many cases – the blood stream. Timoni described how many of these heavy inoculations either failed altogether or brought about a very severe reaction. Although these diametrically opposed responses would appear to be paradoxical, they do in fact fit what one would expect from a number of experimental observations, as we shall now see.

Daniel Sutton conducted a series of trial inoculations, which were summarized by him in his book on inoculation in 1796:

"I have . . . repeatedly tried to communicate the disease, by conveying considerable quantities of active virus into the stomach, in the form of pills, but never with effect; both cool and typical clysters of water, strongly impregnated with the contents of many ripe and unripe pustules, have likewise been administered; this way too, I have always failed of communicating the disease."[146]

In addition to these experiments, he attempted to inoculate a number of people with very deep skin injections, again without success.[147] Smallpox virus is known to be highly temperature sensitive, and the prediliction of the virus for the skin surface is probably the result of its ceiling temperature. Variola major will not grow on the chorio-allantois above 38.5°C or variola minor above 37.9°C, whereas the body

temperature reaches 39.4°C and above during the second day and onwards of the illness of smallpox,[148] suggesting that fever is a defensive response of the body against such viral attacks. As Downie has noted, "the onset of fever in smallpox might limit growth in the internal organs while permitting such growth in the skin and in mucous membranes of the mouth and upper respiratory tract, where temperatures may be a degree or two less."[149]

Recent unpublished research by Dumbell however, indicates that smallpox virus can grow at higher temperatures in human than in chick cells, and these ceiling temperatures are therefore probably not so critical as they appear from the published evidence. Similarly although earlier work suggested a correlation between virulence and ceiling temperature for many of the pox viruses,[150] recent and unpublished work also by Dumbell indicates little association between the case-fatality rate of a particular strain of smallpox virus and laboratory measures of ceiling temperature.

There is however one naturally occurring form of "cold" smallpox virus – the strain previously identified as variola minor. There is also experimental evidence to suggest that temperature can be critical in bringing about changes in the virulence in some of the pox viruses. Kim and Braunwald produced a cold variant of vaccinia by growing the virus at regularly decreasing temperatures, losing completely "its virulence in mice by the intra-cerebral route", and its intra-dermal infectivity in rabbits was 41 times weaker than the wild virus.[151] Similarly, Baxby found a correlation between the pathogenicity of seven smallpox vaccines for human beings and their capacity to grow at elevated temperatures on the chick chorioallantois.[152] Relevant to the present argument is the work of Dumbell, Bedson and Nizamuddin, who have successfully produced a thermo-efficient strain of variola major virus. Two strains of virus were grown at increasing temperatures through serial passage in the chick choriallantois, and both became genetically stable viruses capable of greater growth at higher temperatures. One of them – which had been grown at regularly increasing temperatures without a pass at a lower temperature – was also less capable of growth at the lower temperature.[153]

Bringing together all these observations, we may hypothesize that the deep injections of the early inoculators partly failed because they were putting virus directly into the blood stream, the temperature of which was higher than that in the epidermis. However, at the same time, a process of natural selection can be seen as to have been at work, with only thermoefficient strains of virus being able to grow in the higher temperatures of the blood – thus the paradoxical finding described by Timoni, that either there was no reaction to the deep injection, or there was a very pathogenic one. Virus found in skin lesions is likely to be a "colder variant" of that found in the blood; more thermo-efficient viruses would simply not be able to survive the cooler temperatures at the skin's surface.

Thus when inoculators took virus from skin lesions they would be selecting a form of cold variant – human selection of virus which had been naturally selected on grounds of temperature. Although this hypothesis cannot be proven with the evidence which is at present available, it has the merit of being consistent with both the historical and modern virological literature – and linking: (i) findings about the attenuation of smallpox virus through arm-to-arm inoculation in early vaccination; (ii) the importance of the depth of injection in the success of inoculation; and (iii) the overall explanation of the benignity of inoculated smallpox compared to the natural form of the disease.

It should be stressed however that the above is highly speculative and in no way crucial to the overall argument of the present book. Much recent evidence would appear to go against any simple "cold variant" hypothesis, and it is possible that alternative virological explanations – for example, that the inoculation of a large amount of virus would bring about attenuation through propagating "defective" viruses – will turn out to be more plausible. Whatever the ultimate virological explanation, it is clear that an identical virus can be made to specialize in particular organ sites, dramatically limiting its capacity to propagate outside of its zone of specialization. Ledingham and McClean found as long ago as 1928 that vaccinia virus propagated in the rabbit dermis through serial passage, led to enhanced potency of the virus for the dermis, but

a "loss of propagating power on scarification surfaces"[154], i.e. virus adapted to grow in the dermis, lost its capacity to grow effectively on the skin surface. Smallpox virus selected from the skin surface for purposes of inoculation, is likely to have been relatively specialized for growth in the skin, with only limited capacity for generalisation throughout the body.

On the present argument, both inoculation and vaccination involved the attenuation of smallpox virus. Inoculation was the less attenuated form, but although inoculation spread secondary infection on very rare occasions, this was more than counter-balanced by the longer period of immunity produced through the larger and more effective amount of antibody – inoculation protected in the vast majority of cases for life. Also, ironically the belief that inoculated smallpox was as contagious as the natural form of the disease, led many communities to adopt the practice of general inoculation – the inoculation of all vulnerable members of a community at one point in time. But this, and other aspects of the history of inoculation, will be dealt with in following chapters.

CHAPTER 3
The Early Practice Of Inoculation And Factors In Its Retardation

In April 1721 Lady Mary Wortley Montagu had her daughter inoculated in London, and from this date onwards inoculation came into fashion amongst the aristocracy and gentry, particularly after Princess Caroline had her two daughters Amelia and Caroline inoculated in April 1722. According to the inoculation censuses conducted by Jurin and Scheuchzer during the 1720s, there were 897 inoculations in Britain, America and Hanover during the eight years 1721-28.[155] After 1728 no attempt was made to count the number of inoculations, which led Creighton to conclude that

"for the next ten or twelve years they were of no account. The southern counties led the revival in the fifth decade of the century, so that before long some two thousand had been inoculated in Surrey, Kent, Sussex and Hampshire."[156]

This conclusion has been questioned by Miller who has argued that at no time did inoculation cease to be practised, and quoted the examples of inoculations taking place in Haverfordwest, Pembrokeshire in 1732, in Bury and Dumfries, Scotland during 1733, and in Ireland in 1734.[157] However, she also points out that "the number of publications on the subject declined, so that during the 1730s one finds only a few pamphlets and occasional journal articles."[158]

The decline of inoculation was noted by the Reverend J. Hough who wrote in 1737 that "the method loses ground, even in this country."[159] Charles Deering, a medical practitioner in Nottingham, argued in a treatise on smallpox written in 1737 that "all who are inoculating do well, yet such is the way of thinking amongst the Generality of Man . . . that not one in five thousand either submits or is submitted to that Operation."[160] Thus, although Deering indicates that inoculation was not very popular, he does suggest that it was still being practised in 1737. This conclusion is confirmed by an entry in the diary of John Hervey, First Earl of Bristol: "on new years day (1736/37) arriv'd at London, with Miss Betty Hervey to be

inoculated."[161] It was necessary for Hervey to travel to London to obtain inoculation, which suggests that it must have been rarely practised in the countryside.

Whatever the changes in the amount of inoculation during 1721-40, contemporaries were unanimous on the insignificance of the practice at any time during this period. Jurin explained in 1724 why the practice was not more popular: "People do not easily come into a practice, in which they appreciate any hazard, unless they are frightened into it by a greater danger."[162] We shall see later that the fear of catching natural smallpox (particularly during epidemics) was invariably a necessary stimulant to the practice of popular inoculation. The psychology of this attitude is not difficult to understand, for a remote risk, however dangerous, is often preferable to an immediate one. This fact was noted by the Reverend J. Hough in 1737 when attempting to explain why inoculation was losing ground:

"for parents are tender and fearful, not without hope their children may escape this disease, or have it favourably, whereas, in the way of art, should it prove fatal, they could never forgive themselves: for this reason, nobody dares to advise in the case."[163]

Such an attitude could flourish only where there was a known risk of dying from inoculation, and as we have earlier seen the practice of inoculation was fairly dangerous during the period under discussion.

Another important factor in the retardation of the practice of inoculation was its very high cost during the early period. One gentleman wrote the following entry into his diary in 1743:

"Memorandum the 17th of January this year, my son and Miss Molly Tregonwell were both inoculated by Mr Goldwyer, Surgeon of Blandford, whose pay for the said inoculation was 20 guineas."[164]

Inoculation was so expensive at this time because of the lengthy period of preparation and after-treatment in special isolation houses, along with the complicated procedures of blood-letting and

purging, as well as the special medicines prescribed by attendant physicians. The 10 guineas per person would have included board and lodging during the five or six week period "necessary" for the whole operation. The above example was not untypical of the period, as is seen in the accounts of the Bristol Infirmary where £623 was paid for the inoculation of 78 people in 1743.[165] Inoculation was to be had for a cheaper rate under special circumstances, such as the inoculation of the poor. One gentleman wrote in 1750 that:

"Several years ago a noble person near Guildford in Surrey, observing the terror of the country people, on account of the small-pox, allowed Mr Howard a skilful surgeon of that place, the sum of 40s. for every one that he should inoculate and attend."[166]

This price differential between the rich and the poor was maintained throughout the whole of the eighteenth century, although the absolute level of prices was very radically reduced. The price of inoculation during the period 1721-50 was obviously too high for the great bulk of the population, and in 1752 one writer observed that:

"before it can come into general use, it must be done in a less expensive way . . . The poor in general are absolutely cut off from all share in it . . . And not only the very poor people, but multitudes of others, many farmers and tradesmen, cannot be at the expence of so much a head for their whole family, as it is at present demanded, merely for the operation of inoculating, besides the other additional charges which must necessarily accrue."[167]

The high price of inoculation continued to deter people from undergoing the operation as late as 1760, for when a smallpox epidemic struck the Shetland Islands in 1760, "owing to the high fee (two or three guineas) of the operator, only ten or twelve persons availed themselves of it."[168]

A less important factor retarding the spread of inoculation was the opposition due to religious opinion. In 1724 W. Beeston wrote a letter on the subject from Ipswich:

"The practice of Inoculation in this Town, has so inflamed the angry passions, and stirred up the bitter Zeale of the bigotted high Churchmen, and Dissentors, to such a Degree: that they Sentence to Damnation, all that are in any way Concerned in It. They say the practice is Heathenish, and Diabolicall, it is distrusting Providence, and taking the Power out of God's hand, it will draw down Divine Judgments . . ."[169]

The most notorious religious opposition came from the Reverend Edmund Massey, who preached on "The Dangerous and Sinful Practice of Inoculation" from the pulpit of the parish church of St. Andrews, Holborn (London) on July 8th, 1722.[170] More important than formal religious opposition though, was popular prejudice against inoculation, which although couched in religious terms, was really a reflection of anxiety about incurring deliberate risks for a future remote gain. Dr John Andrew illustrated this from his experience in the Exeter area:

"The chief Argument urged by foolishly fond or superstitious Parents, against this Practice, is, that it brings a Distemper upon their Children, which they might never have, and that if any one of them should die, they should never forgive themselves, on Account of their having (as they term it) presumptously tempted Providence."[171]

Partly as a result of these prejudices, Andrew was forced at the beginning of the 1740s to practise inoculation "in the Dark, visiting my Patients only by Night."[172]

However, the major reason why Andrew was forced to practice inoculation under cover of darkness, was probably fear by the general population that his inoculations would spread smallpox within the community. A similar experience to his took place soon after the London Smallpox Hospital was set up in 1746, and patients

who had been inoculated, "on leaving the hospital were often abused and insulted in the street, so that they were not suffered to depart until the darkness of the night enabled them to do so without being observed."[173] The fear of inoculation spreading smallpox sometimes led to drastic action on the part of the local population, particularly when there was no natural smallpox in the area:

"Sutton and Bond, inoculators, having opened a house near Peterborough, the mob rose, to prevent, as they said, the spreading of infection, by introducing a distemper that was not then in that neighbourhood, and threatened to pull down the house, which they effected next day, after an obstinate resistance, in which several were wounded, and the undertakers obliged to decamp."[174]

Hostility to inoculation on these grounds was particularly strong in market towns, where there was great anxiety that a whiff of smallpox would ruin local trade. This was reflected in innumerable entries in local newspapers; for example, on May 12, 1762 the following announcement appeared in a Colchester paper:

"The Practice of bringing people out of the country into this town to be inoculated for the Small-pox being very prejudicial to the town in many respects, but especially to the Trade thereof, and as by this practice the distemper may be continued much longer in the town than it otherwise would, in all probability, it is thought proper by some of the principal inhabitants and traders in the town, that this public notice should be given that they are determined to prosecute any person or persons whomsoever, that shall hereafter bring into this town, or who shall receive into their houses in the town as lodgers, any person for that purpose, with the utmost severity that the law will permit . . ."[175]

The announcement went on to state that it had no objection to the practice of inoculation, as long as it was conducted in houses well isolated from the town. The fear of the townsmen that inoculation would spread natural smallpox was, of course, based on the contemporary assumption that it was just a milder form of smallpox,

which was thought to be as dangerous as the most virulent form of smallpox.

There was also popular opposition to inoculation on medical grounds, although like the belief in the highly contagious nature of the operation, it was not always based on objective evidence. D. Hartley, listed in a pamphlet published in 1733, the following medical objections made by the general population:

"We are not certain that Inoculation is a Security from having the Distemper again . . . Inoculated Small Pox often leaves bad Consequences, as Consumptions, Boils, and Blotches, weak Eyes, etc . . . [and] may communicate other Distempers."[176]

Hartley could not refrain from pointing out, "that the natural Small Pox is apt to leave the same Sort of ill Consequences, is known to everyone", only to a much greater degree.[177] The medical profession itself was by no means unanimous in the earlier period in favour of inoculation; as late as 1747, Mead could write that inoculation "has drawn our physicians into parties, some approving, and others disapproving this new practice."[178]

By far the most important factor in the removal of checks on the spread of inoculation was the reduction of mortality from the operation due to the improvements of technique, as was shown by the very rapid spread of inoculation after the innovations made by the Sutton family. The latter point is illustrated by contemporary descriptions of the effect of the successful Suttonian method:

"it is natural to suppose that the great success attending, and emoluments arising from the Suttonian art, may induce many to become imitators of their method of inoculation. And in fact this is so much the case, that in every county in England you meet with the advertisements of these pretenders and itinerants . . . Some of them as before observed, advertise that they inoculate according to the new method; others according to the Suttonian method; while others have the modesty to deck their imposition with the style of, 'The Suttonian art improved'."[179]

In fact, as we will see later in much greater detail, the Suttonian method was the beginning of the really popular practice of inoculation. In order for this to be possible, it was necessary for the price of inoculation to be radically reduced from what it was during the earlier period. This was carried out by the Suttons who introduced differential prices according to the type of inoculation and the financial circumstances of their patients. The following advertisement was placed in the *Norwich Mercury* on the 25th January, 1777:

"Messrs. Sutton and Son respectfully inform the public that they continue to inoculate for the small-pox at their house in Framingham, near Norwich, on the most reasonable terms. The greatest respect being had to various circumstances of the patients different accommodations are provided from two guineas and a half to ten and upwards. General terms, four guineas. The small-pox being at present very rife not only in Norwich, but in most parts of the county of Norfolk, Messrs. Suttons continue as usual to inoculate parties at their own houses on terms agreeable to circumstances from half a guinea upwards. Servants and the poor in general (not less than eight in number) at five shillings and threepence . . . The officer of any parish, by applying to Messrs. Suttons, may have their poor inoculated gratis."[180]

This type of price discrimination became the most frequent method by which the professional inoculators maximized both numbers inoculated and profit. Not all medical practitioners were concerned about profit, as is shown by an entry that appeared in the *Northampton Mercury:*

"To the Poor of Northampton. As the Small-Pox now prevails on the Town, and many Persons wish to have their Children Inoculated, but are deprived of this Advantage by their Inability to defray the Expence. Dr Hardy informs all Persons of this Description, that on their producing to him a Certificate signed by the Minister or Churchwarden of their respective Parishes, that their circumstances

are such as must render the Expence inconvenient, He will prepare, inoculate, and attend them through the disease, Gratis."[181]

That this was not an isolated humanitarian charitable gesture is shown by the following description of other charitable inoculations:

"Such being the salutary effects of inoculation . . . To this benevolent and public spirited purpose several excellent charitable institutions, both in London and in the country, are entirely devoted; with this view, also, many opulent individuals have been at great pains to introduce it among their tenants, work people; and the (medical) Faculty have shown such a laudable readiness to contribute the utmost of their assistance to the establishment of the practice, that the poor may, almost every where, have their children inoculated gratis; and have even, in some cases, been assisted with money, clothes, medicines, etc. during the course of the disease."[182]

This type of charitable inoculation occurred as early as the 1740s when a local gentleman paid the 40 shillings per head for some of the poor in the Guildford area. However, a much more important form of inoculation was that provided by parish authorities for their "poor". The first record we have of a mass inoculation being paid for by the overseers of the poor is that which took place in 1756 when a large number of the parish poor were inoculated during the smallpox epidemic at Wootten-under-Edge, a market town in Gloucestershire.[183] As the Webbs have pointed out, the poor were defined so as to include most of the wage-earning population for purposes of medical relief.[184] This is illustrated by the general inoculation which took place in Northwold, Norfolk in 1788:

"It was therefore resolved that a general innoculation of such uninfected persons should take place and as the Major part of such persons were unabel to Defray the necessary Expence of innoculating themselves and their families, it was purposed that the Churchwardens should be Impowered by a future meeting to Borrow a sum, not exceeding thirty pounds, free from the payment of

Interest on the Credit of the town Estate, which was Given among other purposes for Charitabel Uses."[185]

A total of 300 people were inoculated, 226 of which were "inoculated on the Parish Charge". The remaining 74 were paid for by the heads of families who could afford to pay for their own inoculations. According to a list of these 25 heads of families, most of them were farmers and artisans – presumably master artisans trading on their own account.[186] The actual cost of inoculation to the parish and the heads of families was two shillings per person.

The price of inoculating the poor was relatively low as early as 1758 when the parish of Beaminster, Dorset paid 5 shillings per head for 27 of its inhabitants.[187] Similarly, the parish of Rye, Sussex paid the local surgeon Frewer two shillings and sixpence per head for inoculating "329 poor persons" in 1767 – a total sum of £41. 2s. 6d.[188] Most students of overseer of the poor accounts have noted the very large sums of money spent on mass and general inoculations.[189] One of the reasons why parishes were prepared to incur such heavy expenditure was the very heavy alternative cost of having to nurse and sometimes bury smallpox cases. Perhaps an extreme example of this is to be found at Thaxted, Essex in 1717, when it cost the parish £6. 17s. 3d. to feed and nurse a family, "Widow Mallie's having the smallpox".[190] Contemporaries were very aware of the economic advantages of inoculation; the Reverend Stuart described how before the successful general inoculation of 1788, smallpox had cost the parish a great deal, both directly and indirectly:

"For nine years that I have had the living of Luton, the average number of small-pox patients is 25. These, at the lowest computation, stand the parish at two guineas each, exclusive of medical assistance. The disease is so apprehended in the country, that the nurses require double pay; and both they and the patients are confined in an airing-house several weeks after the recovery . . . But, alas! these fifty guineas are but a small part of the real charge and inconvenience produced by this dreadful malady. Its almost constant effect is a permanent augmentation of the parish expenditure. If a labourer dies, his family must be supported. If a mother is lost, the children

must be removed to a workhouse, as their father cannot spare time for employments that are merely domestic."[191]

As inoculation of the parish poor cost "not more than two shillings", Stuart advocated that a "plan of annual inoculations take place."[192] That this kind of heavy parish expenditure on smallpox was typical, is indicated by the study of parish poor accounts. According to E. G. Thomas who has analysed the Essex accounts:

"smallpox was the greatest scourge with which the overseer had to contend, and it was, at the same time, the severest drain on the poor rate entailing expensive nursing charges and costs attendant on the isolation of the victims. References are made to the disease in almost every account book."[193]

This type of expenditure was obviously an incentive to parish authorities to inoculate their poor, at least when it had become sufficiently cheap by the 1750s. The price of inoculation paid for by the parish was rarely greater than five shillings or less than two shillings during the latter half of the eighteenth century.

Although the price of inoculation was relatively low during this period, many parishes were reluctant to pay for the inoculation of their poor. Dimsdale described in 1776 the variations from parish to parish in Hertfordshire:

"in the county of Hertford, there have been two methods of public or general inoculation; one to inoculate, at a low price, as many inhabitants of any small town or village, as could be persuaded to submit to it, and at the same time were able to pay, refusing all those who had it not in their power to procure the money demanded. The other method has been, where the inhabitants of a town, or a district, of all denominations, have agreed to be inoculated at the same time, the parish officers or some neighbouring charitably disposed persons, having first promised to defray the expense, and provide subsistence for such of the poor, as are unable to pay for themselves."[194]

The reason for the reluctance of some parishes to pay for the inoculation of their poor was discussed by Dimsdale, and the following lengthy quotation reveals in a humorously macabre fashion the basic attitude of some parish authorities towards the whole question:

"But such is the obstinacy of some parishes, and the parsimony of others, that it is impossible for the poor who are, desirous of being inoculated, to persuade them to advance the small sum that would be necessary to defray the expense; and they are therefore obliged to wait the event of the natural disease, while the principal inhabitants are securing their own families by Inoculation. Another unjustifiable piece of frugality that deserves attention and to be remedied is, that in many places where the whole number of poor have been inoculated at the expence of the parish, illiterate fellows, totally unacquainted with diseases or remedies, have been employed on account of cheapness only, when at the same time the families of the wealthy have been under the care of medical gentlemen of good reputations . . . The inhabitants of a certain parish had a meeting to agree on inoculating all the poor, some medical gentlemen in the neighbourhood offered to undertake the business at a very low price; but as cheapness was the only object of consideration, the parish was about to agree with a blacksmith at eighteen pence a head, when one of the most frugal stated this objection: 'It is very probable that under this man's care we may have some die, and the expence of their burial may cost the parish so much, that it might as well agree with a better man.' This objection was thus removed by the smith: 'Come, I'll tell you what I'll do with you – Give me half a crown a head, and them that die I will carry to the Churchyard without putting the parish to any further expence.' "[195]

A very similar and macabrely humorous experience occurred to Edward Jenner in 1800; he had offered to gratuitously vaccinate the poor of a neighbouring parish to Cheltenham, which was refused until:

"The cost of coffins for those who were cut off by smallpox proved burdensome to the parish; the churchwardens, therefore, moved by this argument effectually exerted their authority and compelled the people to avail themselves of Dr Jenner's kind offer."[196]

Economic considerations were obviously of primary importance in determining the attitudes of parish authorities towards the inoculation of the poor. Such a strict parsimonious attitude illustrated in the above accounts inevitably led to the realisation that it was cheaper to inoculate the poor than to nurse, feed, isolate and sometimes bury them after they had caught natural smallpox. The high cost of such a parish responsibility has already been indicated, which may be further illustrated by the expenditure of the parish of Castle Combe, Wiltshire in 1758 as the result of a smallpox epidemic: the total expenditure on the poor was £141, which was more than double the usual average.[197] It paid such a parish to inoculate its poor rather than pay the expenses associated with a natural epidemic – the parish could have inoculated 560 people for the sum of £70, assuming that each inoculation cost two shillings and sixpence per head, and it is unlikely that the number needing inoculation was as high as 560. Not all poor would have to be paid for by the parish, as sometimes employers paid for the inoculation of their servants, in order to minimize the danger to their own families – advertisements requiring servants to have been inoculated before they could be employed, were common throughout the eighteenth century.

Dimsdale in his account of inoculation in Hertfordshire mentioned large numbers of amateur inoculators who were practising during the period (1776). Although he adopted a very critical attitude towards them, he had to admit:

"that many instances can be produced, where whole parishes of poor have been inoculated, and have succeeded very well, under the care of persons who were totally unacquainted with medicine. I will not here dispute the truth of this assertion."[198]

The amateur inoculators were important in both reducing the price of inoculation and making it available to that section of the population

who could not obtain it through their parish. In fact from the very beginning of the practice of inoculation in England in the 1720s, it was carried out by people outside the medical profession. Some amateurs were practising as itinerant inoculators by the early 1760s, for Dr Thomas Glass described at the end of 1766 how "four or five years since I was desired to visit a Girl, who had been inoculated, with thirteen or fourteen other persons, at a farm-house in the neighbourhood of Honiton [in Devon], by an itinerant Operator."[199] The practice of inoculation by amateurs seems to have accelerated with the simplification of method and technique, particularly that associated with the Suttons. For example, the resident surgeon of the Foundling Hospital in London wrote in 1768:

"Very great success has likewise attended inoculation in many parts of this kingdom: even though it has of late descended into very illiterate hands (a livery servant, belonging to a friend of the author's left his master's service, not a great while since, to practice inoculation)."[200]

This was the time when the success of the Suttons led to the practice of inoculation by "pretenders and itinerants" described by Houlton.

Although amateurs practised inoculation cheaper than the professionals, they were still concerned with the profitability of the practice, and even the blacksmith in the Hertfordshire parish involved in the dispute over costs was asking for a minimum of one shilling and sixpence per head. It is possible that these amateur inoculators made a smaller charge to the ordinary poor when they had to pay for themselves, although there is no evidence on this. One way of having inoculation without paying for it was mentioned by William Buchan in the 1769 edition of his *Domestic Medicine:*

"Should all other methods fail, we would recommend it to parents to perform the operation themselves . . . I have known many instances even of mothers performing the operation."[201]

As late as 1824, a member of the medical profession could report from Canterbury that "the most zealous inoculators were females – often the parents themselves – .frequently officious friends . . ."[202] Four years earlier, Cross had reported that of the many people operating in East Anglia, "the greatest inoculators were the parents of poor children, farriers, blacksmiths, tailors, shoemakers, and old women."[203] Clearly, many parents had taken Buchan's advice, particularly amongst the poor, although obviously other kinds of amateur inoculators flourished well into the nineteenth century.

The opposition to inoculation on religious grounds seems to have diminished relatively rapidly. In 1753 a Chelmsford surgeon noted:

"As to religious objections they are almost given up as 'tis high time they should (except amongst a few bigots indeed) . . . the learned bishop of Worcester's sermons one wou'd think sufficient to remove all kind of objections, religious as well as other, with all reasonable people."[204]

This diminution of religious opposition to inoculation was not confined to the educated, for the same Chelmsford surgeon observed:

"This universal good is inoculation, and notwithstanding envy has laid such batteries against it, yet happy for this kingdom it gains ground daily; the lower class of people coming into it very fast in these parts."[205]

Opposition to inoculation on religious grounds never entirely disappeared at any time during the eighteenth century; for example, William Buchan claimed in 1769 that "the first step towards rendering the practice universal must be to remove the religious prejudices against it."[206] However, religious opposition was not strong enough in itself to retard the spread of inoculation, as is illustrated in the following account of conflict between a Congregational Pastor and his Hitchin, Hertfordshire congregation:

"It was in 1771, a year when the smallpox raged. They were burying townsmen that summer by the score, and throughout the county also, insomuch that Dimsdale, the famous inoculator, opened an inoculating house at Hertford under his own supervision. Foreseeing what was going to happen, Hickman warned his people that 'inoculation was a kind of presuming upon Providence', and that he should refuse to pray for anyone who had recourse to it. In terror for their lives, the richer brethren resolved to trust to the practice of a clever Quaker inoculator [Dimsdale] rather than to the preaching of their solid Independent pastor. They remembered that his wife had died of the small-pox, in spite of all his prayers, only two years before. They made the journey to Hertford and were saved. The poorer members, who could not afford to go to Hertford, had perforce to stay at home and trust in Providence, and several of them died. Unfortunately for Hickman it was the richer sort he had to look to for his stipend, and they were not minded to pay for a minister who would not pray for them. There was nothing for it, therefore, but [for Hickman] to shut down his school and look for better treatment in another part of the country."[207]

It was because the congregation were in "terror of their lives" that they dispensed with the services of their minister, along with his traditional religious beliefs. The most effective demonstration of the decline of religious opposition to inoculation is found in the actual proportions of populations inoculated by the end of the eighteenth century – a topic to be treated at length in the next chapter. Most of the evidence for the persistence of religious opposition to inoculation concerns large towns; for example, Turner writing about Newcastle in 1792 noted:

"there still subsists, especially amongst the lower classes, a great and general prejudice against the practice of inoculation; and some of the most popular grounds of this prejudice have obtained a degree of credit by claiming to be found in religion."[208]

Similarly, one medical observer noted in 1786 with reference to Edinburgh that

"although among the higher ranks inoculation is now become universal, yet among the vulgar, from ill-grounded prejudices, and even from religious tenets, it has made very little progress; notwithstanding the earnest admonitions, and gratuitous assistance of medical practitioners."[209]

Opposition to inoculation on religious grounds never entirely died out, and even when the new vaccination was introduced at the beginning of the nineteenth century, there were minor problems on this account. For example, Thomas Warren, curate to the parish of Flamstead and Kensworth in Hertfordshire, wrote as late as 1803 that there were "some people . . . who object to Inoculation altogether, thinking it sinful and presumptious willingly to incur a disease."[210]

Much of the religious opposition that remained however appears to have been linked to attitudes of fatalistic resignation, which flourished particularly in large towns. Haygarth came nearest to explaining this link. He noted that in Chester

"the lower class of people have no fear of the casual [natural] smallpox. Many more examples occurred of their wishes and endeavour to catch the infection, than to avoid it."[211]

Haygarth tried to account for this fatalistic attitude of parents towards their children catching smallpox, and wrote:

"This . . . prejudice . . . probably prevails in other towns, especially in those which are so large as perpetually to nourish the distemper, by so quick a succession of infants as constantly to supply fresh subjects for infection . . . [whereas] . . . in small towns and villages, especially placed in remote situations, the young generation grow up to have a consciousness of the danger before they are attacked by the dreadful disease."[212]

The implication of Haygarth's argument is that the endemic nature of smallpox in the large towns engendered an attitude of fatalistic resignation amongst parents as a result of the inevitable and regular, year-by-year returns of epidemics, whereas in the countryside relatively infrequent epidemics produced a much greater consciousness of the devastations of the disease. There is no logical reason why this should be the case, except that countryside epidemics were much more spectacular than those in the large towns, mainly because a much larger proportion of the total population, including adolescents and young adults, were attacked. The important role of the spectacular nature of countryside epidemics in encouraging inoculation is illustrated by the response to a smallpox epidemic in the Chelmsford area. In 1779 a local surgeon described the practice of inoculation in the locality:

"it has been neglected by the common people for the last 7 or 8 years. It seems as much forgot in many parts of the kingdom as though it had never been known, until the natural small-pox comes with its usual train of malignant disorder and awakens them out of their lethargy. The Faculty, then are hurried into inoculation, perhaps, with too much precipitary, and are under the necessity of complying with the impatience of the people without proper preparations . . ."[213]

This impatience for quick inoculation in response to a threatening epidemic sometimes led to a kind of panic, as in Blandford in 1766 when "a perfect rage for inoculation seized the whole town."[214] The same type of experience was repeated in Hertfordshire in 1770 when an epidemic threatened, for according to Dimsdale, "the poor in my neighbourhood flocked in numbers, beseeching me to" inoculate them.[215]

Although the poor in large towns did not enthusiastically embrace inoculation as they did in the countryside (at least during periods of threatening epidemics), it would be misleading to assume that the town poor were indifferent to the fate of their children as the result of religious or fatalistic resignation. Haygarth himself described a mother in Chester who refused to have her child

inoculated, on the grounds that "four of my children have already died of the common [natural] smallpox, and if my remaining child should die by inoculation, I could never forgive myself."[216] This does not indicate a fatalistic resignation so much (and certainly not an "endeavour to catch the infection"), but more a psychological unfamiliarity with the protection given by inoculation. John Franks wrote in 1800 of the London poor:

"when smallpox is in a house where there are many children and adults liable to the disease, the proposal to inoculate gratuitously, all those who are not exempt, is too often disregarded by themselves or relations. It is in vain that we expostulate in these situations, and endeavours to convince them of the non-existence of a double infection [that inoculated children would later catch smallpox], or of an accumulation of disease; for the contrary opinion is too firmly impressed to be easily obliterated."[217]

The problem was to familiarize the urban poor with the benefits of inoculation, and although we shall see later this was more or less achieved at the end of the eighteenth and the beginning of the nineteenth centuries, it had occurred much more quickly and effectively in the countryside. The benefits of inoculation were spectacularly obvious in the small towns and villages where everybody could follow the exact course of an epidemic and gain a very personal knowledge of the protective power of Inoculation. A writer to *The Monthly Ledger* explained in 1775 the difference between the countryside and large towns in a discussion on inoculation:

"But those who know most of the country know that it is a place where things cannot be secreted, a transaction at ten miles distance is more talked of than a transaction at two streets distance in London."[218]

The medical profession began to make strenuous efforts to practice inoculation in the large towns only towards the end of the eighteenth century. Although the London Smallpox Hospital was founded in 1746 to provide a certain number of charitable

inoculations, most other large towns made no charitable provision until as late as the 1780s. The vast majority of the population lived outside of the large towns during the eighteenth century, and the gradual spread of inoculation in these places will be discussed in the next chapter. As we have seen, the medical profession itself had been originally divided over the subject of inoculation as Mead had noted in 1747,[219] but eight years later, Hosty, who had come to England to study inoculation, reported in 1755:

"Je n'ai pu trouver dans tout Londres un seul Medecin, Chirurgien ou Apoticaire qui s'opposat l'inoculation, ils en sont au contraire tellement partisans qu'ils font tous inoculer leurs propres enfants. Its regardent cette pratique comme la plus grande decourverte que l'on alt en Medecine depuis Hippocrate."[220]

Similarly at about the same time the College of Physicians unanimously approved a formal statement which concluded that inoculation "is at present more generally esteemed and Practised in England than ever, and that they Judge it to be a Practice of the utmost benefit to Mankind."[221]

The remaining factor checking the practice of inoculation was the fear that it would spread smallpox to unprotected people. This invariably led to certain forms of prohibition of the practice – exceptions were made for inoculations in isolated houses – except when an epidemic threatened. Although general inoculation was encouraged when an epidemic threatened, inoculation was frequently discouraged immediately afterwards. The parish of Beaminster near Taunton paid for the inoculation of 379 of its poor in 1791, but then directed that

"from this time Inoculation shall cease in the Town, and If any Surgeon or Apothecary resident in the Town shall set in defiance this Resolution – We shall consider him an improper Person to have Care of the poor at any future Time."[222]

Parish authorities were still frightened that those inoculated would spread the disease to unprotected cases, but they obviously had much less

to fear when all the previously unprotected population had been inoculated. This led to what was known as general inoculation, which will be discussed in a separate chapter. It is sufficient to note here that the fear that inoculation would spread smallpox frightened some parish authorities into compelling members of their parish to be inoculated, in order to eliminate the possibility of spreading smallpox. For example, Cowper the poet wrote in 1788:

"the smallpox has done, I believe, all that it has to do at Weston. Old folks, and even women with child, have been inoculated . . . No circumstances whatsoever permitted to exempt the inhabitants of Weston. The old, as well as the young, and the pregnant, as well as they who had themselves within them, have been inoculated . . ."[223]

CHAPTER 4
Growth In The Practice Of Inoculation

We have seen in the last chapter that inoculation "lost ground" in the 1730s, and before about 1740 was practised only on an insignificant scale. According to the medical historian Moore, writing in 1815, "the American reports [of a more successful form of inoculation] were so encouraging that about the year 1740 the practice was revived by a few surgeons in Portsmouth, Chichester, Guildford, Petersfield and Winchester, and gradually extended in the Southern Counties."[224] The American reports that Moore referred to were the large number of inoculations carried out by Mowbray and Kilpatrick during the 1738 epidemic in Charleston. According to Kilpatrick 800 people were inoculated, of whom eight died, a fatality rate of one per cent. This lower fatality was probably partly due to the improved technique of inoculation associated with the names of Sloane and Ranby; Kilpatrick later claimed in 1754 that an essay of his written in 1743 on the improved technique of inoculation "had been of some effectual tendency to revive the practice."[225] It has recently been argued by one medical historian that Kilpatrick was prone to self-advertisement, leading him to exaggerate his role in the revival of inoculation in England.[226] It is clear, however, that he contributed to the spread of inoculation through publicising its relative success in Charleston.

Inoculation during this period was almost entirely restricted to the rich; the inoculation of the poor near Guildford during the 1740s paid for by "a noble person" was an exception, although it contained the seeds of popular inoculation for the poor:

"Country people came every market day to have the operation performed, then went home, kept themselves warm, drank whine whey, and in eight days took the distemper; and so much success attended the practice, that it was answer to their acquaintance, of 3 or 4 hurrying along the town together, that they were going to be *oculated.*"[227]

The inoculation of the poor and the general population became increasingly widespread during the 1750s. We have already noted the surgeon from Chelmsford who wrote in 1753 that "the lower class of people [are] coming into it [inoculation] very fast in these parts."[228] Similarly, Kilpatrick described the beginning of popular inoculation in his book published in 1754, although as a respectable member of the medical profession he was somewhat appalled by this development:

"But since we have certain Accounts that the Populace, who were at first strongly prepossessed against this Practice, and who so rarely stop at the Golden Mean, are rushing into the contrary Extreme, and go promiscuously from different Distances to little Market Towns, where, without any medical Advice, and very little Consideration, they procure Inoculation from some Operator, too often as crude and thoughtless as themselves; congratulating each other after it over strong Liquor, and returning immediately to their ordinary Labour and Way of living . . ."[229]

The operators that Kilpatrick referred to were probably country surgeons and apothecaries, for he discusses them in very critical terms with reference to their role in inoculation; some of these "operators" may also have been amateurs.

Although Kilpatrick accused the populace of "rushing into the contrary Extreme", he himself probably greatly exaggerated the practice of popular inoculation during this period. As late as 1751 the *Gentleman's Magazine* could refer to inoculation as "this new fashionable operation",[230] and it has already been noted how another gentleman observed in 1752 that the "poor in general are absolutely cut off from all share in it . . . and not only the very poor people, but multitudes of others, many farmers and tradesmen . . ."[231] It is partly possible to assess the extent of inoculation by analysing the overseers-of-the-poor's accounts and contemporary descriptions of particular local inoculations. During a smallpox epidemic at Bradford-on-Avon, Wiltshire in 1752/53, 1,456 people caught natural smallpox while another 127 were inoculated – a small proportion of the total population at risk [232] Similarly, during an epidemic at

Salisbury in 1753, there were 1,309 cases of natural smallpox and 422 inoculated cases, a somewhat larger minority.[233]

At Blandford, Dorset, in 1753 there were 309 people inoculated, while only 40 actually died from smallpox. It is not known how many people were at risk of catching the disease, and therefore needed the protection of inoculation, although at the beginning of a similar epidemic 13 years later (in 1766) it was estimated that 700 persons in the town were at risk. As 44 people died from smallpox during the 1766 epidemic and 384 were inoculated, it is reasonable to assume that under 700 people were at risk in 1753, and therefore about a half of the vulnerable population were inoculated in that year.[234] This was a significant proportion and perhaps another example of the same kind is to be found at Wootton-under-Edge, Gloucestershire, in 1756 when 336 "paupers" were inoculated,[235] although we do not have sufficient information to estimate the proportion of the population at risk who were protected in this way.

The analysis of overseers-of-the-poor's accounts is somewhat misleading in arriving at conclusions about the extent of inoculation during the 1750s, as many parishes did not pay for the inoculation of their poor until well after this period. For example, in Rye, Sussex "this system [of mass inoculation paid for by the parish] was first introduced in 1767", when 329 poor persons were inoculated.[236] We have seen earlier that as late as 1776, Dimsdale stated that some parishes in Hertfordshire refused to pay for the inoculation of those in the parish who could not afford to pay for themselves. Although Kilpatrick probably exaggerated the extent of the practice of inoculation in the 1750s, there was undoubtedly a change in the attitude of the general population towards inoculation from before 1749 when it "gained but little credit among the common sort of people, who began to dispute about the lawfulness of propagating diseases, and whether or not the smallpox produced by inoculation would be a certain security against taking it by infection",[237] to the early 1750s, when they rushed "into the contrary Extreme".

It has been previously indicated that inoculation did not become really popular until after the Suttons introduced their improved method of inoculation in the 1760s. This conclusion is confirmed by a statistical estimate of the number of people inoculated in England up

until 1766, which according to the medical historian Klebs was 200,000 persons.[238] However this estimate was arrived at, it is unlikely to be very accurate, as some inoculation was performed by amateurs and others who must have been very difficult to include in any systematic survey. Evidence of a more reliable kind is provided in Andrew's monograph on inoculation published in 1765:

"Tho' Inoculation has been introduced into Exeter, and the County of Devon, more than twenty three Years . . . it is still sparingly practised . . . In this City [Exeter], according to the best Calculations I can make, since the Year 1741 there have been about 700 Persons inoculated . . ."[239]

Similarly, a correspondent wrote in the *Monthly Ledger* in 1765:

"I have been witness to the progress of inoculation, from the introduction of the Suttonian method, thro' a very considerable part of a populous country [Hertfordshire]: at the introduction of that method, the subjects obnoxious to the disease [i.e. at risk and not protected by inoculation] were more numerous in proportion to the example, than they could possibly be in London at any period."[240]

This correspondent was discussing whether inoculation spread smallpox or not, and used the favourable experience of inoculation in Hertfordshire during 1766-67 as evidence that a similar form of popular inoculation could be used safely in London. Inoculation only really became popular in Hertfordshire with the introduction of the Suttonian method. This was discussed by our correspondent as a part of his general argument:

". . . could the prejudiced surmount their prejudices, and the poor surmount their poverty, and inoculation become as general, throughout London, as it was in Hertfordshire, in 1766 and 1767, the article of smallpox, in the first succeeding year's bill of mortality, would, instead of increasing, sink to 100, and, in a year or two more, to less than twenty."[241]

Contemporaries were unanimous that the Suttons had introduced a method of inoculation that quickly became extremely popular. Woodville in his *History of Inoculation* published in 1796, described the impact of the new method as follows:

"A new era in the history of inoculation had now taken place, by the introduction of the Suttonian practice, which in the year 1765 had extended so rapidly in the counties of Essex and Kent as to much interest the public, who were not less surprised by the novel manner in which it was conducted, than by the uninterrupted success with which it was attended upon a prodigious number of persons."[242]

The success of the Suttonian method enhanced the reputation of inoculation in general and accelerated its practice. This is illustrated by the response to the epidemic in Blandford, Dorset in 1766, when "the general success of Inoculation, in other places, had so prejudiced the minds of the people in its favour, that they were perfectly careless and secure about the consequences."[243] This new popularity of variolation spread to all parts of the country, both south and north, as is illustrated by the example of the surrounding countryside of Leeds in 1768, reported in the "Country News" of a Leeds newspaper:

"Inoculation is now in such universal repute that it is thought there are not less than 10,000 people under the care of practitioners in this part of the world. Many farmers and their families have undergone the operation, and there is scarcely an instance of its failing."[244]

This popularity of inoculation was not confined to the wealthy and the middle-class, but also extended to the labouring part of the population. This was reflected in the (somewhat amusing) complaint of the author of a pamphlet on *The Dearness of Corn and Provisions* published in 1767:

"Inoculation for the small-pox has so very much prevailed in the country, that thousands and ten thousands have escaped the fatal effects of that distemper in the natural way: but what are the

consequences of so good an invention? No sooner are the lower sort recovered, but they aim (the women especially) to get a servitude in London, or to use their own words to better themselves; this is the only objection that can be made to inoculation, and indeed it is one, for before they did not dare to quit the place of their birth for fear of that distemper, so remained honest and useful in the country . . ."[245]

Smallpox was endemic in London, a fact which no doubt did stop people from the country migrating there if they had not previously had smallpox or been inoculated. The important point here, however, is that "the lower sort" were being inoculated in large numbers at about this time. Even as far afield as the Shetland Islands, the Suttonian innovation significantly transformed the practice of inoculation. In 1761 only ten to twelve people had been inoculated, but during the next smallpox epidemic of 1769/1770, a local surgeon "inoculated several hundred, chiefly of the lower class,"[246] after which date inoculation was "performed by a great number of native doctors"[247] including the locally renowned John Williamson.

In order to convey the nature of the transformation brought about by the Suttons, I will quote at length from a series of letters written by a Mr Thomas Davies, who was bailiff to the Glynde estate in Sussex, to his employer's agent residing in London. These letters give a vivid sense of the popular interest and excitement created by the new mass practice of inoculation, and show that the Suttons had many rivals by the year 1767 (when the letters were written), and that these rivals were often cheaper and sometimes even more effective as inoculators. The following were all written from Glynde, a very small village, at a time when smallpox had just begun to affect neighbouring areas:

"28 Feb. 1767 . . . Even those who have had it [smallpox] themselves, as we expect to be so surrounded with it soon, don't know but they may be a means of bringing home to their families, which is my case. This danger together with the great Success and liberty of moving about and freedom from even Sickness, in the new method, to all Ages and Constitutions, made me wish I could persuade our little Parish to do as Tunbridge Wells and Ryegate and such

places have done i.e. to inoculate all in order to be clear of it in about a fortnight or three weeks . . . 18 March . . . yesterday an Agreement was made with Mr Watson & Co . . . who have inoculated above 2,000 people this winter about Rye, Winchelsea, Romney and the East of Sussex, with equal Success but less Physicking and more expedition than Sutton or his people. His method is, to innoculate without previous preparation; and physick afterwards as occasion requires . . . The Terms he offered to inoculate us I think is reasonable enough, as he was very desirous of making an Attack on Sutton who innoculated at the Park House and environ, i.e. about the Broyle, little Horstead etc [in the neighbourhood of Glynde]. He undertook as many as would be innoculated of Glynd people for 20 Guineas and if there were not 40 people in all he would not insist on so much . . . I should think it probable we shall have more unless terrified by the Nonsense of our Neighbours. 19 March . . . This day Blackman of Southover came here to hear our Terms, he talked of about 300 in their parish that have not had it and Watson offered to inoculate them all for £100. He supposes they will comply. This will spoil Sutton's Trade in the Pleshut House who takes in none under 6 Guineas and 4 Guineas where the lowest price people are crowded 2 In a Bed and 8 Beds in a room. They clear there at present at the rate of 100 Guineas a Week besides other parties, so that it is high time to pull down their prices; or else they would run away with all the Cash of the Country . . . 14 April . . . There are at least a Score of Inoculating Doctors advertising every week in the Lewes Journal, all in the newest Fashion, and I believe as far as I can hear, all with the same Success. For if but one should happen to die, all the County would soon hear of it. Our Doctor is above advertising and has not once appeared in print. I believe him to be as good as any of them, Sutton & Co not excepted, and I know he is by much more expeditious."[248]

The small parish went on to successfully inoculate its forty or so vulnerable parishioners – the fears of their neighbouring rival parish of Firle that inoculation might spread the natural form of the disease were countered by isolating the inoculated in a special inoculation stable – and one of the many rivals to the Sutton family proved that he was more than their match in inoculating

skills. (Davies quite correctly realised the advantages of no preparation and minimal "physicking"). We see in Davies's account that the fear that inoculation would spread smallpox was a major incentive for a general inoculation of all vulnerable members of a community – a theme to be discussed in the next chapter – and that this had become a common practice in Sussex by 1767. The Suttons' prices quoted by Davies were the ones charged for private patients undergoing inoculation in one of their special inoculation houses, but as we will now see, the Suttons were forced by the ruthless market forces revealed by Davies, to significantly lower their prices particularly to the parish poor.

Although Robert Sutton had perfected a much safer technique of inoculation at the beginning of 1762, it was his son Daniel who was responsible for its popularisation. Not only did he inoculate 417 of the "poor" of Maldon, Essex in one day, as well as 70 of the "tradespeople and gentry", to clear the town of smallpox in 1764, but also

"Several other large parties in Kent, and in various parts of the kingdom have been inoculated in the same manner (as at Maldon), and with the same success."[249]

The numbers inoculated by Daniel Sutton accelerated rapidly during this period; he inoculated 1,629 in 1764, 4,347 in 1765, and 7,613 in 1766. In addition, about 6,000 inoculations were carried out by his assistants who had been "taught . . . his method".[250] Sutton's own number of inoculations were taken from his record books, but the scale of his activity is confirmed by independent evidence from other sources. We have already seen the success of his mass inoculation at Ewell in Surrey in 1766, and similar feats were performed at Maldon in Essex and elsewhere. Sutton quoted the following example in his book written in 1796:

"About ten or fifteen years after I had introduced and established the new method of inoculation, I was employed to inoculate a large party, of the same town, consisting of above 700 persons. About one half of them were inoculated before twelve o'clock, and the other half were begun upon, at half past three in the afternoon: They

were all inoculated by my own hand, from the same individual subject . . . the medicines were procured from the same druggist . . ."[251]

These mass inoculations performed in one day usually took place amongst the ordinary population, with wealthier parishioners insisting on the more expensive and medically orthodox period of preparation. Although Sutton performed all the inoculations himself in the example quoted above, it is clear that he employed assistants to do much of the work for him. According to Woodville, Sutton's "practice in Kent [before 1767] being also very extensive, he was under the necessity of employing several medical assistants."[252] By 1796, Sutton could claim to have been involved in "of near 100,000 instances of inoculation, in which I have been either immediately employed, or have had some concern, in consultation with others."[253]

There is, however, some ambiguity about who these other inoculators were. The Sutton family set up a series of partnerships in all parts of the country. Houlton described in 1768 how this came about:

"Every paper throughout the kingdom echoed with its [the Suttonian method's] success. Eminent physicians and surgeons were daily applying to the family to be appointed partners for particular counties, or for foreign parts. Connections have been made with many gentlemen of the faculty, while others continue still to apply for that purpose."[254]

He then listed all members of the Sutton family (six sons and two sons-in-law of Robert Sutton) and partners who were "authorised" members of the enterprise at the time of publication in 1768:[255]

Mr Robert Sutton senior	Framingham Earl, Norfolk
Mr Robert Sutton junior	Paris
Joseph Power M.D. partner	Paris
Mr Daniel Sutton	London
Mr William Sutton	London

Mr Peale partner	London
Mr Joseph Sutton	Oxford
Mr Gamble partner	Oxford
Mr Thomas Sutton	Newport, Isle of Wight
Mr James Sutton	Wakefield, Yorkshire
Mr Hewitt, son-in-law	Hague
Alex Sutherland M.D., partner	Hague
Mr Shuttleworth, son-in-law	Birmingham
Mr Robard	Bristol
Mr Ford	Bristol
Mr Ludlow	Bristol
Mr Read	Gloucester
Mr Vaux	Hereford
Mr Vaux Junior	Worcester
Mr Tatum	Salisbury, Wiltshire
Mr Wick	Salisbury, Wiltshire
Mr Jones	Bradford, Wiltshire
Mr Marsh	Highworth, Wiltshire
Mr Smith	Winchester, Hampshire
Mr Jones	Portsmouth, Hampshire
Mr Sampson	Sherborne, Dorset
Mr Jay	Maiden Newton, Dorset
Mr Assey	Taunton, Somerset
Mr Bromley	Exeter, Devon
Mr Hooper	Exeter, Devon
Mr Campble	Truro, Cornwall
Mr Steed	Ingatestone, Essex
Mr Buck	Ipswich, Suffolk
Mr Argles	Wisbech, Cambridgeshire
Mr Byre	Chertsy, Surrey
Mr Newland	Guildford, Surrey
Mr Kerr	Dorking, Surrey
Mr Barnwell	Sussex

Mr Levet	Buckingham
Mr Saunders	Buckingham
Mr Dent	Buckinghamshire
Mr Terriers	Buckinghamshire
Mr Bond	Northamptonshire
Mr Richardson	Huntingdonshire
Mr Bevil	Manchester
Mr Goodwin	Liverpool
Mr Lynn	Shrewsbury, Shropshire
John Denman M.D.	Bakewell, Derbyshire
Thomas Rutherford M.D.	Durham
Mr Lyde	Brecknockshire, Wales
Mr Bevan	Glamorganshire, Wales
Mr Houlton	Dublin, Ireland
Mr Blake	Dublin, Ireland
Mr Sparrow	Dublin, Ireland
John Harley M.D.	Cork, Ireland
John Morgan M.D.	Straband, Tyrone Ireland
Mr Vachell	Soon to be appointed to a particular district in Ireland
Mr Ward	Soon to be appointed to particular district in Ireland
Mr Shields	Soon to be appointed to particular district in Ireland
Mr Arnold	Soon to be appointed to particular district in Ireland
Mr Jewitt	Jamaica
Mr Smith	Virginia

The Suttons and their partners were to be found in most counties of England at this time. This laid the foundation for the almost universal practice of the Suttonian method of inoculation in England, so that a correspondent to the *Gentleman's Magazine* in 1796 could write:

"It is now 30 years since the Suttons and others under their instruction, had practised the art of inoculation upon half the kingdom . . ."[256]

It was for this reason that inoculation was referred to as the Suttonian system during a Parliamentary debate in 1808. In 1806 Lipscomb claimed that "Mr Dan. Sutton and his brothers . . . are still living to prove that they have inoculated more than five hundred thousand persons with uniform success."[257] It is impossible to be sure whether this claimed number of inoculations included those performed by partners as well as actual members of the Sutton family, although this is unlikely given the spread of even more popular forms of inoculation which would have made partnership with the Suttons (and therefore the use of their name) increasingly irrelevant. The relative decline in the fortunes of the Sutton family can be traced in the history of Daniel's practice. According to Woodville,

"In 1767, Mr D. Sutton removed to London, where he hoped to profit by his profession still more than he had done in the country; but his practice fell far short of his expectations; and the two houses, one at Kensington Gore, and another at Brentford, which were procured for his inoculated patients, were soon abandoned."[258]

This suggests that Sutton failed as an inoculator in London, and his eclipse from the fame he acquired during the 1760s is reflected in a pathetic advertisement that he put in the front of his book in 1796:

"I find it has been circulated, That I am not the person who introduced the New System of Inoculation . . . that for many years I had quitted my profession, and was long since dead."[259]

The main reason for this decline I have discussed previously, i.e. Sutton's unwillingness to abandon preparation and associated medical treatment, and the relatively high cost of his practice. He was displaced by more popular inoculators who simplified the method of inoculation to its logical conclusion, and reduced its price. However there was another reason why he did not succeed in establishing a fashionable practice in London:

"The terms of Sutton are so moderate that men in mean circumstances, men of low education and dissolute life, repair to his house, which is so confused and disorderly a place that one would admire one-tenth part of his patients do not perish by their irregularities."[260]

We do not have to take this elegant disdain too literally to recognize that Sutton was no longer attracting the wealthier type of client. The Suttons had obviously made a great deal of money out of inoculation – Daniel Sutton is reputed to have made 6,000 guineas between 1763 and 1766[261] – and they appear to have sold the "secret" of their method to their partners for between fifty and one hundred pounds, or a half share in the profits.[262] He seems to have missed his chance of lasting fame and wealth when he refused to go to Russia to inoculate Empress Catherine II. Dimsdale, who successfully performed the operation, was made a Baron of the Russian Empire, awarded a sum of £10,000 and an annuity of £500.[263] The reasons for Sutton's refusal are unknown; he may have been frightened of the consequences of failure with someone of such eminent status. It is clear he very soon concentrated on the inoculation of the ordinary population as was reflected in the following advertisement.

"Sutton-House, London, Jan. 20, 1770. The many thousands of industrious poor, who have past happily through the Small-Pox by Inoculation, under the direction of Mr Daniel Sutton . . . induce him to offer to the public, the following more extensive plan . . . Convenient houses, in different parts of the Town (each being inhabited by a reputable Surgeon or Apothecary, instructed by him) are engaged – that to these houses he proposes such patients as have tickets of recommendation from subscribers, are to repair on the day and hour appointed in the said tickets, in order to receive preparatory medicines and instructions, they will be informed, when to return to be inoculated . . . when they will finally receive such medicines and ample directions for their conduct, during the progress of their disease, at their own habitations, as will render any further attendance unnecessary . . . This plan is principally intended for the

benefit of the industrious poor; such as the families of artificers, handicraftsmen, servants, labourers, etc."[264]

It is not known how successful this plan was, although clearly it must have had only limited success for Sutton's relative eclipse by 1796, although he was still practising by then. However, we cannot assume that the plan was a total failure, for according to Gorton's *Biographical Dictionary*, "Daniel simplified and improved his father' mode of practice, and settled first at Ingatestone, Essex, and afterwards in London, where he was very successful."[265] If we take Sutton's own claims to the numbers inoculated by him and his assistants, it is possible to trace the change in extent of his practice. Between 1763 and 1766 he claimed to have inoculated about 20,000 people and by 1796 a total of 100,000. During the first period he was inoculating about 6,700 people a year, whereas between 1766 and 1796 the average number was about 2,700. It is unlikely that his London plan could have succeeded on any extensive scale, owing to the expense and inconvenience involved for the poor, as well as the necessity of being recommended by a subscriber. In this respect the plan was similar to that of the London Smallpox Hospital, which was only a limited success for the same reasons. Sutton, of course, did not confine his activities to London as was demonstrated by his inoculation of "a large party, of the same town, consisting of above 700 persons" at some time during the late 1770s. Whatever the fate of Daniel Sutton's individual practice, it is clear that the Sutton family as a whole, along with their partners, continued to practice inoculation on an extensive scale until at least the end of the eighteenth century. There is evidence for this conclusion independent of the Sutton family itself; for example, Fewster, a surgeon in Thornbury, Gloucestershire, wrote in 1798:

"The late Mr Grove was a very extensive Smallpox inoculator, frequently having 200 to 300 patients at one time . . . Mr Grove and myself formed a connection with Mr Sutton, the celebrated inoculator . . ."[266]

It is not clear whether this type of partner was included by the Suttons in their claims about the numbers inoculated by them, the wording of their statements often being ambiguous. For example, Robert Sutton claimed in an advertisement published in 1777 "that in the SUTTONIAN PRACTICE, which has been established nearly thirty years . . . nearly three hundred thousand persons have happily passed through smallpox [i.e. been inoculated] . . ."[267]

In 1763, Houlton stated that the Suttons had inoculated about fifty-five thousand people (with only six deaths),[268] and we have previously seen that the Suttons claimed in 1806 to have inoculated more than five hundred thousand persons. If these statistics were reliable and referred to the same type of practitioner (i.e. either just the Suttons or the Sutton family plus partners throughout), they would indicate a decline in the extent of the practice after 1777. The average number of yearly inoculations according to these figures was about 27,200 between 1768 and 1777, and approximately 6,900 between 1777 and 1806. It is unlikely however that these statistics do refer to all the inoculations performed under the "Suttonian system", as it must have been almost impossible for the Suttons to collect returns of numbers inoculated by all their partners. For example, Fewster, a partner of the Suttons, was very vague about the number of inoculations that he carried out; if he had kept statistical records he no doubt would have quoted them. No statistics can ultimately measure the importance of the Suttons, as their method greatly influenced practitioners such as Dimsdale, and less well-known local surgeons, apothecaries and amateur inoculators who spread the method, until finally inoculation itself was referred to as the Suttonian system.

Howlett summed up the general position in 1781 with reference to the extent of inoculation:

"In provincial towns and villages, as soon as this disorder [smallpox] makes its appearance, inoculation takes place amongst all ranks of people; the rich and poor, from either choice or necessity, almost instantly have recourse to it."[269]

Howlett's reference to "from either choice or necessity" probably referred to the existence of compulsory inoculation in some

parishes. Cowper's description of compulsory inoculation at Weston, Norfolk in 1788 has already been quoted. It was possible for parish authorities to exercise compulsory powers through their control of poor relief, although obviously this type of compulsion would not apply to richer parishioners.[270] It is impossible to give an exact account of the extent of inoculation by this period, as there are no reliable, comprehensive statistics available. However, it is clear from all the evidence cited (including examples of general and mass inoculations mentioned in previous chapters and to be discussed in the next chapter) that inoculation was practised very extensively in the countryside by about 1780. That this was not only a function on the reduction of cost is illustrated by the history of inoculation in Beaminster, Dorset. In 1758 at a Vestry meeting,

"It was agreed and Ordered that Mr Oliver Hoskins, Mr Jms. Daniel and Mr Jms. Cox shall be paid and Allow'd for Inoculating, Attending and Supplying Physick to all such Poor Parishioners as are willing to be Inoculated after the Rate of Five shillings p. head . . ."[271]

The result was that £6.15.0d was "paid for Inoculating 27 poor at 5/- each." This sum must be contrasted with that paid out in 1780 to John Daniel, John Cox and James David who each received "£17.13.4d for Inoculating the Poor." Similarly in 1791 a total of £66.6.6d was paid for the inoculation of 379 poor people, at three shillings and sixpence per head."[272] The population of Beaminster was 1,708 in 1775 according to a local census, and assuming a birth rate of 35 per 1,000, about 60 children were born into the parish every year. Between 1780 and 1791 there would have been about 660 children born into the parish, some of which would have died from diseases other than smallpox between the two dates. The inoculation of 379 poor persons in 1791, suggests that the majority of the population at risk, (mostly children in this case) were inoculated and thus protected against attacks of natural smallpox by the end of the eighteenth century. From all the evidence taken together it is justifiable to conclude that inoculation protected the majority of the population at risk from at least as early as 1780 in Beaminster.

Ironically, one of the best pieces of general evidence for the extent of the practice of inoculation in the country at large came from the pen of Edward Jenner. In his *Inquiry* written in 1798, he stated that the prophylactic powers of cowpox had probably first been noticed with "the general introduction of inoculation,"[273] and elaborated on this in his pamphlet on the origin of vaccine inoculation published in 1801:

"My inquiry into the nature of Cow Pox commenced upwards of twenty-five years ago. My attention to this singular disease was first excited by observing, that among those whom in the country I was frequently called upon to inoculate, many resisted every effort to give them the Small Pox. These patients I found had undergone a disease they called the Cow Pox . . . a vague opinion prevailed that it was a preventive of the Small Pox. This opinion I found was, comparatively, new among them; for all the older farmers declared that they had no such idea in their early days – a circumstance that seemed easily to be accounted for, from my knowing that the common people were very rarely inoculated for the Small Pox, till that practice was rendered general by the improved method introduced by the Suttons. So that the working people in the dairies were seldom put to the test of the preventive powers of the Cow Pox."[274]

In fact, Jenner's claims for his new type of inoculation were very modest in his first publication on the subject. He saw it merely as an improvement on an existing practice that was highly successful and very widespread:

"Should it be asked whether this investigation is a matter of mere curiosity, or whether it tends to any beneficial purpose? I should answer, that notwithstanding the happy effects of Inoculation, with all the improvements which the practice has received since its first introduction into this country, it not very unfrequently produces deformity of the skin, and sometimes, under the best management, proves fatal."[275]

Jenner himself had of course been a variolator for many years, and was in a position from personal experience to reach the above conclusion about the history of inoculation in the second half of the eighteenth century (he had been inoculated as a boy in 1756).

Nearly all contemporaries were unanimous that inoculation was very extensively practised in the countryside but greatly neglected in the large towns. For example, Watkinson wrote in 1777 that "since the year 1755 . . . inoculation, tho' much practised in the country parts of England, made no progress in the capital."[276] The neglect of inoculation in London is confirmed by Black who wrote in 1781 that "inoculation at the expiration of sixty years, since its first introduction, has made very little progress in London."[277] We have seen previously that the neglect of inoculation was also prevalent in other large towns during this period. In 1774 Aikin described the severe smallpox epidemic in Warrington which had taken place in the previous year; he noted:

"Not ten, I believe, were inoculated in the whole town and neighbourhood: these all did well, yet their example was not sufficient to overcome some accidental prejudices taken against it."[278]

Similarly Percival noted in 1773 that "inoculation is not much practised here" in Manchester.[279] The initial reaction of medical observers to the slow spread of inoculation in the large towns (as against its rapid spread in the countryside) is illustrated by the following statement made by Haygarth in 1780:

"And it cannot be supposed that the inhabitants of towns are more ignorant or more obstinate [than those living in the countryside]. There is not a reasonable doubt that our poor fellow-citizens would eagerly and universally embrace a proposal to preserve [by inoculation] their children from death and deformity, if the intelligent and opulent would humanely exert their influence and assistance to carry it into execution."[280]

Haygarth was referring to the fact that there was little or no free provision of inoculation in the large towns.

The only large town with an institution providing charitable inoculation was London – the London Smallpox Hospital was founded in 1746 partly to provide inoculation for the poor. However, as no person under the age of seven was inoculated by the hospital, the vast majority of the population at risk (in London practically all cases of smallpox occurred amongst children under the age of seven) was excluded automatically. For a poor person to be inoculated at the hospital, it was necessary for him to be recommended by one of the subscribers to the charity. The hospital was partly founded and used during the earlier period by the wealthy of London for the inoculation of their servants, many of whom were migrants from the countryside.[281]

There is no evidence that any of the parish authorities in the large towns made provision for the inoculation of their poor as was the universal practice in the countryside. The first person to attempt to remedy this situation in the towns was Lettsom, who established a London society for inoculating the poor in their own homes in 1775. He described the background to the events leading up to the establishment of the society and its effects as follows:

". . . to a very useful, and the most numerous part of the [London] community, the advantages resulting from it [inoculation] have hitherto in great measure been lost, either from the confined circumstances of the poor, or from their prejudices against so extraordinary an innovation in practice. At length, however, examples of the dreadful effects of the natural, and the wonderful success of the artificial disease [inoculation], have overcome these ill-founded prejudices, and nothing seemed wanting, to enable the poor to reap the benefit of this practice, but an establishment suited to their condition and circumstances . . . no Institution for that purpose existed here till the year 1775, when the Society for General Inoculation of the Poor was first established . . . The poor, however, though slow in admitting new improvements, are not soon to be reasoned out of self-evident facts, and their willingness to try Inoculation continues to augment with the success of the practice."[282]

It appears that Lettsom's plan of general inoculation of the poor in London was having considerable success, but it was unfortunately thwarted by the opposition of Dimsdale who argued that the inoculation of children in their own homes would spread smallpox through the over-populated courts and alley-ways. Lettsom, instead of responding that "most born in London have the smallpox before they are seven" anyway (a fact confirmed by subsequent statistical study), engaged Dimsdale in a bitter polemical dispute. As we have seen, Lettsom's main argument was that no "instance occurred to the medical practitioners engaged in this institution, to prove that the infection has been propagated from an inoculated patient."[283]

It was unnecessary as an argument, for even if inoculation spread smallpox it would have been irrelevant in a situation like London where the disease was already endemic. Lettsom attempted to organise popular inoculation for the poor in London a second time in 1779, but this too failed, mainly through Dimsdale's opposition.[284] The only provision of free inoculation for the poor in London until the beginning of vaccination was that provided by the London Smallpox Hospital.[285] It did increase significantly its number of inoculations (particularly after the adoption of the Suttonian method), partly through allowing the inoculation of out-patients and lowering the minimum age to five years.

According to an account of the hospital's history written in 1830, there were 48,062 people inoculated by the hospital between 1746 and 1822 when the practice was discontinued.[286] This is an average of about 632 inoculations per year throughout the whole period, which of course was insignificant in a town like London with a population of something like 800,000 (assuming a birth-rate of 35 per 1,000, about 28,000 children would be born every year). It is difficult to know how popular inoculation became in London through the practice of Daniel Sutton and other private inoculators, as there is so little evidence available. Watkinson stated in 1777 that "inoculation has become very fashionable" in London during "the last four years".[287] The fashion must have largely excluded the London poor, for as we saw in the last chapter, Franks found great prejudice amongst them against inoculation as late as 1800. Evidence

arising out of the polemical dispute between the supporters of the new vaccination and the old inoculation at the beginning of the nineteenth century, indicates that there was a fairly marked change in popular attitude at the turn of the century. According to a report in the *Gentleman's Magazine* in 1803:

"Mr. Wilberforce observed on the popular prejudice, that, out of 100 who had been vaccinated at the Smallpox Hospital, not five would have submitted, had they not supposed it to have been the old-fashioned mode of Inoculation."[288]

Similarly Jenner wrote to Lettsom in 1807 about an interview with Mr. Percival

"with the sole view of inquiring whether it was the intention of Government to give a check to the licentious manner in which small-pox inoculation at this time was conducted in the Metropolis."[289]

Adams confirmed this upsurge of inoculation, and wrote that it "increased to such a degree [in London], as to alarm many well intentioned people."[290] This and similar evidence makes it quite clear that inoculation was very extensively practised in London by the beginning of the nineteenth century, and this practice extended to the ordinary population. The popular support of inoculation in London was associated with an opposition to vaccination and when Jenner was awarded £20,000 by Parliament for his discovery of vaccination, John Gale Jones the radical leader and an apothecary, sent a message to Jenner at his lodgings in Bedford Place to advise him "immediately to quit London, for there was no knowing what an enraged populace might do."[291] Jenner had urged the Government to suppress inoculation, and occasional convictions of inoculators for "spreading smallpox" did occur. For example, according to one report in 1815:

"Another conviction has taken place of a person, of the name of Burnett, practising as an Apothecary etc. in London, (who held out the lure of gratuitous inoculation), for ordering children to be

exposed in the streets while under smallpox, whereby the infection was disseminated. He was sentenced, to six months imprisonment."[292]

There is no need to stress the unfairness of this conviction, for it must have been impossible to discover whether Burnett's inoculations did spread smallpox or not in a place like London where natural smallpox was endemic. However, this along with other evidence indicates that inoculation was very extensively practised amongst the ordinary population of London by the beginning of the nineteenth century.

Inoculation became popular in other large towns at an earlier date than it did in London, and this was mainly due to the establishment of special institutions for the inoculation of the poor. Haygarth summarized the position in 1793 when he discussed methods of eliminating smallpox:

"That, in large towns, inoculation, at stated periods must be performed, as already practised in Chester, Liverpool, Newcastle, Leeds, Dumfries etc."[293]

Free general inoculations first occurred in Leeds and Liverpool in 1781. There was a severe smallpox epidemic in Leeds, and during the first six months of 1781, 462 young children were attacked and 130 died; the plan of general inoculation was then adopted and during the next six months 385 were inoculated. Although this number of inoculations appears small in a town like Leeds with a population of 17,117, smallpox was virtually endemic in such towns and therefore the population at risk was only a fraction of the total population, i.e. infants and very young children born between epidemics, which occurred approximately every year in these towns. It was found by survey that there were only 700 persons (children) who had not been previously infected with smallpox in Leeds by the middle of 1781; 385 of these 700 were inoculated during the latter half of the year.[294] Very little else is known about inoculation in Leeds, except that there was at least a second attempt at some kind of general inoculation in 1788. Lucas, one of the surgeons to the General Infirmary at Leeds, discussed this inoculation in an account published in the *Gentleman's Magazine* in 1790:

"I had no sooner taken down the names of such children as offered for inoculation [March 1788], than I was requested by several persons to extend the same privilege throughout the parish. As such a plan exceeded my intended limits, I acquainted a noble Lord with my proceedings, who immediately approved what had been done, and, in the most polite manner, requested that he might be at the sole expence of executing a scheme which every family to whom he had applied had, not many years before, peremptorily refused. Notwithstanding a few private patients, near eighty were inoculated, without even any apparent danger; whilst two out of five who caught the natural infection died."[295]

This illustrates the unsatisfactory nature of the type of provision of inoculation in some of the large towns. Unlike small towns and villages, only sporadic attempts were made at general inoculations, and these sometimes only when there was financial backing from a charitably disposed local inhabitant of wealth. Clearly such infrequent "general" inoculations were inadequate in a large town like Leeds, which really required them every year. However, the success of these "general" inoculations (in terms of the immediate saving of life) is likely to have led to the adoption of private inoculation of the poor through the practice of apothecaries, amateurs and parents (as in Canterbury and East Anglia). The "general" inoculation at Liverpool was less successful, for out of "about three or four thousand liable to the disease" in 1781, only 417 were inoculated gratuitously and 100 more in private practice."[296] There was a second gratuitous inoculation in Liverpool in the following spring of 1782, but nothing is known of further inoculations after that date, except what Currie wrote to Haygarth in 1791:

"I lament much that our [Liverpool's] plan for general inoculation is dormant at present, but I hope it will be revived. Our experience, for several years, was uniformly in favour of its utility."[297]

Currie referred to the practice of general inoculation "for several years", which indicates that the inoculations of 1781 and 1782 were

subsequently repeated. Also we must not assume that inoculation ceased to be practised in Liverpool in 1791, as it might have been the case that there had not been a recent severe epidemic to provide the necessary incentive to inoculation. It was stated generally to be the case by a medical observer in 1807:

"Unless, therefore, from the immediate dread of epidemic Small-pox, neither Vaccination nor Inoculation appear at any time to have been general, and when the cause of the terror has passed by, the Public have relapsed into a state of indifference and apathy, and the salutary practice has come to a stand . . ."[298]

The most systematic attempt to inoculate regularly each year in a large town occurred in Newcastle. Dr John Clark, applied Lettsom's ideas of general inoculation in the homes of the poor, and between 1786 and 1801 there were 3,268 children inoculated gratuitously by the Newcastle Dispensary.[299] This is an average of just over 200 inoculations a year, which must have protected only a minority of the population at risk, although this population was confined to young children as reflected in the ages of those inoculated: of 1,056 inoculated during the four-and-a-half years 1786-1790, only 73 were above the age of five and nearly a half (460) were infants under the age of one.[300]

It has been previously noted how in Newcastle (1792) "especially among the lower classes, a great and general prejudice against the practice of inoculation" existed. There is no evidence that this prejudice had greatly diminished by the end of the eighteenth century, although private inoculation by apothecaries and others may have become increasingly popular. There were other dispensaries which steadily offered gratuitous inoculation to the children of the poor: those at Whitehaven, where there were 1,079 Inoculations from 1783 to 1796.[301] Haygarth was responsible for starting free inoculation of the poor in Chester and between the Spring of 1780 and September 1782 there were 213 poor children inoculated as well as 203 done privately.[302] This is an average of about 200 inoculations a year; of a total population of 14,713 in Chester in 1774, 1,060 had not had smallpox and were therefore in need of

protection. Assuming the years 1780-82 to have been similar to 1774, nearly forty per cent of the population at risk were inoculated.[303] Haygarth claimed in 1793 that the poor were generally indifferent to inoculation – although later we will see that he probably exaggerated this:

"In Chester, the lower class of people have no fear of the casual small-pox. Many more examples occurred of their wishes and endeavour to catch the infection, than to avoid it."[304]

The most successful practice of inoculation in a large town of which a detailed description is available, was that in Carlisle. John Heysham, physician to the Carlisle Dispensary that was founded in 1782, gave a year-by-year account of epidemics and the progress of inoculation between 1779 and 1787 in Carlisle and the surrounding neighbourhood. This account is of sufficient importance, to be quoted at some length:

"1779 . . . several hundreds were inoculated in the neighbourhood of Carlisle, and it is a pleasing truth, that not one of them died . . . Yet so great is the prejudice against the salutary practice of inoculation amongst the vulgar [in Carlisle], that few, very few, can be prevailed upon, either by promises, rewards, or intreaties, to submit to the operation . . .
1781. Great numbers were inoculated both in town and country villages.
1783. In September and October, the small pox became so general, and were of so fatal a kind, that the monthly committee of the dispensary were of opinion, that a general inoculation of the poor and indigent inhabitants, would be attended with very beneficial effects. Early in the month of November, a general inoculation accordingly took place. Great numbers were inoculated, not only by the surgeon to the dispensary, but also by most of the other surgeons in the town.
1785. Early in the month of December, 1784, the small pox were introduced by some vagrants . . . As soon as the disease made its appearance within the walls of the city, the monthly committee of the dispensary resolved, that a general inoculation of the poor inhabitants, agreeable to the plan which I proposed in the year 1783,

should take place at the dispensary, and it was with great pleasure I observed the prejudices of the vulgar against that most salutary invention, were greatly diminished. For as soon as the resolution of the committee was made public by the town crier, great number from all quarters of the town flocked to the dispensary, to reap the benefits which it held out of them . . . So that upon a moderate calculation, the whole number inoculated this year, amounted to two hundred; every one of whom recovered.
1787. The small-pox made their appearance in January, and were in Carlisle the greatest part of the year; but inoculation soon became general, which prevented the disorder from raging with any great degree of violence . . . Eighty four were inoculated at the dispensary, all of whom not only survived, but had the disorder very favourably; and considerable numbers were likewise inoculated by several surgeons in the town."[305]

The protection which inoculation gave the population at risk in Carlisle was reflected in a sharp decrease in the numbers dying from smallpox, which will be discussed later. Heysham estimated that 300 children caught smallpox in 1779, and if we take this number to be the approximate population at risk in 1785, when Heysham estimated that a total of 200 had been inoculated, it is clear that a majority (about two-thirds) were protected by inoculation in 1785.

It is of some interest to note that according to Heysham in 1779 the population of the villages surrounding Carlisle were already practising inoculation quite generally, while in Carlisle itself the ordinary population was still opposed to the practice. This difference between country and town did not disappear at any time during the eighteenth century, even in a town like Carlisle (although two thirds of the population at risk were inoculated in 1785), and this was reflected in a statement made by the medical writer Walker in 1790:

"Of late, physicians have made a distinction between partial and general inoculation. Partial, is the mode in which inoculations are carried on at present in the metropolis, and all the capital towns of Great Britain, where everyone who favours the measure, puts it in

practice at his own conveniency. General inoculation supposes an agreement of the whole inhabitants to have their children, and all susceptible of the disease, inoculated in one day, a measure which only can be practised in villages and small towns."[306]

It is possible to learn a great deal about the practice of inoculation at the end of the eighteenth and the beginning of the nineteenth century by studying the practice of vaccination. One thing that has always puzzled medical historians is the fact that vaccination was very much less popular in England than it was on the Continent. This is illustrated by a question asked by the Royal College of Physicians in its inquiry on vaccination published in 1807:

"Why the Practice of Vaccination has not been more generally adopted, especially less so in this country than abroad?"[307]

The answer to this question is to be found in the evidence submitted to the College. It was noted that vaccination made little headway in those areas – outside the large towns – where inoculation was generally practised. For example,

"The Reports from the professional members of this Society resident in different parts of the County of Somerset universally represent the almost insuperable Difficulties attending their attempts to diffuse the benefits of vaccination owing to the powerful prejudices of the lower classes of the people and also describe the destructive extent to which the practice of Variolous Inoculation is carried on by several persons, some not of the Medical profession . . ."[308]

We do not have to pay too much attention to the critical language used about inoculation, as such language was common to both the supporters of vaccination and inoculation. Vaccination was much more popular in the large towns: from Manchester it was reported that

"The lower classes also in large towns, where they can be vaccinated gratis at the public Charities, avail themselves pretty generally of this advantage."[309]

On the present argument, the reason for this was that inoculation had never been universally popular in these large towns as it had been in the country. There was a very specific reason why those familiar with the benefits of inoculation rejected vaccination. From Leeds it was reported in 1801 that

"a very intelligent Practitioner, about seven or eight Miles from Leeds, to whom I sent [vaccine] Matter, and who has inoculated [vaccinated] 150 children in the new Mode, informs me, that a Child, whom he had, a Year before, inoculated for the Cow Pock, and who went thro' the progressive and regular Stages of that mild Disease, has lately been seized with the natural Smallpox which prevailed epidemically in the Village . . . he has had too much Experience in this Way, that he asserts the fact as clear as decisive . . . The Practice [of vaccination] has, however, from such Rumours, declined considerably, and we are now but little in the habit of it in this place, many giving the preference to the inoculated Small Pox."[310]

Contemporaries familiar with inoculation expected to be protected for a lifetime, and vaccination only protected for relatively short periods, although it significantly mitigated the severity of attacks even in the longer period. This affected not only the general population but also the medical profession itself, most of whom had been initially enthusiastic supporters of vaccination:

"very lately [1807] the Small Pox appeared in several parts of Devonshire and Somersetshire, where Vaccination had been practised, and the people insisted on Inoculation, with which some of the Faculty were obliged to comply, seeing the infection spread so fast. That Mr. Goss, of Dawlish, had resorted to a general inoculation, and had submitted his own children, whom he had formerly vaccinated, to the test, two of whom received the Small Pox, and one resisted it."[311]

Mr. Goss submitted his children to the test by inoculating them, and inoculation took on two of the three children. Contemporaries assumed that this meant that such children were not protected by vaccination

against natural smallpox. The limited protection given by vaccination against future attacks led to Jenner's reputation deteriorating, even amongst his early wealthy supporters, and he wrote in 1811:

"And now this single, solitary instance [Lord Grosvenor's son caught smallpox 10 years after being vaccinated] has occurred, all my past labours, and the result of those labours are forgotten, and I am held up by many, perhaps the majority of the higher Classes, as an object of derision and Contempt."[312]

Jenner was under-stating the number of such cases, although it might have been the first one amongst the aristocracy. The result of contemporary disappointment with vaccination was the continuation of the practice of variolation. Generally, the medical profession continued to be strongly in favour of vaccination, while the population at large – particularly in country areas – remained attached to the old inoculation. For example,

"The small-pox was accidently introduced into the village of Luddington in the year 1815. A gentleman who was the overseer of the parish immediately endeavoured, in conjunction with Mr Pritchard [senior surgeon to the Stratford-On-Avon dispensary], to persuade the poor of that village to have their children vaccinated. But with the exception of one family, and of one individual in another family, all the poor inhabitants were obstinately determined to have their children inoculated for small-pox: and, with the exception of one infant, they had them inoculated accordingly."[313]

Other examples were found at Wickforn, Berkshire in 1821, where out of a total of 51 children involved, 48 were inoculated, and only three vaccinated,[314] and Aston Cantlow near Henley in Warwickshire in about 1816, when only one of 75 people chose vaccination in preference to inoculation – and then only because the doctor allowed him to continue drinking ale in the one and not the other. (He later changed his mind and was inoculated.)[315]

The persistence of inoculation in East Anglia was described in some detail by Cross in his study of the smallpox epidemic which occurred in Norwich in 1820. In some areas inoculation was "practised entirely by old women and a Druggist", whereas in another region centring on Norfolk "itinerant inoculators, irregular practitioners and old women introduced and extended the disease to all quarters by inoculation."[316] Cross, like most of his medical contemporaries was deeply hostile to inoculation by this time, and was shocked to find that in one Hundred made up of 22 parishes, "12 of these were inoculating gratuitously" – and was even more shocked to discover "that several persons of the lower class, some of the inhabitants of Work-houses, were going about the country inoculating."[317] The dilemma that popular demand for inoculation put the medical profession into, was revealed as follows:

"Many medical men, desirous of doing their duty by discouraging variolous inoculation, have been placed in the most unpleasant situations, and not unfrequently have been compelled to commit an act which they believed to be immoral and injurious, because they could not afford to sacrifice the small emmolument arising from it; some have reluctantly inoculated whole parishes of the poor, at the instigation or order of an overseer."[318]

Vaccination was clearly resisted in many areas because of the preference for the old inoculation, and in some places the new operation was not introduced for several years after Jenner's first announcement of his discovery. Dr Forbes, senior physician to the Chichester Dispensary and a supporter of vaccination, gave a very detailed description of the history of inoculation and vaccination in the Chichester area from 1806 until 1821, which indicates the general position of prophylactic measures taken against smallpox during the first two decades of the nineteenth century:

"The last general inoculation for small-pox that took place in the city of Chichester and neighbourhood was in 1806; six years later, a considerable number were inoculated in Havant and Emsworth, and the vicinity, but since that time, variolous inoculation has been nearly

unknown throughout the district. A few cases of small-pox have, at different times, been introduced by strangers, and a few of the Practitioners in the country have occasionally inoculated a small number of persons, but the occurrence of these solitary cases has tended rather to increase the practice of vaccination than to spread the small-pox: and the general fact, on the breaking out of the late epidemic [1821], certainly was that nearly all the children born in this district since the period above mentioned, had either been vaccinated, or left entirely unprotected from the infection of small-pox. Owing to the prejudices and thoughtlessness of the common people, vaccination had certainly been much less practised than it ought to have been; being, in a very considerable degree confined to the children of the upper and middle classes. The relative proportion of children vaccinated, and those left unprotected, during the period that has elapsed since the abolition of variolous inoculation, may be, in some measure, estimated from the facts that about 500 have been annually vaccinated by all Surgeons of the district, before the present year, and that between 2 and 3,000 were vaccinated, and about an equal number inoculated, during the panic of the late epidemic."[319]

Nearly a half of those inoculated during 1821 had been previously vaccinated, and this was the result of distrust of the power of vaccination to protect against future attacks of smallpox. Even so Forbes indicated that the vast majority of the population at risk were protected either by inoculation, vaccination or a combination of both. Vaccination was more popular amongst the "upper and middle classes" and inoculation amongst "paupers" and others "of the same class of society". Forbes does not say much about the practice of inoculation at the beginning of the nineteenth century, except that "the last general inoculation . . . was in 1806", which implies that it was the last of a preceding series.

This is confirmed by the claims of one of the amateur inoculators practising in 1821, 107, a farmer who had practised during the earlier period, and as we have seen boasted "that of 10,000 persons inoculated by his father, not one died, and that his own success has been as great." Pearce also claimed that his mother had been a very successful inoculator, which was not unusual in this area, for Forbes

noted "many sly poachers of the other" sex who were rivals to Pearce; he also had three male rivals who were amateur inoculators – a knife-grinder, a fishmonger, and a whitesmith.[320]

From the evidence about the practice of inoculation during the period that vaccination began to be practised, we may conclude that inoculation was almost universally established before the introduction of vaccination, except in large towns such as London, Manchester, Glasgow, Newcastle and Whitehaven – in these latter places vaccination was introduced at an early date and on a wide scale, which suggests that inoculation was never practised amongst the majority of the population at any time.[321] However, it must not be forgotten that only about a fifth of the total population lived in towns of 10,000 and above in 1801,[322] and that in some of them (such as Carlisle, Leeds and London) inoculation had made considerable headway by the end of the eighteenth century. Rowley, a surgeon, in a defence of inoculation published in 1805, claimed that "Smallpox inoculation was a well-known, proved, and absolute prevention from receiving the natural Small-pox infection, as millions of people now living can testify",[323] a conclusion which perhaps is not unjustified in the light of available evidence. Inoculation was eventually abolished by law in 1840, by which time the importance of repeated vaccination had been well established, so diminishing the major objection to it (a limited period of protection). However, even as late as 1840 the Bishop of London could say during the debate of the bill banning inoculation that

"it was well known that, in agricultural districts of the country, there had not been for many years past the least difficulty in obtaining vaccination gratuitously, but many of the ignorant poor were strongly prejudiced against it, and paid a much greater attention to empirics than to the advice of the clergy. He thought that the bill would not do half the good that was intended, unless those persons were prevented, by penalty from practising inoculation."[324]

To understand how the poor and the population at large became so attached to inoculation, and to see the exact extent of inoculation, we

must turn to a consideration of the practice of general inoculations, which laid the foundation for the virtual elimination of smallpox.

CHAPTER 5
General Inoculations

The practice of the general inoculation of all vulnerable members of a particular community arose through the interaction of two factors: (i) the fear that partial inoculations of only some members of the community would spread the natural form of the disease to the rest; (ii) the highly successful innovation of method brought about by the Suttons, which made inoculation both sufficiently safe and cheap to obtain general acceptance. The "airing" of patients and sending them into the community as a part of the Suttons' "cold treatment", aggravated the fears of the vulnerable population, which initially provoked great hostility, but later led to the widespread acceptance of general inoculation. Thus ironically, what has traditionally been thought of as a major reason for rejecting inoculation as a cause of falling smallpox mortality – that it spread the natural form of the disease to vulnerable members of the population – was in fact one of the major reasons for the establishment of its practice, with a consequent impact on mortality.

General inoculation was therefore a logical outgrowth of contemporary belief and practice, although not all parish authorities agreed in the earlier period to find the sums of money required to pay for such relatively large numbers of inoculations. The person whose name and authority came to be most closely associated with the advocacy of general inoculations, was Thomas Dimsdale, who was the most influential single inoculator after Daniel Sutton. The history of general inoculations may be largely traced through Dimsdale's writings, and we can start by quoting his account of the success in clearing his home town, Hertford, of smallpox in the period 1766-1774:

"In a former publication, I gave an account of the occasion of a general Inoculation [in 1766] at this place [Hertford]; from that time the town was released from any apprehensions of the disease, until the year 1770, when it appeared again . . . we had then upwards of two hundred and fifty [inoculated] patients, some of whom were new inhabitants, but the rest consisted for the most part of very young children . . . In the year 1774 the disease appeared a third time the

same request was renewed, and with the same assistance afforded, the whole town was inoculated once more, and now the number amounted only to about 120; from that time we have heard nothing of Small-Pox, and I verily believe, that within these ten years not six persons have died in Hertford of this disease . . ."[325]

Practically all the population at risk must have been inoculated in Hertford, as only about six persons had died from the disease in the previous ten years, and some of these six must have been among the "two or three" which died in 1770 (and presumably in 1774) before the general inoculation took place. Dimsdale contrasted the success of inoculation in Hertford, with what he considered to be the unsatisfactory "partial" inoculation of the town of Bedford:

" . . . the melancholy account of the consequences of a precipitate Inoculation of the greatest part of the inhabitants in a populous town [Bedford], within this last year [1778]. A pretty general Inoculation was suddenly agreed on, and within one week 1,100 were inoculated . . . but many others in the same town, from religious opinions, ill health, or timidity could not be prevailed on to assent to the scheme; 250 of these soon caught it, and the distemper proved uncommonly fatal to them, for about two in seven died, so that in a few weeks 59 at least lost their lives from this circumstance."[326]

Dimsdale considered this mass inoculation to be "precipitate" and "melancholy" in consequence, but although 59 people dying from smallpox was obviously of no small consequence, this should be set against the great majority of the population being protected against the disease. Dimsdale blamed the deaths of the 59 on the partial nature of the inoculation, but he failed to mention that "a bad kind of natural smallpox had broken out in the town before the inoculation began."[327] Dimsdale and many contemporaries believed that inoculation was highly contagious, and this was enough to provoke them to insist that all vulnerable members of a community should either be inoculated or removed from the scene where the inoculations were taking place.

Dimsdale published in 1781 what he considered to be the ideal plan of general inoculation and how it should be carried out:

"A list of the names and ages of such inhabitants of every town and village as have not had the smallpox, is first necessary to be obtained; and marks should be made against the names of those, who, on account of their ill state of health, or other reasons, are not thought fit subjects for the operation, in the judgment of the inoculator, and such persons should be provided with a separate abode, where they may not be in danger of receiving the infection: the rest should be collected in one place, inoculated at one time ... During the whole of this time, and indeed throughout the whole process, the sick may continue at their own houses."[328]

Dimsdale had in effect carried out this plan in Hertford, virtually eliminating smallpox as a result, and extended it to a number of other neighbouring parishes at about the same time; in 1776 he described how the practice of general inoculation had grown:

"Assisted by my learned friend Dr. Ingenhouz and my two sons, I inoculated, at different times, the neighbouring [to Hertford] parishes of East Berkhamstead, Hertingfordbury, Bayford, and the liberty of Brickenden . . . more than 600 [people] ... this mode of practice, as I have been informed, has been also used successfully by many others in different parts of England. So far as has come to my knowledge, general Inoculations have hitherto been confined to small towns and villages . . . "[329]

Two years later in 1778, Dimsdale noted even further development of general inoculation in the counties surrounding London – he was discussing an expected decline of smallpox deaths in London as a result of country migrants being inoculated before moving to London:

"the extensive practice of general Inoculations in the country, which have prevailed in a remarkable manner within the last two years in the counties of Bedford, Bucks, Herts, and Cambridge, and others contiguous to London, and these patients have been generally such inferior persons as may be supposed to supply London. To such

extent has this practice been carried, that I imagine the number must amount to many thousands, and for the most part it has been conducted properly, that is to say, every one has been inoculated, or retired from the scene of infection . . ."[330]

This development occurred throughout all areas of the country; Haygarth noted in 1785 that "whole villages in the neighbourhood [Chester], and many other parts of Britain, have been inoculated with one consent"[331] and as we have already seen, Howlett stated earlier in 1781 that when smallpox appeared in provincial towns and villages "inoculation takes place amongst all ranks of people, the rich and poor, from either choice or necessity, almost instantly have recourse to it."[332]

Once a community had experienced one successful general inoculation, it appears to have become a regular occurrence whenever a smallpox epidemic threatened. Maldon in Essex was one of the first places to undergo a general inoculation – Sutton had inoculated all vulnerable people in the town and had completely cleared it of smallpox in 1764 – and general inoculations were subsequently repeated in 1767, 1779, 1788, 1797 and 1806.[333] It is not known exactly how many people were inoculated at any of these dates (except 1764), and the references are merely summary statements, so that, for example, the Maldon parish register merely states on February 17th, 1797 – "a General Inoculation/Small-Pox", and on February 27th, 1806 – "Small Pox a General Inoculation". It is clear, however, that inoculation was practised quite generally about every nine years, presumably when a new epidemic threatened. By the last two decades of the eighteenth century, the term general inoculation had become so commonplace that it was used in a matter-of-fact way; for example, Jenner quoted a letter from Dr John Earle, stating how "in March 1784, a general inoculation took place at Arlingham" in Gloucestershire[334] and Jenner himself quite casually referred in his *Inquiry* to "a general inoculation taking place" in Berkeley in April 1795.[335]

Jenner was not the only member of his family to practice smallpox inoculation. "The paupers of the village of Tortworth, in this county (Gloucestershire), were inoculated by Mr Henry

Jenner, Surgeon, of Berkeley, in the year 1795,"[336] and G. C. Jenner published "an account of a general inoculation at Painswick (Gloucestershire)" which took place between the end of May and the end of July 1785, seven hundred and thirty-eight people being inoculated."[337] This latter general inoculation was only resorted to after the outbreak of a virulent smallpox epidemic, which "destroyed nearly one-third of those who were infected by it."[338] This delay in resorting to inoculation until an epidemic had broken out, obviously led to the risk of some of the inoculated catching the natural disease before they had time to be protected – and any deaths resulting would be attributed to inoculation.

Sometimes a parish acted with very great speed when an epidemic threatened. The following is an account of the reaction of the Northwold (in Norfolk) parish authorities taken from the churchwardens accounts:

"Northwold. Jan 28th 1788. At a Meeting of the principal inhabitants of this parish holden at ye Bull Inn, it appered that Ann Robinson a poor widow and her family were ill with the small pox in the naturell way: that upwards of three hundred and seventy persons legaly setlled who were never had been infected by the small pox where resident in the parish. It was therefore resolved that a genereld innoculation of such uninfected persons should take place . . . "[339]

As most of these 370 people "were unabel to Defray the nessery Expence" of inoculating themselves and their families, the parish paid for the major part of them; 226 were inoculated at the parish expense, 74 paid for it privately.[340] The parish seems to have excluded the poor who had not a legal settlement from this form of medical relief; how common this kind of exclusion was is unknown – it is the only example that has come to light in the evidence reviewed for this book.

Reluctance to undertake a general inoculation could however take extreme forms, and the following instance in Lewes, Sussex for the year 1794 is quoted at some length as it illustrates so vividly both

the great fear that smallpox created in people, and the measures and lengths that they were sometimes prepared to go to, in order to contain the spread of the disease and avoid the necessity of a general inoculation:

"On Monday 4th of January, it was represented to the Chief Officers of the Borough that the Small Pox was at that time at its full height in the House of George Apted, in St. Mary's Lane . . . he was determined they [his family] should all remain where they were. The Constables then resorted to the early Measures they saw within their Power; they caused a high wood Fence to be erected around his Door, and placed a Watch both by Night and Day, to prevent the infected Family from mixing any more with other Persons in the Neighbourhood. On Friday the 10th at Six in the Evening, another Meeting on the same Business was called by the Constables. At this second Meeting (which entirely filled the Town Hall) it appeared that the Disorder further manifested itself in the families of several other Persons within the said St. Mary's Lane, and that each of them refused to remove, the Determination of this Meeting was to block up the infected Lane at both Ends . . . Several of the Heads of infected Families having, in the Hall (at a meeting on Saturday, 11th), refused to remove their Children etc or to suffer them to be removed, a general Inoculation was by some thought advisable; it was therefore deemed proper to request the Constables again to adjourn the Meeting to the next Evening (Sunday) and to give the most public Notice by Hand Bills and by Proclamation at the several Parish Churches that the Question of the Necessity of a General Inoculation would on that Evening, be discussed and determined . . . It was afterwards resolved that in the Consequence of the Opinions given to the Faculty, a General Inoculation does not at present appear necessary. On Monday, the 13th every Gentleman of the Faculty within the Borough with one of the Constables visited the infected Families, and finding the Disorder much wider spread than they had expected, they desired the Constables again to call a Meeting of the Inhabitants which was very numerously and respectably attended – at this Meeting it was determined that a General Inoculation being an Evil much less

dreaded than a General Infection, in the Natural Way, which was very likely to take Place within this Town & Neighbourhood, it was solemnly put and carried that 'Circumstances as are at present are, a GENERAL INOCULATION ought to be adopted within the Borough': The Inoculation accordingly commenced the next Day, & was continued till the 20th when the Town was again convened, & determined that the General Inoculation in the Town of Lewes, ought to Cease, and that a Continuance thereof, by the Introduction of Strangers, would be injurious to the Trade etc of the Gentlemen who had undertaken the Business by Inoculation to desist from that Practice within the said Borough. The number of Persons inoculated in Consequence of the above mentioned Resolutions ammounted according to the best Accounts the Constables could produce to about 2890, of which number 46 died under Inoculation.

John Richards
Arthur Lee.
(Constables)."[341]

This account reveals a great deal about contemporary attitudes to smallpox and inoculation. 46 of the 2,890 people inoculated died – and probably the majority of these deaths were the result of previous infection with natural smallpox during the period of delay. These deaths would have fuelled the fear of the next general inoculation, perpetuating a cycle of fear and delay. However, in spite of these 46 deaths, the general inoculation was in overall terms a great success. Given that about one in three people catching smallpox died of the disease at this time, the number of lives saved was significant (it would be of the order of 950 people); the extent of the inoculation is indicated by the fact that the total population of the town of Lewes was only 4,909 in 1801 – thus well over half of the population would have been inoculated in 1794.

The number of people dying from smallpox caught during the delay before general inoculation seems to have varied greatly from place to place. Reference has already been made to the advertisements placed by the Ewell authorities in the *Gentleman's Magazine* describing a mass inoculation without loss of life and that entered by the Overseers of Irthlingborough in the *Northampton*

Mercury, giving an account of the "upwards of Five Hundred People" inoculated in the village without a death in 1778. A similar entry was placed in the *Ipswich Journal* by the minister, surgeons and churchwardens of the town of Diss in Norfolk on the 3rd June. 1784:

"In March last, the smallpox broke out in this town; It was of so favourable a kind, that the sick did not confine themselves to their houses; by means of which the disease was communicated to several families, which induced the inhabitants to submit to a general inoculation. In Eight or Nine days, more than Eleven hundred were inoculated, from the age of one month to between Eighty and Ninety years; of which number not One person died. Scarce any of the poor were kept from their labour more than Two or Three days; many not at all. These circumstances are published as inducement to other parishes to adopt the same happy mean's of irradicating this dreadful disorder."[342]

The motives of the parish authorities may not have been as unambiguously altruistic as claimed, for the advertisement was prefaced with the statement that "there is not ONE PERSON In This Town that has the SMALL POX", and trade was probably a factor in informing the public of this fact. We saw how in Lewes the town authorities were anxious to suppress inoculation once the general inoculation of the townsfolk was over, because it would be "injurous to the Trade" of the town-people fearing to come into a town where smallpox, even in the inoculated form, was known to be present – and this motive to preserve trade provoked other parish authorities to place protective advertisements in their local newspaper, e.g. the churchwardens, overseers, physician and surgeons of the parish of Hadleigh in Suffolk put the following entry in the *Ipswich Journal* for June 17th, 1778:

"Whereas a general Inoculation for the Small Pox took place in this parish in the month of March last. We, whose names are hereunder written, do hereby give notice, That the said town is now, and

has for some time past been, entirely FREE from the said infection, and that the parish may be resorted to with safety."[343]

The clearing of a market town of smallpox must have been a very great economic incentive for parish authorities to pay for general inoculations; smallpox was often in market towns for periods of up to two years in the pre-inoculation era, and this could virtually ruin the trade of a town for that period.

The economics of inoculation is brought out most clearly in the following very detailed account of a general inoculation carried out at Brighton in 1788. It seems to have been conducted along the lines suggested by Dimsdale, with a house-to-house survey of all those who had smallpox previously, and those still vulnerable to the disease. The account is quoted in full, as it is unrivalled in the detail that it supplies about a specific general inoculation:

"TOWN OF BRIGHTHELMSTON
Be it Remembered that on the 25 Day of Jany 1786 at a Public Vestry held at the Town Hall in Brighthelmston Pursuant to Public Notice given, It was (in consequence of the Heavy Expense Brought on the said Town by the Removal of Patients in the Natural Small Pox to the Neighbouring Pest Houses, which usually amounted to Six Pounds Each Person and in consideration of the Small Pox Breaking out in Several Different Places in the Town at once) then Unanimously Agreed by the Inhabitants there assembled that no more Persons should be Removed at the Parish Expense. And it also appearing Impossible to Prevent the Infection from Becoming General It was also Agreed for the Poor in the Town House and such other of the Inhabitants of said Town to be inoculated as should be Deemed Proper objects of Relief, by the Churchwardens, Overseers, and Twelve of the Principal Inhabitants of said Town Appointed by the Vestry as a Committee for managing and conducting Inoculating the above Poor at the Parish Expense.

And for carrying the foregoing into Execution in the most Exact Manner it was Determined to find out the Numbers of Persons

who Had the Small Pox and those who Had Not had the same in said Town

And on a Survey Made by the above Committee and Parish Officers Were Found the Following Numbers:

January 26 1786	*Numbers Who Had Smallpox*	*Numbers Not Had Smallpox*
In West Street etc.	351	322
In Middle Street & Lanes	231	272
North Street & Lanes	234	295
Ship & Black Lyon	318	336
Knab Cliff Bn Place	260	291
Little East Street	308	291
East Street N. Row Steyne & Pool Lane	31	50
Poor in the House	31	50
Numbers Supposed to be Got into Town after Taking Numbers	–	30
Total	1733	1887

Out of the Above Number Five Hundred and forty five were Inoculated at the Parish Expense.

At the Same Vestry Messrs Lowdell Gilbert, Parkhurst & Tilson Surgeons and Apothecaries of This Town Agreed to Inoculate all the Poor Above mentioned, all Servants and Day Labourers at Half a Crown Each, Medicines Included – and all Other Persons at three Half Crowns Each.

In consequence of the Foregoing the Inoculation Commenced on the 27 of Jany and in Course of a few Day's the Aforesaid Numbers of Eighteen Hundred Eighty Seven were Inoculated – Persons of all ages from One Day to Near Fourscore Years Old.

It Also Appears that by the Goodness of Providence and the Care and Attendance of the Physical Gentn though in the above Number were Persons of all Ages and Complaints Women very

Near their time etc. Yet the very Small Number of [blank] were all that Died of the Small Pox.
The Expense of Attending Patients:

1785. Fifteen in Natural Small Pox to Dr. Lander Dennett Carr etc	£82.16. 0
1786. Ten Natural Patients & Expense of carrying them Also Funerals of Two at Mr Dennett	£57.04. 4
	£140. 0.4
1786. Expense of Inoculating 545 Persons at 2/6 each	£68. 2. 6
To Expense of Relief to Different Families By Flour Coals Cash etc During their Inoculations	£82.17. 6
	£151. 0. 0

Messrs. Dennett of Storrington and Sanders of the Broyle assisted in the General Inoculation of the Town and Had a Considerable Number of Patients on Same Plan as the Doctors of the Town."[344]

The cost of nursing and burying twenty-five natural smallpox patients was nearly as great as the inoculation of 545 poor people in the town. Economic considerations were listed even before the threat of a smallpox epidemic as factors in carrying out the general inoculation, which appears to have been performed with great thoroughness. All of the 1,887 people not yet protected against smallpox were inoculated, although only 545 of these were paid for by the parish (the large total numbers involved may have accounted for this low proportion). The general inoculation was successful, although according to a local historian writing in 1818, 34 of the 1,887 cases died. A general inoculation was organised along the same lines six years later in 1794, and the enumeration of the population revealed a greatly increased total of 5,669 – of which 2,113 were inoculated (about 250 coming in from neighbouring villages).[345] "No more than 50 died" of the 2,113 inoculated, but it is likely that in both the 1786 and 1794 general inoculations, the delay in starting inoculation before natural smallpox cases had occurred, contributed to the deaths amongst the inoculated.

However, it would seem that only a very small proportion of the total inoculated were affected in this way; perhaps a more typical general inoculation was that which occurred at Tenterden, Kent in 1798, when "toward the end of the year there was a general Inoculation took place and out of eleven hundred and sixty seven who had the complaint only three died."[346]

Most general inoculations seem to have included people in all age groups, ranging in Brighton in 1786 "from One Day to Near Fourscore Years." Similarly at Dursley in Gloucestershire when the local surgeon, Mr Fry, undertook a general inoculation in the Spring of 1797, he "inoculated fourteen hundred and seventy-five patients, of all ages, from a fortnight old to seventy years."[347] However, with the repetition of general inoculations every few years when epidemics threatened, the average age of those inoculated obviously dropped significantly; for example, Mr Wayte a surgeon who practised at Calne in Wiltshire described the general inoculation of the parish as follows:

"in September, 1793, when the poor of the parish were inoculated . . . we inoculated six hundred and upwards . . . Besides the poor, I inoculated about two hundred [private] patients . . . Now in inoculating a whole parish, we have no choice of patients, all ages, and the sickly as well as others, were inoculated, but these were mostly children, as I assisted in inoculating the whole parish, about twelve or thirteen years ago."[348]

There were only eight deaths registered as being due to smallpox in Calne during the period 1783-1802, and as we shall in a later chapter, repeated general inoculations led to the inevitable consequence: the almost total elimination of smallpox.

CHAPTER 6
The Practice of Inoculation in America, Scotland, Ireland and on the Continent of Europe

It is possible to discuss the practice of inoculation in other countries in Europe only very briefly, but having attempted to re-define the nature and effectiveness of inoculation, it might be of some interest to look at some of the evidence on its practice in these countries. One general point can be made at the outset: those countries where smallpox tended to return only infrequently and therefore take an epidemic form, were usually the ones where inoculation was most widely practised. This was true of the British American Colonies (later the United States), Ireland and the Highland areas of Scotland. The psychology associated with the epidemiology of the endemic as against the epidemic form of the disease was discussed earlier. Townspeople tended to become fatalistically resigned to a regularly returning disease affecting only a minority of their children in any one year, whereas country people reacted with panic at the prospect of an epidemic striking a large proportion of the population at one point of time.

America is undoubtedly one of the countries in which inoculation was most widely practised, which makes it all the more surprising that no comprehensive scholarly study of its history in that country has ever been published. We have seen in the earlier period how active inoculators were in the American Colonies, and how much influence they had through the publication of the works of people like Boylston and Kilpatrick. The neglect of the history of American inoculation is all the more surprising given the comprehensiveness of some of its statistics. The figures for Boston cover the whole of the eighteenth century period and include details of population, numbers of natural smallpox cases, smallpox deaths and people inoculated at different times. The complete set of figures will be discussed in the last chapter, but here it is sufficient to note the rapid growth of the practice of inoculation, particularly after 1752, so that by the end of the century all but a tiny minority of the

vulnerable population were protected by inoculation. The following Table gives a summary of the relevant figures:

Smallpox and Inoculation in Eighteenth Century Boston, U.S.A.[349]

Date	*1721*	*1730*	*1752*	*1764*	*1776*	*1788*	*1792*
Natural Smallpox Cases	5769	3600	5545	699	304	122	232
Numbers Inoculated	287	400	2124	4977	4988	2121	9152
Left The Town	–	–	1843	1537	–	–	262
Escaped Disease In Town	–	–	174	519	–	–	221

This Table reveals the extent of protection against smallpox: by the end of the century in 1792, there were only 232 cases of natural smallpox as against the 9152 people inoculated. 262 people chose to leave the town rather than be inoculated, and there were 221 people who stayed in the town but escaped the disease. In terms of chronology, it is

interesting to note that as in England, there was fairly significant increase in the practice of inoculation at the beginning of the 1750s. In Boston, however, the major growth of inoculation occurred in 1764 – before the Suttonian innovation of technique. Although the Suttons had exported their method to Boston through partnership arrangements, the evidence is that popular inoculation arrived before their innovation in technique and method.

It would appear that an improvement in method had taken place between 1730 and 1752 in Boston – the mortality rate amongst the inoculated dropped from 3.0 per cent to 1.4 per cent[350] – and this may have been associated with the lighter method of inoculation advocated by Ranby, Sloane and Kilpatrick. But this evidence on mortality amongst the inoculated is unreliable as far as Boston is concerned. Inoculation was only practised after smallpox had broken out in the town – it was forbidden by law at other times, at least in the later period – and many of the deaths amongst the inoculated were almost certainly due to a delay in inoculation. This would explain why the mortality rate amongst the inoculated in 1792 – 2.0 per cent – was as high as when inoculation was first introduced in 1721 (2.0 per cent).[351] Notwithstanding this mortality amongst the inoculated, we will see later that inoculation very dramatically reduced the total number of deaths from smallpox in Boston.

Although inoculation was introduced into Scotland at about the same time as it was in England, it grew much more slowly in the former. 5553 persons were known to have been inoculated in Scotland by 1764,[352] which is very much smaller, even taking into account relative sizes of population, than the estimated 200,000 inoculations in England by 1766. Monro, who drew up the account of inoculation in Scotland, summarized the position in 1764:

"The greater number of the gentry, and most of the medical gentlemen . . . have their children inoculated; but the . . . tempting of Providence, weighs more among many of the populace, who will not allow the small-pox to be artificially implanted."[353]

Calvinist theology again provided arguments for the opponents of inoculation, although from evidence about to be considered, other factors were probably of greater importance in retarding the practice of inoculation in Scotland. The differential spread of inoculation in Scotland and England was reflected in a statement by Aberdour, an Edinburgh physician, in 1791:

"It is now about seventy years since inoculation was practised in England, and sixty in Scotland, and though it is now become general, still there are many individuals who will not permit inoculation; and many objections are made to the practice, especially by the lower class of people in North Britain."[354]

Fortunately, we are in a position to assess this statement in some detail as far as Scotland is concerned, for many incumbents discussed inoculation in their parishes in their returns compiled for the *Statistical Account of Scotland* during the 1790s.[355] Of the 243 incumbents who discussed inoculation, 162 said it was widely practised in their parishes, as against 91 who said that it had still to become general. Examples will best illustrate this difference. In 1792 the incumbent of Durrinish, Skye, wrote:

"this increase in population may be attributed . . . above all, to the inoculation of the smallpox, which has been universally practised in this island for thirty years past, and has been the means of preserving many lives."[356]

This may be contrasted to Eaglesham where, according to the incumbent in 1792

"the smallpox carry off great numbers of children; but there is no reconciling the minds of the lower ranks to inoculation."[357]

Most incumbents who stated that inoculation was not general in their parishes also attempted to explain why. One recurring explanation given was as follows:

"The notions of absolute predestination, which are still deeply rooted in the minds of the country people, lead the generality of them to look upon inoculation as implying an impious distrust of Divine Providence, and a vain attempt to alter its irreversible decrees."[358]

This religious objection was perhaps a generalisation of a more concrete attitude, that "the thought of bringing trouble on their children as they call it, with their own hands, outweigh every argument that can be advanced in its favour."[359] This was one of the initial major reasons why parents were reluctant to have their children inoculated in England, and it is of interest to examine why this attitude persisted in Scotland much more than it did in England. The Calvinist religious belief in predestination no doubt buttressed this attitude, but perhaps more important was the age incidence and nature of smallpox epidemics in Scotland. Monro had noted in 1765 that

"The inhabitants of Scotland generally have the smallpox in their infancy or childhood; very few adults being seen here in this disease."[360]

Monro was somewhat puzzled by this, but suggested that it may have amongst other things been due to the fact that "in the villages the peasants are generally assistant to their neighbours of whose family any is sick" and did not "fly from the place where it [smallpox] is" as they did in England.[361] The periodicity of smallpox epidemics was, however, partly a function of the geographical situation of a place and areas such as the Western Isles are known to have been free from smallpox for very long periods of time. The same was probably true of the Highlands, and it is no accident that inoculation was almost universally practised in such areas. For example, the incumbent of Portingal in Perthshire wrote in 1792 "that fewer children die in the Highlands than almost anywhere, particularly since inoculation has been universally practised, which it has been, for a good many years back, to the saving of many lives."[362]

Areas where epidemics were infrequent were likely to respond in panic to the threat of an epidemic (as they did in England), and perhaps this was the most important factor in determining the rate of

spread of inoculation in different parts of Scotland. However, several incumbents noted that even in the areas where inoculation was not general, "the people entertain no prejudice against inoculation, but grudge the expense of it."[363] Unlike England, there was no organised system of poor relief at the parish level which could have been used for free inoculation of the poor, although Sinclair claimed that this problem was overcome in large measure through charitable inoculation paid for by the local gentry, free inoculation by the medical profession and even the practice of inoculation by ministers of the Church. The relative lag of inoculation in Scotland compared to England was reflected in the acceptance of vaccination in Scotland:

"it [vaccination in Edinburgh] has been much more generally adopted by the lower orders of the People, than ever the inoculation for the Small Pox, and they [the Royal College of Physicians of Edinburgh] believe the same to obtain all over Scotland."[364]

Some parents still objected "to the production of any disease among their children", but it appears that vaccination was more popular in Scotland than in England during this period, indirectly confirming the conclusion that inoculation was less general in the former than in the latter by the end of the eighteenth century.

Inoculation was introduced into Ireland in 1725 but was only very sporadically practised until the advent of the Suttonian method. Little is known of the exact chronology of its practice. We have seen previously that the Suttons appointed several partners in different parts of Ireland, and Houlton noted in 1768 that of the imitators of the Suttons, "some, I am informed since my arrival in Ireland, are now travelling over several parts of the kingdom . . ."[365] This probably was the beginning of the practice of itinerant inoculation in Ireland which was to become very important. There were very few hospitals or doctors in the country at this time, and this was related to the vast majority of the Irish population living in isolated hamlets and scattered cabins.[366] Some inoculations were performed by county infirmaries,[367] but most were performed by "individuals [who] proceed from village to village several times during the year for the purpose of inoculating the infantile population."[368] We are fortunate

to have a detailed description of one of these itinerant inoculators; a Frenchman, De Latocnaye, gave the following account of a meeting which took place during 1796-97:

"In the mountains (of Mayo) I fell in with a man who had the air of being something of a *bon-vivant.* He told me that his profession was that of an inoculator, and that he was about to inoculate the children of the peasantry in this wild country. He assured me positively that of 361 children inoculated by him this year only one died. When it is understood that if he has been unfortunate enough to have a child die on his hands, not only is he not paid, but he must escape promptly in order to avoid a beating by the afflicted parents, it will be seen that the poor devil must take great pains with his patients. But for the death of his patron, the inoculator, who was born in the area, would have become a priest. It was just the time when inoculation had begun to be put into practice and the terrible effects often produced by smallpox on these mountain folk gave him the idea of visiting them and taking up the profession of inoculator, after he had taken some lessons in the hospitals. Now he has practising with success for thirty or forty years, but all he makes by way of income is not more than thirty or forty pounds sterling per annum. On the Continent, not only would the peasants refuse to allow their children to be inoculated, but even people comfortably off would make a like refusal. In England well meaning proprietors are often obliged to beg the parents to submit; in Scotland they have not yet succeeded in securing the adoption of the method, and yet it is generally adopted in Ireland even in its wildest parts."[369]

There may have been a note of exaggeration in some of De Latocnaye's statements – we have just seen that inoculation was widely practised in some parts of Scotland – but the overall emphasis on the universality of the practice in Ireland (and its almost total neglect on the Continent), was almost certainly correct. James Moore, the first director of the National Vaccine Establishment, agreed with De Latocnaye's conclusions while trying to explain why the Irish were so reluctant to accept vaccination:

"Variolous inoculation was formerly patronized in Ireland by the Popish Clergy, and had, therefore, been much more generally adopted by the common people, than in any other country. The degree of security which this afforded, rendered many unwilling to try a new plan . . ."[370]

The emphasis on the importance of religious influence was probably misplaced; while it may have been a factor in the popularity of inoculation in Ireland, the most important variable was almost certainly an epidemiological one: the remoteness of most Irish communities, and thus the epidemic nature of smallpox when it struck a particular area.

The resistance of the Irish to vaccination led to a number of accounts – both current and retrospective – of the activities of the inoculators. For example, one opponent of inoculation described in 1807 itinerant inoculators as follows:

"Variolous Inoculation had been long, almost exclusively, in the hands of a particular branch of the profession, whose prejudices and interests were strongly opposed to the new practice [of vaccination]; and by their being the usual medical attendants in families, and especially in the diseases of children, their opinions had greater effect upon the minds of parents."[371]

These Inoculators were elsewhere described [in 1807] In more pejorative terms as "the lower class of apothecaries", "Quacks travelling about the country" and "some Quacks and old women."[372] It appears that the apothecaries concentrated on the towns and the "Itinerant Quacks" on the countryside. This was not all that dissimilar to England during the same period (1807), except that the "empiric" inoculators in Ireland were necessarily itinerant. It is clear that inoculation was very extensively practised in Ireland, for as the Royal Dublin College of Physicians reported in 1807:

"The Small Pox is rendered a much less formidable disease in this country by the frequency of Inoculation for it . . . hence parents, not unnaturally, objected to the introduction of a new disease

[vaccination], rather than not recur to that [inoculation], with the mildness and safety of which they were well acquainted."[373]

This conclusion is confirmed by other independent evidence; the Reverend H. Townsend described in his *Statistical Survey of the County of Cork* published in 1810, "the universal custom of inoculating children for the smallpox."[374] Although there is no information about the chronology of the practice of inoculation between 1788 and 1807, it appears from remarks made on the subject in the latter year it had been practised for some considerable time, e.g. Dr Castle wrote from the area of Derry in 1807:

"And it is remarkable that altho' this latter inoculation [vaccination] is but sparingly practised in the neighbouring country, the former variolous inoculation is not followed as much as it was won't to be amongst the country folks a few years ago . . ."[375]

Inoculators seemed to have practised in all parts of the country, for a summary of all the letters sent to the Royal Dublin College of Physicians in 1807 stated that "Quacks travelling about the country and inoculating with variolous matter . . . are mentioned in the letters which have been received on the subject,"[376] and practically all letters quoted mentioned them. Inoculation continued to be practised in Ireland until at least 1875 (particularly in rural areas), and Sir William Wilde noticed the extensive activities of the inoculators as late as 1851.[377] Ireland like England, was one of the few European countries in which vaccination was not generally practised during the first forty or fifty years of the nineteenth century, and members of the Irish medical profession explicitly linked this with the near universal practice of inoculation.

Other than the places considered, inoculation appears to have been practised only on a limited scale in Europe. Dimsdale, who had personal experience of inoculation in Continental countries (e.g. Russia and Austria) stated in 1778 that "it is extremely probable, more persons of late years have been inoculated here [in England], than in all the rest of Europe."[378] We saw earlier that it was used regularly and extensively in the Hague and at Geneva. Inoculation was strongly encouraged in some countries (Sweden, Russia and

Austria)[379] and apparently hardly practised at all in others (Spain).[380] The relative lack of popularity of variolation on the Continent was reflected in the willingness of most of these countries enthusiastically to embrace vaccination, although ironically, vaccination in the earlier part of the nineteenth century was probably only a more attenuated form of the old inoculation.

CHAPTER 7
The Reliability Of Smallpox Mortality Registration

The mortality from smallpox is substantially determined by the type of virus responsible for the disease, and as we have already seen, although there were only thought to be two major variants – variola major and variola minor – it now appears that there is a whole spectrum of viruses, ranging from the very mild to the very severe. Smallpox is fairly easily recognizable from its skin lesions and other symptoms with a distinctive chronology in their appearance, although Dixon has pointed out that

"It cannot be too strongly emphasized that in the 'difficult' case of smallpox the skin lesions may closely resemble those occurring in other diseases, but the timing of their appearance in relation to the general symptoms determines whether the disease can or cannot be smallpox."[381]

Although the timing of the appearance of symptoms enables smallpox to be distinguished from other diseases in which skin lesions occur, it occasionally happens that smallpox is confused with these other diseases, particularly measles and chicken-pox. This type of confusion is illustrated by an account of an unsuccessful inoculation given by Dimsdale:

" . . . a general inoculation having been performed in a parish in the country . . . they judged themselves, they were safe from any danger from the smallpox – some made the trial, and went into houses where the real smallpox ranged; this trial cost them dear, for I believe most, if not all, fell with the real smallpox, and died. On a strict enquiry it was reported, that the matter used in inoculation was taken from a subject having the chicken-pox."[382]

This type of mistake must have been very rare, for it is the only example that has come to light from an examination of historical records used in this research. The main reason for the ability of contemporaries during the eighteenth century to recognize smallpox

was its epidemic nature in most parts of the country, where it returned regularly every few years. Under such conditions smallpox must have been very easy to recognise.[383] In large towns, however, it would have been more difficult because of the endemic nature of the disease; for example, in London smallpox deaths were recorded every week during the seventeenth and eighteenth century.[384] In such a situation it would be possible to confuse individual cases of smallpox with other diseases such as measles and chicken-pox.

There is one form of smallpox that has only recently been recognized, as it does not give rise to the eruption of lesions, the main symptom by which smallpox has been historically recognized. It has been labelled Fulminating Smallpox (Purpura Variola), and is of sufficient importance to describe it at length. Dixon has given the following account of this form of the disease:

"After an incubation period of about eleven to twelve days the patient is suddenly taken ill, with a feeling of intense prostration accompanied by severe headache, and often backache. In spite of these symptoms the patient is very 'wide awake' and peculiarly apprehensive. In some patients the infection is so overwhelming that death may occur within twenty-four to thirty-six hours with no outward manifestations at all . . . At post-mortem there may be a few haemorrhages in the submucosa, in the respiratory and alimentary tracts and in the heart muscle, the latter possibly contributing to death in some cases. The appearances are very indefinite, with no findings on which to base a certain diagnosis . . . This is 'sledgehammer' smallpox, and the diagnosis both clinical and at autopsy is impossible unless smallpox is thought of and unless laboratory facilities are available and used to grow the virus to detect soluble antigen in the blood during life, or after death. If the patient survives more than forty-eight hours there is often a slight but temporary improvement in the general condition, followed by the appearance of an erythema on the face and back of the hands, and a blotchy erythema on the arms and trunk, particularly the anterior abdominal and upper part of the thighs . . . The erythematous areas of the skin will reveal petechial which during the next twenty-four hours rapidly enlarge forming ecchynoses of a peculiar bluish-purple colour . . . but just before death,

which occurs within forty-eight hours of the onset of these haemorrhages, the whole body may be affected. When the haemorrhages occur only in the 'bathing drawers' area, a confident diagnosis of smallpox can be made; but when the haemorrhages are more general, as is common, the picture has no completely characteristic features to distinguish it from other hyper-acute infections, although in smallpox there is a greater tendency towards symmetry . . . With the appearance of haemorrhages in the mucous membrane or skin the patient's life may be terminated by massive haematenesis, intestinal or uterine haemorrhage . . . From the diagnostic point of view it cannot be overemphasized that the absence of any vescular eruption is the main feature of this condition and increases the difficulty in differentiating it from other acute haemorrhagic catastrophes. Mortality 100 per cent."[385]

This form of smallpox probably occurs for two major reasons: (1) the virulence of the smallpox virus; (2) the defencelessness, weakness or lack of resistance on the part of the host. It tends to attack most commonly at the extreme ages of life (young infants and old people),[386] and is particularly manifest when smallpox attacks a community which has been free of the disease previously, such as the American Indians, whole tribes of which were wiped out by smallpox during the seventeenth and eighteenth centuries.[387] The vulnerability of the extremes of age is probably due to constitutional weakness, whereas populations like the American Indians suffered from a lack of antibodies and an absence of genetically acquired resistance to the disease.

How does the existence of fulminating smallpox affect this study? Firstly it appears that the haemorrhagic fulminating form was sometimes recognized as smallpox. As early as the seventeenth century, Sydenham described this form of smallpox:

"this summer . . . the pox was in many apt to turn black and there would appear blew spots upon the skin . . . Purple spots . . . declare the great malignity . . . with the certainty of the patient's death."[388]

Later, at Kendal and the surrounding area in 1772 there prevailed

"a false species of the smallpox, which has carried off more than 700 people. The affected were at first taken with a very uncommon bleeding at the nose, and generally expired immediately after the first stage of the disorder, which was so putrid in nature, that the whole body of the deceased was covered with large purple blotches, and was exceedingly offensive even before dissolution."[389]

This disease was presumably recognized as smallpox because of the presence of more ordinary forms of smallpox in the area. In other cases, however, fulminating smallpox may have been thought to be another disease. Possibly an example of this is an epidemic which broke out in Norway in 1741:

"It must have been a highly dangerous type of typhus which in some unknown way had gained a foothold in Norway in the autumn of 1741. In a vivid description of the course of the disease, Hanneria described it as 'quite as infectious and fatal as the plague itself'. Death often came with a haemorrhage."[390]

There was certainly an epidemic of smallpox in neighbouring Sweden at the same time, which would tend to confirm the suspicion about the disease in Norway. Although contemporaries noted that a very severe haemorrhagic disease was somehow associated with smallpox, they did not always recognize it as a form of smallpox. Thomas Short writing in 1749 on the history of epidemics in England stated:

"In 1670, 71 and 72 reigned an irregular Small Pox . . . this gave way to the Bloody Flux, and returned again when that was out."[391]

It seems possible that this 'Bloody Flux' was a form of fulminating smallpox, and as the bloody flux was considered a separate disease and registered as such, it is likely that some smallpox mortality arising from the fulminating disease went unrecognised. Contemporaries were aware of the under-registration of smallpox mortality because of the fulminating nature of the disease amongst very young infants. It was recognised that smallpox killed many young children (particularly under the age of six months) before the usual

eruption of pustular lesions, and such deaths were invariably accompanied by convulsive fits; for example, Dr Percival wrote in 1768:

"A considerable number of those who die of the natural disease, before the expulsion of the variolous eruption, are infants or very young children . . . Hence the convulsive paroxysms which often precede the appearance of the pustules . . . are always alarming, and when they happen to very young infants are frequently fatal."[392]

This conclusion about the effects of smallpox on young infants was confirmed by the experience of inoculating the same age group; one of the deaths of the inoculated cases enumerated by Jurin during the 1720s was "Adam Urquart, Son of William Urquart, Esq., of Meldrum aged One Year and a half, [who] was inoculated at Meldrum in Aberdeenshire, August 29, 1726, sicken'd the Seventh Day, and died the Eighth Day before any Appearance of an Eruption, of Fits . . ."[393] Most deaths from smallpox without regular pustular eruptions and accompanied by convulsive fits appear to have occurred amongst infants under the age of five months. Monro in his account of inoculation in Scotland described the effects of inoculation amongst children and the importance of the age of the child:

"Several, considering how much more liable very young children are to convulsions (the most frequent dangerous symptom in the inoculated smallpox) than those farther advanced in life, decline performing inoculation in very young infants . . . More of the patients who died of inoculation were killed by convulsions, near the time of eruption of pimples, or upon their subsiding on the second or third day after their first appearance, and by erysipelatous or rashy eruptions, with spasms when the smallpox were blackening, than by any other cause . . . Of twelve infants, inoculated within a fortnight of their birth, not one had the smallpox; but in some of them a rash appeared about the time when the varlolous eruption used to be seen – Children five months old, inoculated at the same time, and with matter from the same subject, had the smallpox in the regular manner."[394]

The implication of Monro's description is that the majority of the inoculated died under the age of five months from a form of smallpox which may have been of the fulminating type. There are other descriptions of deaths of inoculated infants which confirm this interpretation,[395] but with the onset of post-Suttonian inoculation these become difficult to find as the much safer method of inoculation led to very few deaths even amongst infants. Although the existence of large numbers of deaths from smallpox amongst young infants was frequently noted by contemporary medical writers, this recognition did not necessarily lead to registration, unless accompanied by the classical symptoms of smallpox. Haygarth pointed out that

"The disease most fatal to infants is convulsions, arising from various causes; one of them is the small-pox. The two circumstances will explain the reason why, under one year old, the proportion of deaths by the small-pox is less than in subsequent periods . . ."[396]

This conclusion is confirmed by the available statistical evidence on smallpox mortality amongst different age groups of young children during the eighteenth century:

Number Of Children Dying From Smallpox At Different Ages

	0–5 Months	*6–11 Months*	*1–2 Years*
Manchester, 1768–1774[397]	21	119	226
Warrington, 1773[398]	8	39	84
Chester, 1774[399]	7	44	38
London, 1774–1796[400]	6	18	44

These figures reveal that there were very few infants under the age of six months dying from smallpox, and one possible explanation is the non-registration of smallpox deaths due to the fulminating form of the disease amongst this group. Another explanation, for which there is

good evidence, is that infants are born with a form of natural immunity – the exact nature of which is unknown[401] – which protects them from attack by smallpox for four or five months after birth, until this immunity declines with age. In spite of this type of natural immunity, it appears that infants over the age of two weeks were often successfully inoculated, although it was recognized by contemporaries that this was a little hazardous on account of the danger of complications such as convulsions. Probably both explanations discussed are relevant to infant deaths: the naturally acquired immunity at birth protected against attack from smallpox in many cases, but where the disease managed to penetrate this immunity barrier either through inoculation or the forms of natural attack described by Percival, it struck in a fulminating manner, producing convulsions and the rapid onset of death.

Most of these fulminating infant deaths probably were not registered as being due to smallpox, and this must have been a significant factor in the under-registration of smallpox deaths in large towns like London, where the disease was endemic and therefore struck particularly at young children. It would have been much less important in the countryside where smallpox was an epidemic disease returning every few years, therefore affecting adults as well as children. In Godalming, Surrey for example, where epidemics returned about every thirteen years, of a total of 157 deaths from smallpox during the period 1701-23, 75 were of adults.[402] Only a minority of the total population lived in large towns in England during the eighteenth century, so it might be thought that the under-registration of infants' deaths from smallpox is of only minor importance in the context of the wider study. Unfortunately most records of smallpox mortality are for large towns – smaller towns and villages having no system of registration except for parish registers.

Lettsom estimated that smallpox mortality in London was twice that recorded in the Bills of Mortality, "the generic article convulsions having swallowed up, in his opinion, a large number of the smallpox deaths of infants."[403] Smallpox deaths amongst infants also appear to have been registered under other headings, such as "infants", "chrysoms" and even diarrhoea.[404] It is impossible to know

what proportion of smallpox deaths amongst young infants were registered under other headings. Perhaps Lettsom's estimate is as good as any, for he had a great knowledge of the disease of the poor through his work with the *London Dispensary* and the *Society for General Inoculation of the Poor*. In about 1776 he claimed that "during the last three years, I have attended nearly six thousand poor persons, into many of whose habitations I have entered."[405] Whatever the exact degree of under-registration of smallpox mortality due to its registration under other headings for infants, it is a factor to be borne in mind when the records of smallpox mortality in large towns are later discussed.

There is one general problem in attempting to estimate the contribution of any one disease to total mortality, and that is the indirect mortality which cannot be specifically attributed to that disease. Peter Newman in his study *Malaria Eradication and Population Growth* concluded that "deaths actually reported as due to malaria constituted only about one-fifth of all those which would have occurred if malaria had not been present."[406] He reached this conclusion by comparing regions in which malaria had been eradicated with those in which there had been little attempt to deal with the disease. This kind of analysis is only possible where there are good statistics of mortality (both total and from specific diseases) in different types of region, and even where this type of data is available, its interpretation often becomes problematical (there has been considerable dispute about the validity of Newman's conclusions). Also, it is not possible to analyse smallpox mortality during the eighteenth century in this way as the relevant statistics are not available. Although Newman's method of analysing the contribution of a specific disease to total mortality is not applicable to our problem, his findings at least suggest that the indirect mortality arising from a disease can be very significant.

A similar conclusion was reached by some contemporary commentators on smallpox. Jurin wrote in 1724 that it was "notorious that this Distemper [smallpox] frequently occasions other Diseases, of which the Patients die of a considerable Time after."[407] Similarly Lettsom noted over fifty years later in 1778:

" . . . there is reason to believe that nearly double the number die annually in the metropolis of the Natural Small-Pox, more than the bills of mortality ascertain . . . some have been deprived of sight; many have been afflicted with the evil and scrophulous complaints, to which they had been previously strangers; many have been disabled in their limbs . . . and more still have languished under hectic symptoms, and at length, emaciated and totally debilitated, they have sunk under their miseries, and filled up the amazing list of consumptions, many of which originated from the violence of the Natural Small-Pox."[408]

It is impossible to assess Lettsom's estimate of the indirect consequences of smallpox, and again we can only note that he had very extensive experience of treating diseases in London, and that his conclusions appear to have been universally accepted by his contemporaries. For example, Watts quoted the following conclusion in his pamphlet on inoculation published in 1767:

"After the distemper [smallpox] is over, there follow inflammations of the eyes, foul ulcers, abscesses, swelling of the joints, pulmonary consumptions, decays and the like."[409]

Similarly, Willan wrote in 1800 of the "glandular swellings, ulcers, cutaneous affections, disease of the lungs" which followed small-pox.[410] Although most of these secondary complications would have affected mortality indirectly, it does appear that delayed death from smallpox was occasionally recognized in the registration of smallpox mortality; in the list of people dying from smallpox in Godalming, Surrey:

"early in ye Morning Dyed Lawrence Kern's wife of Ffarnecomb after a longe illness when ye smallpox was off from her but was not well after it till she dyed."[411]

Modern scientific studies of secondary diseases arising out of smallpox confirm in overall terms the conclusions of Lettsom and other eighteenth century doctors who wrote on the subject, although because

these studies are based on clinical and post-mortem findings, they are unable to provide any statistical measure of the effects of these illnesses on mortality. Dixon has listed the following as secondary causes of death: (a) bronchopneumonia; (b) streptococcal septicaemia; (c) staphlococcal septicemia; (d) pyaemia; (e) multiple staphylococcal abscesses; (f) osteo-myelitis; (g) empyaema.[412] Many of these illnesses of course arise through environmental conditions, particularly poor hygiene, and this would have been a more severe problem in the pre-antibiotic era. By far the most serious of the secondary infections appears to have been broncho-pneumonia. Councilman and his colleagues published their findings in 1909 on the autopsy of fifty-four victims of smallpox as follows:

"Some degree of bronchitis and broncho-pneumonia was found in fifty-four cases. It did not differ from the forms of broncho-pneumonia so commonly seen in diptheria . . . These lung lesions were found in all stages of the disease, from the earliest to the latest. We should probably regard them as probably the most common and the most serious complication in small-pox. In many of the cases the lesions of the lung were so marked that they constituted a sufficient cause for death of the individual without the accompanying specific infection."[413]

Autopsy analysis cannot tell us of course what the outcome of an attack of smallpox would be on those who immediately survive the onslaught of the disease, although given the almost universal incidence of bronchitis and broncho-pneumonia among Councilman's cases and the emphasis that eighteenth century writers on smallpox placed on subsequent secondary mortality from lung diseases, it is difficult to resist the conclusion that indirect mortality from smallpox may well have been as high as Lettsom and others claimed. All these writers emphasized consumption as a secondary outcome of smallpox, and this is compatible with what is now known about the triggering of latent tuberculosis infection by other diseases. The tuberculosis bacillus can lie latent within human cells for very long periods – and then be released by a viral infection such as smallpox, which disrupts the structure of the cell.[414]

Although this clearly is a topic which requires further specialized work before any firm conclusion can be reached, it does suggest that Lettsom and his contemporaries may have been right about the scale of secondary mortality from smallpox.

That smallpox is capable of producing serious long-term complications is illustrated by what we now know of its effect on the epididymis in males. Councilman and many other workers found much evidence of focal lesions in the testes and the epididymis through post-mortem analysis; of thirty cases studied in Councilman's sample, thirteen were found to have testicular lesions.[415] The most detailed work on this subject was carried out by Chiari, and Councilman summarized his findings on testicular lesions as follows:

"He found the lesions in fifteen cases of children, and in a further examination of sixty-three cases, mostly of adults, lesions were found in forty-five. The lesions showed a perfect agreement in their stage of development with the skin lesions . . . Chiari regards the affection as due to the direct action of the small-pox virus carried to the tissue by the blood. He thinks that in the testicle the conditions for action of the virus may be just as favourable as in the skin. The lesions show in their histological details similarity to the skin pocks."[416]

The similarity of skin lesions with those found in the testes was confirmed by Bras as recently as 1952; he found that

"The testicular lesions correlate with those of the skin; as a rule they occur after the onset of the vesicular stage and they can still be present when the skin lesions have healed."[417]

Although the persistence of these lesions in the testes was generally recognized, the possibility of them leading to male infertility tended to be discounted, until a very recently completed study revealed a high correlation between the presence of lesions in the epididymis and infertility.

A. M. Phadke and his colleagues published in 1973 their findings on research involving 8,000 patients who had registered at the Bombay Family Welfare Bureau during the previous nineteen years for

treatment of infertility. I will summarize the conclusions of the study by quoting from it at some length:

"In 895 cases there was history and evidence of prior smallpox infection. These cases (designated the smallpox series) were analysed according to the sperm counts and according to the lesions observed in the testicular biopsies. For the control series, another group of 895 serially registered infertile patients, who had not had smallpox, was analysed in an identical way . . . the incidence of azoospermia [absence of viable sperms in the semen] was 42.57% in the smallpox series and only 17.87% in the control series. Likewise, the incidence of normospermia [normal amount of viable sperm in the semen] was only 30.17% in the smallpox series and as much as 52.52% in the control series . . . In clinical practice, the association of a history of smallpox infection with the occurrence of obstructive azoospermia is of proverbial frequency. The present study bears out the frequent clinical impression that the incidence of obstructive azoospermia is remarkably high in patients who have had the smallpox. Four of five such cases [in the azoospermia category] have obstructive lesions. The site of obstruction is usually at the lower end of the epididymis, and the testes miraculously seem to escape the brunt of the disease . . . In India, at least, obstructive azoospermia produced by smallpox infection in childhood frequently occurs . . . Smallpox infection is the most important and most frequently encountered single etiologic factor in India which produces obstructive azoospermia in man."[418]

I will discuss the demographic implications of these findings in the next chapter – we will see that there is some evidence for a correlation between changing smallpox mortality and changes in fertility. For the present, it is sufficient to note that smallpox is capable of inflicting life-long damage to vulnerable parts of the body, in ways not known about until very recently. It is probable that we shall never know with any exactness the full impact of the disease on overall mortality, given uncertainty about secondary causes of death such as broncho-pneumonia and the lack of research of the kind carried out by Phadke et al on male infertility.

One area on which there is very precise information on smallpox and its effects on mortality relates to pregnant women; it was recognized from a very early date – from at least the beginning of the eighteenth century – that pregnant women were highly vulnerable to the disease.[419] More recently, Rao and colleagues compared the outcome of smallpox in 94 pregnant women with that for a group of 348 matched non-pregnant cases in the Infectious Diseases Hospital at Madras; the overall fatality rate in the pregnant women was three times as high as that among the non-pregnant women, and in the twelve unvaccinated cases amongst the pregnant women, nine – 75 per cent – died, compared to seventeen of the sixty-six (25.7 per cent) non-pregnant women.[420] This scale of mortality among pregnant women who caught smallpox is what might be expected from the historical literature, and it is clear that this demographically critical group was periodically decimated in country areas where the age incidence of the disease would make pregnant women vulnerable. Rao *et. al.* speculate that this vulnerability may be due to the high level of circulating cortieosteroid in the blood of pregnant women, inhibiting the formation of antibodies. There is a fragment of historical evidence that women were also more vulnerable to smallpox during menstruation,[421] but the bearing that this has on Rao's explanation of the vulnerability of pregnant women is unclear.

In addition to the problem of the dark area of unknown mortality due to the indirect effects of smallpox mortality, there is the difficulty that we cannot assume that smallpox was always registered even where it was known to be the primary cause of death. Haygarth pointed out from his experience in Chester "that shopkeepers of almost all denominations, not only neglect every rule of prevention, but, lest their trade should suffer, conceal, as much as possible, every instance of the natural smallpox, which occurs in their families."[422] Although it would have been difficult for such shopkeepers to conceal death from smallpox in their families, they may have on occasions been able to bribe the "searchers" who in places like London were responsible for ascertaining the cause of death and were described in 1783 as "women advanced in years and indigent in circumstances."[423] Many tradesmen in market towns may have suppressed information about smallpox in

their families and certainly the townspeople as a whole were very anxious to avoid advertising the presence of smallpox in their own town so as to avoid frightening country people from the surrounding area. There are many examples of markets being ruined for more than a year because of the presence of smallpox.

According to the local historian of Dartford in Kent, in 1741 "the country people became so alarmed that the market was nearly deserted, and did not recover for some years."[424] Many of these market towns attempted to isolate smallpox cases in the local pest house, and according to Dimsdale "due care is taken to bury the dead [from smallpox] privately."[425] Not all these private burials at the pest house were registered in the parish register. When Mr Thomas Chubbs wrote to Jurin on the 10th August 1723 to give his account of smallpox in the town of Sarum, he stated that in addition to the 192 registered of dying from smallpox, "there have some died of that Distemper, wch have been carried out of town to be buried, of which I can recollect . . . 9, Tho' probably there may have been more."[426] In this case, the proportion of private burials at the pest house does not appear to have been very large, but it is clear that there were occasions when such unregistered burials could be very substantial; for example, the Maidstone parish register mentioned, but did not list by name, 102 children dying from smallpox in Maidstone in 1760 and "buried out of town."[427] As far as I am aware, no scholarly study has ever been carried out on the history of pest houses, and references to private burials are hard to come by, although they do occasionally occur in local histories; for example, at Hitchin in Hertfordshire: "1751. Paid John Person for a board to lay the dead on at the Folly, 1/6" - the Folly being the local pest house."[428] If private burials of smallpox victims were as common as Dimsdale suggested, it could constitute a serious problem when trying to analyse smallpox mortality through parish registers.

It is unfortunate that where most statistical information on smallpox mortality is available – large and market towns – the registration of smallpox deaths is likely to have been most faulty. The inadequacy of registration was generally much greater in the large towns, for reasons which have been discussed, but it is again unfortunate that systematic statistics of smallpox mortality have only

been compiled for large towns where bills of mortality were kept. Some parish registers of market towns do record death from smallpox, but it is not certain whether all smallpox deaths were registered or whether some deaths were not recorded through the casual nature of the registration procedure. Smallpox mortality and its change over time as depicted by statistics derived from bills of mortality and parish registers will be discussed later in the book.

CHAPTER 8

Smallpox Mortality Before The Introduction Of Inoculation

One of the most reliable methods of estimating smallpox mortality throughout the eighteenth century is to multiply the proportion of the population suffering from the disease by its case-fatality rate, so that if 200 out of a population of 400 catch smallpox and 20 out of every 100 catching smallpox die from the disease, ten per cent of the population would have died from smallpox at that particular point of time. In general terms, in order to estimate the scale of smallpox mortality for the country as a whole it is necessary to estimate the proportion of the total population which caught the disease and then multiply by the estimated case-fatality rate for the whole country. This will be done in very general terms in the following discussion, starting with the attempt to estimate the proportion of the total population affected by smallpox.

Contemporaries during the eighteenth century were unanimous that smallpox was a disease that nearly everybody caught sooner or later, "almost every person must have it once."[429] D'Escheruy summed up contemporary opinion on the prevalence of smallpox:

". . . this distemper spares neither Age nor Sex; Rich and Poor are equally exposed to its Influence. What is the most unaccountable, and so wide from all other Fever, is, that the Difference of Constitution is no preservative against its Attack; inasmuch, that very few escape it, at one time or other . . ."[430]

D'Escheruy wrote this account in 1761 which is after the introduction of inoculation, but there is a great deal of evidence that smallpox was virtually a universal disease at a much earlier period. Smallpox appears to have been present in Europe at least since 581 A.D. when Gregory of Tours gave a very detailed description of the disease.[431] It was mentioned sporadically from then on until the late sixteenth century, when statistical evidence is available on its prevalence. Between 1574 and 1598 the parish register of Allhallows, London Wall, contains information on the cause and age of death and during

this twenty-five year period twelve people were listed as having died from smallpox. Ten of these twelve died under the age of seven years, the other two dying at the age of 12 and 30, the latter being a servant who had probably been recruited from the countryside.[432] This age distribution of those dying from smallpox suggests that the disease was sufficiently endemic in London to be a young child's disease. This is of course generally the case, i.e. where the disease returns every year or so it will be almost exclusively a disease of young children. It is extremely rare for smallpox to attack the same person twice (the antibodies produced by the first attack usually protect for life), and therefore very few older people will be affected by it when the disease is endemic.

It appears that smallpox was more or less endemic in most large towns by the early seventeenth century, for it was a young child's disease in these places. According to the Kirk Session records of Aberdeen in 1610 "there was at this time a great visitation of the young children with the plague of the pocks"[433] At Chester it was noted in the parish register for the year 1636 that "for this two or three yeares divers children dyed of small-pox in Chester."[434] From statistics of smallpox mortality during the eighteenth century it is clear that the disease was a very young child's disease in all large towns. Out of a total of 489 deaths from smallpox in Manchester during the period 1768-74, only 30 of them occurred above the age of five, the majority – 366 – taking place amongst children under the age of two.[435] During an earlier part of the eighteenth century, there are statistics on age and smallpox mortality for Kilmarnock, Scotland, during the period of 1728–62: the mean age at death from smallpox was 2.6 years and of a total of 613 smallpox deaths, 563 occurred under the age of five.[436]

It has sometimes been assumed that smallpox was a child's disease in all parts of Britain by the early part of the eighteenth century. We have previously discussed Monro's statement that smallpox was mainly a disease of children in Scotland and attempted to explain why this was the case, in terms of the Scottish peasantry's custom of visiting each other's family when a member was sick with smallpox. The English seem to have been much more careful when the disease was present in the vicinity and there is

much evidence of the extreme lengths to which some people would go to avoid catching it; for example, a member of the Purefoy family who wrote to the Postmaster at Cheltenham in 1742:

"I having occasion to drink your waters at Cheltenham am oblidged to write to you, the Postmaster, to let me know if the small pox be at Cheltenham, if not I shall be there soon after I have your answer . . ."[437]

There is evidence of brothers refusing to attend the funerals of sisters because they had died of smallpox and brothers refusing to visit brothers because they had fallen sick of the disease.[438] We have seen how anxious the authorities of market towns were to deny rumours that smallpox was in the town because such rumours would ruin trade. The fear of smallpox not only led to the avoidance of places where the disease was present but also a fairly rigorous policy of isolation. Given the nature of smallpox, this is not at all surprising; what is more puzzling, is that the Scots do not appear to have shown the same fear of the disease at least if Monro's account is to be accepted – and there is reason to believe that the Swedes had a very similar attitude. Sweden was one of the few countries in the eighteenth century with national statistics on smallpox, and it is therefore important to see what we can learn from this data about the universality of the disease. The following is the age incidence of the yearly average number of smallpox deaths in the period 1771-1798:

Yearly Average Number Of Smallpox Deaths At Various Ages, Sweden, 1774–1798[439]

Age Group	*Under 1 Year*	*1-3 Years*	*3-5 Years*	*5-10 Years*	*Above 10 Years*	*All Ages*
Average Smallpox Deaths	1137	1223	870	585	306	4131
Percentage Distribution	27.5%	29.6%	21.1%	14.2%	7.4%	99.8%

Only 7.4 per cent of smallpox deaths were over the age of ten, and the vast majority – 78.2 per cent – were under five. What is very surprising about these figures is that Sweden was a very rural country at this time, with much of its population living under very isolated conditions. It is possible that some of the older members of the population had been inoculated previous to the compilation of the statistics, although the little evidence that is available does not support this.[440] The figures as they stand indicate that smallpox was a universal disease with everyone more or less catching the disease as a young child. Although we do not have similar national figures for Britain, the evidence we do have strongly suggests that smallpox was not exclusively a disease of young children in the first half of the eighteenth century. In Sweden epidemics seemed to have occurred every five years or so; in many parts of England, epidemics appeared at very much longer intervals, as indicated in the following Table summarizing evidence from a number of local sources.

Periodicity Of Smallpox Epidemics In Provincial England

Place	*Date Of Epidemics*				
Skipton-in-Craven, Yorkshire[441]	1716-1717	1723	1726-1727	1732	1736
Maidstone, Kent[442]	1734	1741	1745	1753	1760
Taunton, Somerset[443]	1658	1670	1677	1684	
Sherbourne, Dorset[444]	1634	1642-1643	1649-1650	1657-1658	1667
Godalming, Surrey[445]	1672	1686	1701	1710-1711	1723-1724

The periodicity of epidemics in these places varied from a minimum of about every five years in Skipton-in-Craven to a maximum of about every 12 years in Godalming, Surrey, with the other places occupying an intermediate position. The factors determining the periodicity of epidemics were the total size of the population of the place and its degree of geographical isolation from the rest of the

country. A small relatively isolated village is likely to have had epidemics only infrequently, and when such epidemics did occur they would affect the majority of the population, adults as well as children, as many children would grow to adulthood without being attacked by the disease. An example of this is to be found at Aynho, Northamptonshire, which was a village with a population of about 350 in 1723-24 when an epidemic occurred:

Age Incidence Of Smallpox In Ayno, 1723-24[446]

Ages (Years)	*0-10*	*10-20*	*20-30*	*30-40*	*40-50*	*50-60*	*60+*
Smallpox Cases	28	47	25	12	10	4	6

Over forty per cent of all smallpox attacks in this epidemic occurred amongst adults of 20 years and above; this age distribution can only occur when epidemics return relatively infrequently. The exact periodicity of epidemics in Aynho is unknown, but it must have been very similar to that in Godalming, Surrey, where 81 of the total of 151 people who died from smallpox in the period 1701-24 were children, i.e. epidemics returning about every twelve or thirteen years. Not all the population in Aynho were attacked during the 1723-24 epidemic but this was probably because many of them had caught the disease in previous epidemics. There is some statistical information as to the proportion of the population which escaped from an attack of smallpox and had not had the disease.

Smallpox Census Of Ware, Hertfordshire In 1722[447]

Number Of Families	*Number Of People*	*"Had Ye Smallpox Before"*	*"Had Ye Smallpox This Time"*	*"To Have Smallpox"*	*Died Of Smallpox*
564	2515	1601	612	302	72

It is important to note that Mr Anthony Fage, a surgeon at Ware who compiled these statistics in order to answer the enquiry sent out by Jurin about smallpox mortality, included the 302 people who escaped smallpox and had not had it previously under the heading *"To Have Smallpox"*. He assumed that they would catch the disease sooner or later, not an unreasonable assumption in the light of the statistics returned: a total of 684 people caught the disease in 1722, 1601 had caught it previously, leaving only 302 who had not been attacked. It is known that very few people are immune from attack by smallpox, and if frequently exposed to the disease (as all people must have been in Ware) virtually all will catch it. Another example of a similar smallpox census is that carried out by Frewen, a surgeon, after the smallpox epidemic in Hastings during 1730-32:

Smallpox Census Of Hastings, Sussex 1730-32.[448]

Recovered From Smallpox (Including Four That Were Inoculated)	*Died Of It*	*Escaped It*	*Died Of Other Diseases Since The Smallpox Raged There*	*Total Population*
608	97	206	50	1636

During this epidemic in 1730-32 over 700 people caught smallpox (of which 97 died) and at least 725 had caught the disease previously – only

206 escaped it. The number catching the disease was nearly half the total population, which suggests that epidemics were relatively infrequent in the town, but it is clear that most of the population were prone to smallpox sooner or later.

Although the conclusion that smallpox was a universal disease before the introduction of inoculation would seem obvious from the evidence so far considered, there is some evidence to the contrary. The parish register of Riseley, Bedfordshire, lists all the causes of deaths between 1690 and 1742 and there appears to have been no epidemic of smallpox at all during this period. There were one or two smallpox deaths every five years or so, in all a total of 27 deaths from smallpox during the whole period. This might be partly due to the fact that no cause of death was given for "infants" who constituted over a fifth of the total deaths – some of these "infants" might have died from smallpox; alternatively many of the smallpox victims might have been buried privately at the local pest house. It is possible that either the spread of smallpox was prevented by successful isolation of those sick or that the case-fatality rate was particularly low in this parish – the latter seems unlikely, as we shall see in subsequent discussion. It is also difficult to see how smallpox cases could have been so successfully isolated over such a long period of time, particularly in the light of the pattern of smallpox mortality summarized in the following table:

Smallpox Deaths In Riseley, Bedfordshire, 1690-1742.[449]

Date	1690	1701	1702	1703	1710	1714	1715
Smallpox Deaths	4	1	5	2	1	1	3
Date	1716	1720	1722	1724	1725	1732	1740
Smallpox Deaths	2	1	2	1	2	2	1

It might have been expected that the presence of smallpox in the parish during the periods 1701-03 and 1714-16 would have led to at least one significant epidemic. Riseley is estimated to have had a population of 425 in 1671 and an enumerated population of 576 in 1801.[450] Such a

village could normally expect an epidemic every ten years or so, if other statistical evidence is anything to go by. Even in so isolated a parish as the peninsular Isle of Purbeck in Dorset – described by the local incumbent in 1803 as "my insulated parish and neighbourhood" – appears to have had smallpox epidemics about every twenty years, although during the previous forty years to which the incumbent was writing, the parish had been affected by smallpox, "once by infection, and once by inoculation."[451]

There are very good theoretical grounds for believing however that if epidemics did not occur at all during a very prolonged period – say the fifty-two year period 1690-1742 considered in Riseley – the population would become highly vulnerable to a disease like smallpox, leading to eventual high mortality. Populations that are not affected by fairly frequent epidemics that kill off a proportion of their members, become vulnerable to future attacks through a lack of antibodies and the survival of people with low natural resistance to the disease. Frequent epidemics kill off the biologically vulnerable, who are therefore unable to pass on their genes to future generations; there is therefore a process of natural selection at work, with those highly vulnerable disappearing from the population. This theoretical expectation is more than borne out by the historical literature. Communities which were very geographically isolated suffered from very infrequent epidemics, which when they did arrive, were very fatal. An example of this occurred in Greenland in the first half of the eighteenth century:

"Smallpox was first brought to Greenland, in the year 1734, by a vessel from Denmark. Nearly two-thirds of the whole population of that country (which at that time was from 6000 to 7000) were swept away by this disease. Of 200 families living within a circle of from two to three miles from the Danish settlement into which the smallpox was brought, not 30 remained alive."[452]

The only similar case which is known to have occurred in Britain is that when

"in 1720, the disease was so fatal as to be distinguished by the name of the mortal pox. On this occasion tradition tells us, in the remote Island of Foula, probably inhabited by about two hundred people, it left only four to six to bury the dead."[453]

The Island of Foula is one of the Shetland Islands and is naturally isolated from the Scottish mainland, being eighteen miles from the nearest land. There are no examples of other spectacular fatalities on this scale to be found in the British literature, although Holwell in his treatise on Indian inoculation noted a similar phenomena for the isolated island of St Helena (presumably the same remote Atlantic island that Napoleon was exiled to):

"It is singularly worth remarking that there hardly ever was an instance of a native of the Island of St Helena, man or woman, that was seized with this distemper in the natural way (when resident in Bengall) who escaped with life; although it is a known fact the disease never yet got footing upon that Island."[454]

The very high fatality of smallpox amongst the American Indians is also well-known,[455] and the same process of a lack of antibodies and high genetic vulnerability was presumably at work in all these instances.

The absence of highly fatal epidemics in Britain except for that found in Foula suggests that the population was periodically affected by the disease, either in endemic forms in the large towns or in epidemics in the countryside. Practically everybody sooner or later appears to have been infected by the disease, and this is reflected in the contemporary literature of the period. As early as the beginning of the seventeenth century Ben Jonson wrote in his "Epigram to the Small Pox" – "Envious and foule Disease, could there not be One beautie in an Age, and free from thee?"[456] During a later part of the century in 1674 Mr Z. Isham, a Northamptonshire country gentleman, wrote to his brother Sir J. Isham in connection with a smallpox epidemic in the neighbourhood:

"I have no reason on my own particular to be very secure, having never yet had that almost Universall Disease."[457]

This type of statement indicates in general terms that smallpox was widely prevalent even in the countryside. There is a great deal of evidence that most parishes expected smallpox to die out during an epidemic only after it had attacked most of the population at risk. According to a letter sent in 1723 to Jurin in connection with his enquiry into smallpox mortality, "about ten or twelve years ago the small pox went thro' their parish (Bradpole near Bridport) and few escaped it, at that time seven score were sick."[458] Similarly, one of the Purefoy family wrote in a letter in 1736 that "our town of Buckingham is grievously visited with the small pox, the last I hear of it, it was in three score houses, so forasmuch as it is so universall hope it will be soon over."[459]

It is of course possible that some parishes managed to avoid epidemics altogether by effectively isolating those sick from smallpox. Most parish registers which give information about the disease indicate that epidemics sooner or later occurred; Thomas Short, who made a very thorough contemporary study of diseases, concluded that in the case of smallpox, "the Disease keeps certain Periods of Return; as once in three or four years in large Towns, or six or seven years in wild, moorish country Places . . . "[460] He seems to have been referring to epidemics, with which he was mainly concerned; perhaps his conclusion is an approximate summary of what was happening in most parts of the country, although as we have seen epidemics did occur in places like Godalming at much more extended intervals of time.

Short did make it clear that smallpox was endemic in the country as a whole and stated that "there is no general Constitution of Weather wherein the Smallpox are not epidemic somewhere . . . [and sometimes] we shall find them in one Village, Parish, Town, or Corner of a County, and no where else in the Kingdom."[461] As we have seen earlier smallpox appears to have been present in London every week during the seventeenth and eighteenth centuries (according to the Bills of Mortality) and therefore London formed a kind of smallpox reservoir from which the disease was continually exported to the country at large.

The evidence that Jurin collected as secretary of the Royal Society on the comparative mortality of smallpox and inoculation, provided some incidental information on the extent of the natural form of the diseases. Jurin himself assumed in his calculations that "there are great Numbers, that never have the Small Pox",[462] but unfortunately he displayed only a limited grasp of the epidemiological problems confronting him. One of his correspondents, Mr Towgood of Shepton Mallet in Somerset, wrote arguing that he could statistically demonstrate the fallacy of the argument that people could escape natural smallpox even in a rural area of the kind in which he was living in. He wrote to Jurin about

"the Result of an Attempt I have been upon, but thro' hurry of business have not been able to finish. Tis a Computation of what proportion persons above 25 years old that never had the Small Pox bear to the rest of Mankind that have had them. For this end I have fixed on a middle Street of this Town, which is a considerable Place of Trade; and a Village on the skirt of the Town; and on a large Scattered Parish in the Country where there are Scarce two Houses contiguous. I imagine a Calculation made out of these 3 . . . would . . . give no Small Evidence on the Side of Inoculation in Answer to that Clamour so common among us in these parts, that the Inoculated in a great measure run a needless hazard, for they might never have the distemper in the natural way."[463]

Although Towgood had obviously collected some information on the problem – presumably an initial house-to-house enquiry – he had not been able to finish his calculations because of pressure of work in his medical practice. Unfortunately, Jurin dissuaded him from completing his research on the spurious grounds that the risk of dying from natural smallpox was already known from calculations based on the London Bills of Mortality! Smallpox was of course an endemic childhood disease in London; in Somerset it would have been an epidemic disease – in Taunton as we have seen it returned every six or seven years or so at the end of the seventeenth century – and the age incidence would have been much higher.

Jurin did however inadvertently collect some national statistics which throw some further light on this problem; not only did he collect information on the number of people inoculated, but also their ages. Although we cannot assume that these inoculated cases were representative of the population at risk to smallpox they were undoubtedly disproportionately selected from the wealthier social classes, and probably also from the towns they do give some idea of the age structure of the vulnerable population. The following is an age breakdown of the 477 people inoculated by 1724:[464]

Age (Years)	*Number Inoculated*	*Percentage Of Total*
Under One	11	2.3%
One To Two	15	3.1%
Two To Three	31	6.5%
Three To Four	41	8.6%
Four To Five	33	6.9%
Five To Ten	143	30.0%
Ten To Fifteen	82	17.2%
Fifteen To Twenty	56	11.7%
Twenty To FiftyTwo	62	13.0%
Age Unknown	3	0.6%

There were only about thirteen per cent inoculated above the age of twenty, and none above the age of fifty-two; these figures as they stand clearly indicate that most people had caught the disease by the age of twenty, most of those at risk being under that age. Although the figures on which this conclusion is based are not necessarily representative of all social and geographically located groups, they are at least national data, and are probably the best source of information for estimating the national age incidence of smallpox, and therefore indirectly its degree of universality in the population.

I have assumed in the discussion to date that everyone in the population is biologically vulnerable to smallpox if sufficiently exposed to the disease – thus the epidemiological assumption that age incidence will reveal the extent of the disease in the population. If

everyone is biologically vulnerable and all who die are children, everyone must have caught the disease as children – otherwise, there would be people dying at a later age after childhood. There is good evidence however that a small minority of the population is biologically totally immune to the disease, and it may have been this minority that Jurin and others had in mind when referring to some people escaping the illness altogether. Gatti summarized in 1767 the contemporary consensus of opinion, which would probably now be accepted as valid:

"It is certain, there are some who never have it [smallpox]; whole families are free from it for many generations; and it has been observed, that upon a hundred persons dying of old age, five or six had escaped it, though equally exposed to their contemporaries. Inoculators have met with much the same proportion of fruitless attempts."[465]

Gatti could come to such a precise conclusion because of his great knowledge of the history of inoculation, as well as his observation of smallpox over nearly a fifty-year period.

Most contemporaries agreed that smallpox was a universal disease that sooner or later more-or-less affected everyone. Sloane argued in 1735 in favour of inoculation on the grounds that "since it is reckoned, that scarce one in a thousand misses having it some time in their life, the sooner it is given them the better."[466] Its universality was sufficiently great to give rise to the theory of humoral pathology – that it was an innate contagion which had to be discharged and expressed through the blood – and Hillary summarized this view in 1740:

"Nor have we any Account of the Small Pox or Measles, till about the Year 640; tho' they are now become so universal Disease as any we know of . . . This Disease [smallpox] being so universal, it induced those who first wrote upon it, to believe that Infants before their Births contracted a seminal Contagion, a sanguine mestruoso matris (m) . . . This Opinion was constantly received and believed, till the

Discovery of the Circulation of the Blood . . . exploded this ill grounded Arabian hypothesis . . ."[467]

Hillary was premature in believing "his ill-grounded Arabian Hypothesis" to have been completely exploded, for as late as 1767 Bromfield was arguing "from the universality of this disease (smallpox) amongst mankind in most places, it seems, as if nature had some salutary end for the constitution, to be answered by this depuration that the blood undergoes at such time."[468] This view is now known to be invalid, but the combination of the inevitability of catching the disease with such a high risk of fatality made a marked impression on many of the people who wrote on the subject; for example, in 1752 Thompson concluded:

"There is no disease to which mankind is unhappily subject, so fatal in its effects, so universal in its influence, which so deeply affects the minds of all people . . . "[469]

The belief in the inevitability of smallpox continued to be held by medical writers until the end of the eighteenth century. In 1779 Benjamin Pugh, a surgeon, wrote from Baddow near Chelmsford in Essex that

"There is, I believe, scarcely an instance to be produced, in town or village, where any escaped the infection before inoculation was in use; and I have known many who have escaped so long, that they have been persuaded they never should have the small-pox, and yet have died of the confluent kind in extreme old age."[470]

Similarly, another surgeon William Black wrote in 1781:

" . . . but even in country villages, I imagine that very few are grown up to the age of twenty, who either have not had the Small-Pox, or have not been several times exposed within the sphere of variolous effluvia."[471]

Haygarth in 1793 confirmed this conclusion about the inevitability of smallpox with some statistics on smallpox among the Cheshire and Lancashire militia:

"in this neighbourhood [of Chester], neither in town nor country, no considerable number, who are capable of receiving the distemper, escape till they are men and women. To establish the truth of this remark, in 1781, I learned from Mr Edwards, surgeon in the Cheshire militia, that all the regiment had been infected except thirty in six hundred, or one in twenty, the proportion naturally exempted from it through life. When the Lancashire militia was in Chester, I made the same inquiry of Mr Drinkwater, their surgeon. He informed me that nearly the same proportion of them had passed through the smallpox."[472]

Only a very small minority of the militia appeared to have been inoculated, for Haygarth was informed in 1782 that only six of 466 of them had been previously inoculated.[473] Given that most of these men would have caught smallpox in the pre-Suttonian era as children, this confirms our earlier conclusion about the relatively insignificant practice of inoculation in the earlier period. The universality of smallpox among these men cannot be attributed to secondary contagion from inoculation, as it was much too rare to be a major source of infection.

The mortality from a disease is of course a function of both its incidence in a population, and its case-fatality rate. Four main factors can be identified as influencing case-fatality: (1) the biological vulnerability of the population depending upon the periodicity of epidemics as already discussed; (2) the age structure of the population, some age groups having higher case-fatality rates than others; (3) the virulence of the particular smallpox virus leading to the outbreak in question; (4) the hygienic conditions of the population and the presence or absence of other diseases and biological conditions. We have seen that there is little evidence for high biological vulnerability to smallpox in Britain during the eighteenth century. However, there does seem to have been examples of high

mortality, which may have been based on an absence of a pool of antibodies and genetic vulnerability. For example, Lettsom noted that

"in some countries, and even some counties of England, the infection does not appear for the space of some years; but when it does appear, it is more fatal; owing probably to this, that in great towns the infection being always prevalent, it is caught without the accumulated changes of air peculiarly favourable to epidemics; whereas when it comes at stated periods, its malignity seems to be augmented by some unknown but deleterious state of the atmosphere."[474]

Lettsom was of course unaware of the possibility of any kind of biological explanation for difference between town and countryside, but the observations he made are consistent with people in isolated country areas suffering from more fatal smallpox on account of greater biological vulnerability. There are a number of instances of high rural mortality which might be illustrations of this point; Eversley has noted the very fatal epidemic of smallpox which occurred in the eleven villages and one town in the area of Bromsgrove, Worcestershire, during the period 1725-29.[475] It wiped out more than a fifth of the population of Hanbury during 1725, which according to Eversley is a conservative estimate. Only about a third of the smallpox deaths were of children, which indicates that the majority of the population was attacked and thus suggests that the last epidemic had taken place many years previously.

Another example of a major smallpox epidemic during the eighteenth century is that which took place at Burford, Oxfordshire, in 1758. Burford was a very small market town with a population of about 1200, located in a relatively isolated position within the Cotswolds.[476] In the parish register for the year of 1758 is a list of all the persons who died of smallpox in that year, which is summarized at the end by the simple statement that "190 persons died of the Small-Pox."[477] Even this is probably an under-enumeration as the total burials were as follows : 1754 -28; 1755 - 41; 1756 - 39; 1757 - 43; 1758 - 240; 1759 - 41; 1760 - 33; 1761- 27; 1762 - 34. This 1758 epidemic only lasted about four months; 185 of the 190 recorded smallpox deaths occurred

between 10th April and 28th July, during which time there was a total of 199 deaths.

It is safe to assume that there were at least 200 deaths from smallpox, and it is known that over a half of the total occurred amongst children, suggesting that the case-fatality rate was very high, i.e. about 200 people died from smallpox amongst a population of about 1200, not all of whom were attacked by the disease (if all the population had been attacked, a higher proportion of deaths would have occurred amongst adults). Spectacular epidemics of the kind at Hanbury and Burford were sufficiently common for McKenzie to write in 1760 "that when small-pox is epidemic, entire villages are depopulated, markets ruined, and the face of distress spread over the whole country."[478]

The weight of the evidence is however against the biological vulnerability hypothesis; not only does previously considered evidence suggest that smallpox was a more-or-less universal disease affecting nearly all the population, even in rural areas, but even some of the examples quoted of spectacular epidemics lead to the same conclusion. Over half the people dying from smallpox in Burford were children, suggesting fairly regular and frequent epidemics of the kind that would provide a pool of antibodies and kill off the genetically vulnerable. Also, there is some evidence to suggest that smallpox mortality was sometimes lower in rural areas than in urban ones. For example, an anonymous correspondent of Jurin's wrote to him on the 23rd February 1723, making the following point:

". . . a Friend of mine that lives at Bradpole near Bridport [Dorset] informs me, That about ten or twelve years ago the small Pox went thro their parish and few escaped it, at that time seven score were sick whereof no more than six or seven dyed. He saith twas at Lother a Neighbouring parish about the same time, and the Number of them that dyed there was as small in proportion to the Number of the Sick as at Bradpole: the Number of them that dye of the Small pox in Country Parishes is in Proportion to the Sick much less than in populous Towns and Cityes."[479]

On the other hand, another of Jurin's correspondents pointed out at about the same time that about one in five died from smallpox in the town of Portsmouth and its neighbouring area, whereas "Havant a Village seven miles hence lost even more."[480] Nettleton had claimed that "the Proportion of those that die is much the same" in the town and country,[481] but as Haygarth had pointed out at the end of the eighteenth century, fatality rates were greatly influenced by the age incidence of the disease and this usually varied between the town and countryside.[482]

It is partly possible to examine this question by comparing the small number of non-industrial rural villages in Jurin's sample of communities in the smallpox censuses of the 1720s, with the remaining sample of towns and industrial villages. There were seven non-industrial villages – Hatherfield, Havant, Dedham, Aynho, Cobham, Kempsey and Uxbridge – and of 1,234 cases of smallpox, 298 died, a case-fatality of 24.1 per cent. This compares with 11,958 cases and 1,869 deaths – a case-fatality rate of 15.6 per cent – in the remaining 25 towns and industrial villages in the sample.[483] Although this difference is a fairly significant one, it is of a scale that is more likely to be explained by variations in age incidence, than the relatively dramatic factors involved in biological selectivity.

Virtually the only evidence of the effect of age-incidence on mortality in the eighteenth century came from data sent to Jurin on the smallpox epidemic at the small village of Aynho in Northamptonshire in 1723-24. The following is a summary of that evidence.

Smallpox Mortality At Aynho, Northants, 1723-24[484]

Age (Years)	*Cases*	*Deaths*	*Percentage*
0-10	28	4	14.3%
10-20	47	4	8.5%
20-30	25	6	24.0%
30-40	12	3	25.0%
40+	20	8	40.0%

Although some of these figures are based on very small numbers, the pattern to emerge is very similar to that found in larger nineteenth century surveys. For example, the following is a summary of data available for a very large series of cases reported in Berlin, Germany for the years 1865-74

Smallpox Mortality Amongst The Unvaccinated At Berlin, 1865-74[485]

Age (Years)	*Cases*	*Deaths*	*Percentage*
0-10	5270	2124	40%
10-20	218	25	12%
20-30	316	57	18%
30-40	196	52	27%
40+	213	83	39%

Both the Aynho and Berlin figures reveal a U-shaped distribution, although the U is much more even in Berlin than in Aynho. Minimum mortality was found in both surveys to be amongst the 10-20 year age group, and the maximum mortality – found at the extremes of the age spectrum – was of the order of four times as great. The Aynho figures indicate that the under ten age group had a relatively low fatality rate and this is almost certainly a function of the smallness of the numbers involved; all the nineteenth century surveys showed peaks of mortality at both the younger and older age ranges, and this was most vividly illustrated by the figures of smallpox mortality at Homerton Smallpox Hospital (London), which because of the number of cases were broken down into very detailed age categories.

Smallpox Mortality Amongst The Unvaccinated At Homerton Smallpox Hospital, 1870-83[486]

Age (Years)	*Cases*	*Deaths*	*Precentage Fatality*
Under One	155	98	63.2%
1-2	121	83	68.5%
2-3	115	77	61.6%
3-4	129	65	46.7%
4-5	147	60	41.3%
5-10	510	180	35.2%
10-15	317	74	23.3%
15-20	204	86	42.3%
20-25	174	83	47.7%
25-30	105	56	53.3%
30-35	53	22	41.5%
35-40	50	20	40.0%
40+	79	34	43.0%

In Homerton Hospital although the U-curve distribution is still present, the peak mortality occurs in the very young age groups, particularly under the age of three. Most of these surveys show a significant minimum mortality among the 10-20 age group, but in the Homerton figures it is more specifically the 10-15 age group which is at the bottom of the U distribution. The scale of differences is so significant – at Homerton of the order of three to one – that we must clearly allow for age incidence when we are discussing the overall problem of smallpox mortality and how it changed over time.

All the evidence points to a gradual but highly significant increase in the virulence and case-fatality rate of smallpox from the late sixteenth through to the end of the nineteenth century when it sharply began to decline. Before discussing this evidence, we may summarize McVail's conclusions reached through a detailed examination of some of the sources:

" . . . natural smallpox gradually became throughout the eighteenth century, and up to the epidemic of 1870-73, a more virulent and fatal disease, its maximum fatality being on a large basis of facts 45 per cent, and since then it has irregularly, yet persistently, diminished in fatality until we come to the epidemic of 1902-5 with unvaccinated rate of 19.3 per cent."[487]

Although this conclusion applies to the eighteenth and nineteenth centuries, the increase in the virulence of smallpox almost certainly started as late as the end of the sixteenth century. This was reflected in the increasing mortality from smallpox in London during the seventeenth century.

Percentage Of All Deaths Due To Smallpox In London[488]

Period	*Percentage*
1574-98	1.6%
1629-36	2.8%
1650-60	4.8%
1660-70	3.6%
1670-80	7.1%
1680-90	7.3%
1690-1700	4.5%
1700-10	5.3%
1710-20	8.1%
1720-30	8.2%

Although these statistics of smallpox mortality in London are unreliable as indications of total mortality due to smallpox (for

reasons already discussed), they do indicate the increase in the fatality of the disease during the period 1574-1730. As the statistics refer to the same type of environment where smallpox was throughout the whole period a disease of young children, it would appear that the increase in fatality was due to an increase in the virulence of the disease. There do seem to have been fluctuations in the virulence of smallpox during the period, with a first peak during 1670-90 and a second in 1710-30. The increase in virulence during the middle of the seventeenth century is reflected in contemporary comments; for example Dr Tobias Whitaker, who had been exiled with Charles II during the civil war, wrote in 1661 that smallpox

"was constantly and generally in the common place of petit and puerile and the cure of no moment . . . But from what present constitution of ague this childish disease hath received such pestilential tinctures I know not; yet I am sure that this disease, which for hundreds of yeares and before the practice of medicine was so exquisite, hath been as commonly cured as it hapned . . ."[489]

Other commentators writing in the 1660s noticed this increase in the virulence of smallpox,[490] but as late as 1689 Dr Walter Harris could write:

"Smallpox and measles in infants, being for the most part a mild and tranquil effervesence of the blood, are wont to have often no bad character, where neither the helping hand of physicians are called, nor the unbounding skill of complacent nurses is put in requisition."[491]

There was some decline in virulence during the period 1690-1710 according to the London statistics, followed by an increased fatality after the 1710s, which as we shall see later continued throughout the eighteenth and nineteenth centuries. The increase in virulence after the 1710s in London seems to have occurred somewhat later in the country at large, as is illustrated by the following figures:

Deaths From Smallpox In Godalming, Surrey And Skipton-In-Craven, Yorkshire.[492]

Period	*Godalming Deaths From Smallpox*	*Period*	*Skipton-in-Craven Deaths From Smallpox*
1686	50	1716-22	13
1701	24	1723-29	51
1710-11	39	1730-36	54
1722-23	94		

These statistics only refer to two towns and as smallpox mortality is known to vary greatly from one epidemic to another, we must be careful about the conclusions we reach from such evidence. However, all the available evidence does point to a general increase in virulence from the 1720s onwards; for example, according to the Basingstoke parish register which lists smallpox deaths, 50 people died during the 1714 epidemic, whereas 125 died from the same disease during 1741. The increase in virulence is also reflected in contemporary comment; the compiler of the *Northampton Bills of Mortality* noted in 1740 that "the Small-Pox has been very much in this Town this Year, and more mortal by far than in any one Year in the Memory of Man."[493] Similarly Deering noted in 1751 that the smallpox epidemic of 1736 in Nottingham

"was a fatal Instance, for from the latter End of May to the beginning of September, this Distemper [smallpox] swept away a great Number of Souls (but mostly Children) and in the single Month of May, there were buried in St Mary's Church and Church-yard only, 104; In short, the Burials exceeded that Year the Births by above 380, whereas otherwise there is *communibus annis* an increase of about 65; a Mortality, the like I have not been able to discover in looking back into the Church Registers for above 30 years, and I much question whether there has been the like since the Plague, which visited this Town in 1667, and made a cruel Desolation in the higher part of Nottingham . . ."[494]

Creighton in his historical survey of smallpox epidemics in Britain emphasized the virulence of smallpox during the early 1720s:

"The years 1722 and 1723 . . . were one of the greater smallpox periods in England. In Short's abstracts of the parish registers those years stand out very prominently by reason of the excess of deaths over births in a large proportion of country parishes; and according to Wintringham's annals, it was not fever that made them fatal years, but smallpox, along with autumnal dysentries and diarrhoeas."[495]

Creighton was much too much influenced by the evidence immediately available to him, and as we have already seen smallpox was very fatal after the early 1720s in the Bromsgrove area, Nottingham, Northampton, Burford etc. Before considering the general evidence on the change of virulence during the eighteenth and nineteenth centuries, it is necessary to discuss in some detail the statistics of smallpox mortality compiled by Jurin and others during the 1720s, which provide the most accurate and general account of smallpox mortality before the introduction of popular inoculation.

Censuses Of Smallpox Epidemics In England, 1721-31[499]

Locality Of The Epidemic	*Date*	*Cases*	*Deaths*	*Per Cent Fatality*
Halifax	1721-22	276	43	15.9%
Rochdale	1721-22	177	38	21.4%
Leeds	1721-22	792	189	23.8%
Halifax	1722	565	87	15.4%
Bradford	1722	129	36	27.9%
Wakefield	1722	418	57	13.6%
Ashton-Under-Lyme	1722	279	56	20.0%
Macclesfield	1722	302	37	12.2%

Stockport	1722	287	73	25.4%
Hatherfield	1722	180	20	11.%
Chichester	1722	994	168	16.9%
Haverfordwest	1722	227	52	22.9%
Barstand, Ripponden, Scorby and part of Halifax parish 4 miles from the town	1722	230	38	16.5%
Bolton	1723	406	89	21.6%
Ware	1723	612	72	11.7%
Salisbury	1723	1244	165	13.2%
Romsey, Hampshire	1723	913	143	15.6%
Havant	1723	264	61	23.1%
Bedford	1723	786	147	18.4%
Shaftesbury	1724	660	100	15.1%
Dedham, near Colchester	1724	339	106	31.3%
Plymouth	1724	188	32	17.2%
Aynho, near Banbury	1723-24	133	25	18.8%
Stratford-on-Avon	1724	562	89	15.8%
Bolton-le-Moor	1724	341	64	18.8%
Cobham	1724	105	20	19.0%
Dover	1725-26	503	61	12.1%
Deal	1725-26	362	33	9.1%

Kempsey, near Worcester	1726	73	15	20.5%
Uxbridge	1727	140	51	36.4%
Hastings	1730-31	705	97	13.7%
Total		*13,192*	*2,167*	*16.5%*

Twenty-six of the thirty-two censuses were conducted in market towns: in no sense must they be taken as a comprehensive census of all the smallpox epidemics in the 1720s. They appear to have been epidemics that caught the attention of a few medical practitioners and others who were interested in reporting them for statistical purposes. Nettleton of Halifax was responsible for reporting twelve epidemics and thus the predominance of Yorkshire in 1722. The overall case-fatality rate derived from all the censuses was 16.5 per cent, i.e. of 13,192 people catching smallpox, 2,167 died. There was considerable variation in the case-fatality rate from one epidemic to another – the minimum was 9.1 per cent at Deal during 1725-26, the maximum at Uxbridge in 1727, 36.4 per cent. It was noted in connection with the smallpox census at Uxbridge, that "at Uxbridge and in the neighbourhood, the smallpox having been exceedingly fatal all thereabouts."[496] The variation in the fatality of smallpox was well recognized by contemporaries, "it is sometimes so very Mortal, and at other Times so very Mild and Favourable"[497] and "they are fatal in one Place, favourable in another and not known in a third."[498] Statistics of mortality from any one place can therefore be very misleading as an indication of general mortality in the country as a whole, although the degree of variation in the case-fatality rate in the censuses of the 1720s is not that great, most epidemics having a case-fatality rate near the average, 16.5 per cent. This average figure is probably unrepresentative of the country as a whole, inasmuch as a disproportionate number of industrial market towns are included in the sample, and it appears that most cases of smallpox in these areas occurred amongst children.

Of seventy eight people inoculated by Nettleton in the Halifax area in 1723, 83 per cent of them were under the age of seven, with a median age of approximately four years.[500] Presumably therefore in these areas most people caught smallpox after the age of four at some time during their younger childhood. In a rural area like Aynho on the other hand, many people caught smallpox during their adulthood, with the result that the case-fatality rate of that village was slightly above average. We have seen that case-fatality in the seven non-industrialized villages in the smallpox census sample was 24.1 per cent, and this higher than average fatality rate was probably due to the greater population of adults affected in rural areas. It is therefore likely that the figures derived from the smallpox censuses understate the national case-fatality rate during the 1720s, as most of the population would have been living in small villages rather than towns and industrial areas.

The relevance of age to smallpox mortality was certainly recognized by contemporaries; for example, the Reverend David Some writing in 1725 stated "that of young Children that have it, one in six or seven commonly die of it; and of grown Persons, at least one in three."[501] This fits quite well with what we know of the relationship between age and case-fatality rates, although there are marked variations within the childhood category which influence smallpox mortality. An illustration of this is found in the writings of Isaac Massey, one of the early opponents of inoculation. In 1723 he claimed that among the children of the Christ's Hospital school, "not One out of fifty have died these last twenty years of that Distemper [smallpox]"[502] – a figure which may well have been accurate, as all the children in the school were between the ages of eight and fifteen, the period of minimal case fatality. These variations in fatality and mortality rates illustrate the danger of taking isolated examples as in any way representative; it is necessary always to take evidence from a number of different sources and places – such as the smallpox censuses of the 1720s – and place it in a general context of what is known about the influence of a variable like age.

The low fatality amongst the Christ's Hospital children may also have been partly a function of what appears from the London mortality figures to have been a dip in the virulence of the disease, at

least for the period 1690-1710. The Reverend Some claimed in 1725 "that of late Years, it [smallpox] has been more mortal than usual."[503] It is possible that some of this increase in mortality was due to a deterioration in hygiene – poor hygiene is thought to increase the fatality of smallpox[504] – but there is evidence that the case-fatality rate was rising independently of environmental conditions during the eighteenth century. The following summarizes changes in case-fatality rates in the second half of the eighteenth and first half of the nineteenth century in the London Smallpox Hospital, which as far as is known provided a fairly homogeneous environment during the whole period.

Case-Fatality Rate Of Smallpox In London Smallpox Hospital.[505]

Period	*Number Of Cases*	*Percentage That Died*
1746-63	6456	26%
1776-1800	7017	32%
1836-51	2654	38%

The London Smallpox Hospital was moved from its original site to a new hospital at St Pancras at the end of the eighteenth century, and it is therefore unlikely that the increase in case-fatality was due to a deterioration in hygiene. It is also possible that there was a change in the age distribution of patients admitted to the hospital, which might partly account for the increase in fatality, although the hospital throughout most of its history only admitted patients above the age of seven, and most of them appear to have been servants of the subscribers to the hospital.[506] The mean age of smallpox patients during 1836-51 was 17.5 years,[507] and this was probably the mean age of patients throughout the eighteenth century, most of whom were probably young servants from the countryside. The case-fatality rate in the hospital would have been higher than outside, as it only admitted the more serious cases, although this would be counter-balanced to some extent by their average age putting them into the minimal risk category.

The increase in the natural virulence of smallpox can be further traced through various local smallpox censuses that were

conducted after the 1720s during the rest of the eighteenth and nineteenth centuries.

Case-Fatality Amongst The Unvaccinated In Smallpox Epidemics, 1740-1893.

Location Of The Epidemic	*Date*	*Cases*	*Deaths*	*Per Cent Fatality*
Northampton[508]	1740	899	132	14.5%
Northampton[509]	1747	821	126	15.5%
Salisbury[510]	1753	1244	165	13%
Chelmsford[511]	1753	290	95	33%
Chester[512]	1774	1385	202	14.5%
Leeds[513]	1781	462	130	28%
Huddersfield & Neighbourhood[514]	1783	458	103	22.5%
Norwich[515]	1819	200	46	23%
Sheffield[516]	1887-88	552	274	49.5%
Dewsbury[517]	1891-92	366	92	25%
Warrington[518]	1892-93	68	24	35.5%
Leicester[519]	1892-93	158	19	12%
London[520]	1892-93	409	199	48.5%
Gloucester[521]	1892-93	768	314	41%

As with the smallpox censuses of the 1720s, this Table reveals a considerable amount of variation from place to place in fatality rates at any one point in time. Nevertheless, there is a clear long-term trend, with fatality increasing more or less over the whole period. McVail's finding that virulence peaked in the early 1870s is consistent overall with these figures, except that the Sheffield epidemic of 1887-88 seems to have had the highest case-fatality rate recorded for any community survey. The 1892-93 epidemic in the five towns at the end of the Table had an overall fatality rate of 35.4 per cent amongst the unvaccinated (2321 cases, with 822 deaths), which was over twice that found in the censuses of the 1720s, even though fatality had probably begun to decline at the end of the nineteenth

century. There is no evidence that this change had anything to do with age incidence, and this can be illustrated by comparing the figures for Aynho earlier quoted with those for the 1887-88 Sheffield epidemic.

Case-Fatality In The Aynho, 1723-24 And Sheffield, 1887-88 Epidemics.[522]

Age (Years)	*Cases*		*Deaths*		*Percentage Fatality*	
	Aynho	Sheffield	Aynho	Sheffield	Aynho	Sheffield
Under Ten	28	228	4	100	14.3%	43.9%
10-20	47	175	4	85	8.5%	48.6%
20-30	25	98	6	61	24.0%	62.2%
30+	32	49	11	28	34.4%	57.1%
Total	132	550	25	274	18.9%	49.6%

Overall case-fatality in Sheffield was nearly three times that of that in Aynho, and the Table clearly reveals that this increase was not a function of age; for example, in the 10-20 age group, the case-fatality rate was 8.5 per cent in Aynho and 48.6 per cent in Sheffield. This was an extreme difference, but the smallest difference is still a significant order – in the 30+ age group: 34.4 per cent in Aynho and 57.1 per cent in Sheffield.

Although some of this increase in fatality may have been due to a deterioration in hygiene – particularly when we compare a rural area like Aynho with an urban district like Sheffield – we saw earlier that the hygiene hypothesis had little to support it. Also it is likely that overall personal hygiene was significantly better in the last half of the nineteenth century than it was in the eighteenth century.[523] The most likely explanation of the increase in fatality is that more virulent strains of smallpox were being introduced into the country with the growth of world trade. We have seen that Sarkar and his colleagues found a correlation between the virulence of a virus and its excretion in the throat and urine and this

confirmed Dixon's clinical observation about the relationship between severity and infectiousness.

With the growth of world trade, virulent viruses would drive out more benign ones, although this would not explain the down-turn in fatality at the end of the nineteenth century. This may have been due to more effective inoculation and vaccination programmes in the countries with the higher fatality rates and more virulent viruses. Whatever the explanation for the increase in virulence, the evidence from a number of sources certainly leads to the conclusion that the fatality of smallpox increased during the eighteenth and nineteenth centuries. The statistical evidence was confirmed by a number of literary accounts, and we may conclude the discussion of this topic with a quote from Lettsom, writing in 1805 at the end of the period of greatest interest to this book:

"I think, from my own experience, that the malignity [of smallpox] even in London is augmenting. When I practised here, 35 years ago, one in ten was the calculation; but I think one in six is now a fair proportion."[524]

We must now sum up the extent and fatality of smallpox before the introduction of inoculation, which has necessarily involved detailed consideration of a number of complex technical issues. The following conclusion emerges from the preceding discussion: smallpox was a universal disease affecting all members of the population except for a minority of about five per cent who had natural immunity against the disease. Fatality varied from place to place, depending in the main on the periodicity of the disease and the resulting age incidence. In the 1720s the case-fatality rate in towns appears to have been of the order of 15.5 per, cent, and in the countryside where the majority of the population lived, about 24 per cent.

These figures cannot be used however as a direct basis for the estimation of smallpox mortality before the introduction of inoculation for two main reasons: (i) evidence that secondary mortality from complications such as broncho-pneumonia and fulminating infantile convulsions would raise the true mortality

figure well above this level; (ii) the case-fatality of smallpox was increasing throughout the whole period under consideration. From a demographic point of view, inoculation and vaccination played a major role in preventing a major decimation of the population. Smallpox had a case-fatality rate of about 45 per cent in the 1860-1880 period, which all except five per cent of the population would normally have caught. Merely to contain smallpox mortality to a stable earlier level would have been a considerable achievement in the light of the very significant increase of the virulence of the disease, but smallpox mortality actually declined during the last third of the eighteenth century. Additionally, the gradual elimination of a disease that appears to have such an impact on fertility, is obviously of great demographic importance. This along with the discussion of the decline of smallpox mortality will be dealt with in the next chapter.

In order to complete our discussion of smallpox mortality before the practice of inoculation, it is necessary to analyse the figures of smallpox mortality that emerge from parish registers and the like. Given the difficulties discussed above about reaching a true smallpox mortality figure – in particular the secondary causes of mortality – these figures have to be treated with considerable caution. Also there are two specific problems of using the figures that are available, both hinging on the fact that they are in the main for towns, rather than country areas: (a) the differing age-incidence and probably lower mortality than average resulting; (b) the existence of pest houses and private burials which were not included in parish registers (this would have been particularly true of market towns). With these major reservations, we can look at all the available figures of registered smallpox mortality in the pre-inoculation period, which in the countryside at large I have assumed occurred before the 1760s, when the popular Suttonian method was introduced. In some of the larger towns I have assumed that the pre-inoculation period extended to the end of the 1770s (it was probably later in the really big towns, but I have attempted to make the more cautious assumption).

The following Table is compiled from statistics that are thought to be reasonably accurate. I have expressed smallpox

mortality as a proportion of smallpox deaths per 100 births/ deaths, although I have used the ratio per 100 births wherever possible as it more accurately summarizes the proportion of the population (ever born) who were registered as being killed by smallpox. Where population is static, the proportions expressed as a ratio of births or deaths will be the same, but in large towns like London in the eighteenth century the death rate was much higher than the birth rate. We are mainly interested in the proportion of young children killed by smallpox, as the disease primarily affected young children in large towns like London and most of the other towns included in the Table. Where reliable statistics of the number of births are not available, I have expressed smallpox mortality as a proportion of the number of total deaths. For example, according to available statistics about a third of all children born in Dublin between 1715 and 1746 died from smallpox – this is an unusually high proportion, and may be due to the under-registration of births of Roman Catholics who objected to Anglican rites.

Smallpox Mortality Before The Introduction Of Popular Inoculation

Place And Period	*Smallpox Deaths*	*All Deaths*	*All Births*	*Average Annual Smallpox Mortality*
Dublin, 1661-90[525]	472 (Annual Average)	2236 (Annual Average)		21 per 100 deaths
Dublin, 1715-46[526]	13,759	74,585		18.5 per 100 deaths
Kilmarnock, 1728-62[527]	621		4514	14 per 100 births
London, 1730-39[528]	19,700		170,000	11.5 per 100 births
Boston, Lincolnshire, 1749-57[529]	106		691	15.5 per 100 births
Maidstone, 1752-61[530]	252		1462	17 per 100 births
Manchester, 1769-74[531]	589	3807		15.5 per 100 deaths
Liverpool, 1772-74[532]	662		3559	18.5 100 births
Chester, 1772-77[533]	369		3970 (1764-73)	15 per 100 births

The proportion of total deaths due to smallpox in Dublin (about 20 per cent) is relatively high compared to the statistics of mortality in other places during the same period – the proportion of total deaths due to smallpox in London was never much greater than eight per cent during the same period (1661-1746). This difference could partly be due to economic and environmental factors, i.e. poverty may have raised smallpox mortality in Dublin, although there is no particular evidence for this conclusion. An alternative explanation is that the registration of smallpox deaths was much more accurate in Dublin than in London, and there is certainly evidence of significant under-registration of smallpox deaths in London. However, registered smallpox mortality in Dublin was higher than that for most other places where statistics are available, although there are examples of apparently higher mortality over a long period of time. For example, an account was sent to Howlett of smallpox mortality in Great Chart, Kent, where "its burials in a period of twenty years immediately subsequent to the revolution [1688-1707] were 192 – but almost 100 of them were occasioned by the small-pox."[534]

There is no mention of deaths from smallpox in the Great Chart parish register, although the total number of deaths during the period mentioned according to the register is 192. There is no indication that there was any great epidemic in the parish during the period, but this does not necessarily mean that the number of smallpox deaths mentioned by Howlett's correspondent did not occur, for there is evidence that smallpox did not always create epidemics but sometimes only produced a few deaths at any one time; for example, Haygarth's statement that "on comparing several neighbouring villages, we observe, some entirely free from the distemper, others have only a few infected, others suffer a general seizure."[535]

Nevertheless, the smallpox mortality at Great Chart according to Howlett's account (about 50 per cent of all deaths due to smallpox) was much higher than most known mortality in England during the same period, which leads us to suspect the accuracy of

Howlett's correspondent's account. According to the statistics presented, mortality varied between 11.5 and 21.5 smallpox deaths per 100 births/deaths during the eighteenth century before the introduction of popular inoculation. These statistics also indicate a tendency for smallpox mortality to increase over time and this conforms with the known rise in the case-fatality rate during the same period.

The significance of smallpox mortality before the introduction of popular inoculation is not only depicted by statistical evidence, but is confirmed by literary sources. In the church at Great Barrington, Oxfordshire, there is a monument to the Bray family who were the local gentry:

"Sir Giles Bray married Frances Ashcomb, of Alvescot in Oxfordshire. They had five sons and two daughters, and lost six of them from smallpox. Reginald, the first-born died of smallpox, December 23, 1688; Edmund, died of the same disease when serving as an officer with the Army at the siege of 'Mastrich'. Giles, John, Ashcomb, and Mary, All Dyed also of the same fatal Distemper to this family."[536]

Edmund, who had died at "Mastrich", was the father of two children, Jane and Edward,

"She dyed of the Small Pox at her Aunt Catchmay's in Gloucester, on Monday the One and Twentieth of May 1711 in the Eighth Year of her Age . . . He dyed upon Christmas Day 1720 of the Smallpox at the Royal Academy at Anglers in France, in the Fifteenth year of his age . . ."[537]

This extreme mortality of smallpox was not of course typical at this time, as it was more fatal in some families than others. This is reflected in the diary of John Score, a wealthy citizen of Exeter, who recorded the illnesses of his family:

"1711 – 'This summer the Small Pox raged very much in Exeter.' A son had it and recovered (Sept. 15th). 1716, September

7th a daughter had smallpox and recovered – 'A great many dyed this season in the Small-Pox.' 1724, Feb. 3. A child had smallpox and recovered. 1724, March 3. A son had smallpox and recovered. 1729, August. A son had smallpox and recovered. 1729, September. Two daughters had smallpox and recovered. 'The Small Pox was very fatall to some. Mr Vivian lost all his children, being four sons.' 1731, Feb. A son aged 2 yrs 5 months had 'Small Pox of the confluent kind' and died on the twelfth day."[538]

Of the eight Score children who caught smallpox, only one died – this is to be contrasted to the Vivian family, where all four children caught and died from the disease. It is clear that large numbers of children were dying from smallpox in Exeter, during epidemics which returned every five years or so. There are many examples of a large number of a particular family being wiped out by the disease, and I shall conclude this chapter by quoting one final example reported in Dodsley's Annual Register in 1762:

"The Hon. John Petre, brother to the Lord Petre, who died lately, aged 24, is said to be the eighteenth person of that family that has died of smallpox in 27 years."[539]

CHAPTER 9
The Conquest of Smallpox

It is possible to assess the impact of inoculation on smallpox mortality in particular parishes through the parish register evidence. It is unfortunate that this type of evidence is only rarely available and a disproportionate amount is for large towns which contained only a minority of the total population during the latter half of the eighteenth century. Because inoculation was practised more extensively in the countryside than in large towns, the statistics which are most easily available (those for the large towns) give a misleading picture of the extent of the decline in smallpox mortality. It is partly possible to overcome this difficulty by: (1) using statistics compiled in connection with general inoculation; and (2) those derived from parish registers when information on smallpox mortality is given. Some of the figures mentioned in the literature in connection with general inoculations do give a number of people dying from smallpox, presumably either those who caught the disease before the general inoculation or exempted themselves from it. For example, in the general inoculation at Northwold, Norfolk in 1788, when 300 people were inoculated, eleven died from natural smallpox.[540] An even better example, is the general inoculation which took place at Hevingham, Norfolk in 1794:

"In the month of May of this year were inoculated for the Small Pox 3 Adults, 223 under 20 years, and 11 took ye Disease by Natural Infection."[541]

The eleven people catching the natural disease represented only about 4 per cent of all cases, and assuming a case-fatality rate of about 1 in 5 at this time, over 44 lives were saved by inoculation in this parish. The general inoculation at Hungerford, Berkshire in 1794 indicates an even greater saving of life; "about one thousand" were inoculated, "not above two or three of which number died," while "about 20 perished with the natural sort."[542] These figures suggest that about 90 per cent of all cases were inoculated (the 20 dying from smallpox representing 100 cases with a case-fatality rate of 1 in 5) and

that about 200 lives were saved. Even when a substantial number of people refused to participate in a general inoculation, the saving of lives through inoculation was very considerable; for example, 59 of 250 unprotected people catching smallpox in Bedford in 1778 died from the disease, so the 1100 people inoculated "within one week" would represent the saving of about 280 lives.[543] Assuming that 339 people would have died (59 + 280) without a majority of the population at risk being protected by inoculation, the actual number of smallpox deaths (59) represents under fifteen per cent of the number who would have died without inoculation.

This inoculation at Bedford was considered by Dimsdale to be unsatisfactory because of the relatively large number of people not inoculated at the time of the mass inoculation. Dimsdale was satisfied that "the extensive practice of general Inoculations in the country, which have prevailed in a remarkable manner . . . has been conducted properly."[544] The result of inoculating practically all the population at risk was the virtual extinction of smallpox mortality, as had been achieved in Hertford through Dimsdale's personal efforts. Similarly smallpox was in effect extinguished from Calne in Wiltshire through the repetition of general inoculations: 800 people were inoculated in the year 1793, while there were only six deaths from smallpox in that year.[545]

This elimination of smallpox necessarily followed from the adoption of Dimsdale's plan, and in a place like Brighton where all the vulnerable members of the population were inoculated, the only deaths from smallpox were those that preceded the general inoculation. Most of the examples of general inoculations given in Chapter 5 were probably of this type, and this is illustrated by the proportion of the total population involved. The following Table gives the numbers inoculated set alongside the population size in 1801, for those parishes where information on the number of natural smallpox deaths is not available:

Place	*Date Of General Inoculation*	*Numbers Inoculated*	*Parish Population In 1801*
Irthlingborough, Northamptonshire	1778	Above 500	811
Diss, Norfolk	1784	1100	2246
Painswick, Gloucetsershire	1785	738	3150
Brighton, Sussex	1794	2113	5669 (1794 Population)
Lewes, Sussex	1794	2890	4909
Dursley, Gloucestershire	1797	1475	2379
Tenterden, Kent	1798	1167	2370

Most of these general inoculations involved approximately half the total population, although in several instances it is even higher. Most of them took place in quite large market towns, and were probably like Brighton in 1786, having about the same number of people who had already had smallpox as required inoculating (in the case of Brighton in 1786, 1,733 who had been through the smallpox, as against 1,887 who were inoculated). Inoculation continued to contribute to the saving of lives during the nineteenth century and helped to significantly diminish smallpox mortality. For example, during the epidemic in the Chichester region of 1821/22, there were "not more than 130 or 140 cases of natural smallpox" of which number "about twenty proved fatal,"[547] an insignificant number when set against the two to three thousand both inoculated and vaccinated (i.e. a total of between four to six thousand).

Smallpox Mortality From Epidemics In Boston, U.S.A. In The Eighteenth Century.[546]

Date	*1677-78*	*1702*	*1721*	*1730*	*1752*	*1764*	*1776*	*1788*	*1792*
Population	4000	6750	10700	13500	15684	15500			19300
Natural Smallpox Cases			5759	3600	5545	699	304	122	232
Natural Smallpox Deaths	700	213	842	500	539	124	29	40	69
Smallpox Deaths Per 1000 Cases			146	139	97	177	95	328	298
Inoculated Cases			287	400	2124	4977	4988	2121	9152
Deaths Of Those Inoculated			6	12	30	46	28	19	179
Deaths Per 1000 Inoculated			21	30	14	9	6	9	20
Total Smallpox Deaths	700	213	848	512	569	170	57	59	284
Deaths Per 1000 Population	175	32	79	37	36	11	10	6	10
Left The Town					174	519			221
Had Smallpox Before					5998	8200			10300

The second type of statistic referred to earlier – measuring changes in smallpox mortality over time by using parish registers and similar documents – is really the most satisfactory way of assessing

the effect of inoculation on smallpox mortality. Ideally, the type of information required to measure the effect of inoculation on mortality is that illustrated in the preceding Table for Boston, U.S.A. during the eighteenth century, such information not being available for anywhere in Britain during the same period.

Three important conclusions may be reached from this Table: (1) the overall smallpox death rate was reduced from 175 deaths per 1000 living in 1677-78 to 10 per 1000 by 1792; (2) this was achieved in spite of a general increase in the case-fatality rate – about 30 per cent of those catching the natural disease died from it in 1792; (3) the reduced mortality may be directly attributed to inoculation, which protected the vast majority of the population at risk from 1764 onwards.

Unfortunately similar evidence is not available for Britain during the same period, and it is only possible to quote statistics of smallpox mortality in places where inoculation is known to have been effectively practiced. The Maidstone parish register contains entries of people dying from smallpox and we know from Howlett's pamphlet on the population and health of the town that popular inoculation was introduced into the town in 1766 when Daniel Sutton conducted a mass inoculation.

Smallpox Mortality At Maidstone, Kent, 1740-1799[548]

Period	*Smallpox Deaths*	*All Deaths*	*Smallpox Deaths As A Proportion Of All Deaths*
1740-51	260	1594	16.3%
1752-63	202	1616	12.5%
1764-75	76	1798	4.2%
1776-87	122	1992	6.1%
1788-99	31	2308	1.3%

These statistics of changing smallpox mortality are subject to the deficiencies and inaccuracies in registration discussed in an earlier chapter. This is particularly the case with respect to data derived from parish registers, where the completeness of smallpox deaths is uncertain. It is possible that several deaths from smallpox were

simply not registered, as registration served no function other than the interest of the incumbent keeping the register.

However, these Maidstone statistics do indicate the trend of a marked reduction in smallpox mortality during the latter half of the eighteenth century. By the end of the eighteenth century smallpox had disappeared from the register as a cause of death, the last mention of the disease occurring in 1797 when two children were listed as having died from it. That this was due to inoculation is indicated by Howlett's account of the subject quoted earlier in the book.

Another parish register which lists death from smallpox is that for Calne, Wiltshire.

Smallpox Deaths In Calne, Wiltshire, 1703-1802[549]

Period	*1703-22*	*1723-42*	*1743-62*	*1763-82*	*1783-1802*
Number Of Smallpox Deaths	84	205	122	54	8

This Table suggests that smallpox became more fatal during the period 1723-24 which is consistent with the earlier analysis of increases in virulence and case-fatality, although it is possible the relatively small number of smallpox deaths during 1703-22 might be due to under-registration. The Table also indicates that smallpox mortality began to decline from the period 1743-62 onwards. The history of inoculation in Calne before 1782 is unknown, but general inoculations were carried out in the town in 1782 and 1793. The clearest indication of the reduction of smallpox mortality in the town is to compare the epidemic of 1732 when 173 people died from smallpox, with the numbers dying in the two years of general inoculation: ten in 1782 and six in 1793. The scale of the decrease in the number of smallpox deaths between the earlier (1732) and later period (1782-93) is consistent with the known number of inoculations during the latter – over 800 people inoculated during September 1793.

A similar pattern of smallpox mortality to that in Calne is found in Basingstoke, Hampshire during the eighteenth century.[550] The

first registered epidemic occurred in 1714 when 50 people died from smallpox. The next major epidemic occurred in 1741 with 125 registered as dying from the disease; this number is, however, probably an under-statement of the number of smallpox deaths as the total number of deaths rose to 220 during that year, whereas the average number of deaths was 50 for the preceding and following three years – suggesting that there were about 170 smallpox deaths in 1741. No major epidemics of the magnitude of that in 1741 occurred after that date, although 52 people are listed as dying from the disease in 1781. Thirty people died from smallpox between 1782 and 1803, the result of one or two people dying every year or so. Nothing is known about the history of inoculation in Basingstoke, but presumably it must have been practised on a fairly extensive scale in order to prevent the recurrence of epidemics of the 1741 type. There is other evidence that is similar to that for Basingstoke, which is suggestive rather than conclusive because of the lack of information about inoculation.

Smallpox Mortality at Boston, Lincolnshire, 1749-1802[551]

Period	*Smallpox Deaths*	*Baptisms*	*Smallpox Deaths Per 100 Baptisms*
1749-75	360	2551	14.1%
1776-1802	244	4622	5.3%

Smallpox epidemics occurred regularly every seven or eight years in Boston and therefore most deaths would have been of young children. It is therefore appropriate to express smallpox deaths as a proportion of baptisms, and the reduction from about fourteen to five per cent of all children dying of this disease during the latter half of the eighteenth century was a substantial demographic gain.

There appears to have been a similar decline in smallpox mortality at Chester. About fifteen per cent of all children born died of smallpox during 1772-77 in the town – 369 smallpox deaths in 1772-77, 3970 children baptised in 1764-73[552] – and although there is no exactly comparable information for a later date, the parish register of Holy Trinity, Chester, does suggest that there was a marked decline in

smallpox mortality after the first period 1772-77. During 1787-95 in Holy Trinity, Chester, there were 35 smallpox deaths and 458 baptisms, a ratio of 7.6 smallpox deaths per 100 births, whereas between 1796 and 1802 there were 28 smallpox deaths and 559 baptisms; a ratio of 5 per 100.[553] Holy Trinity was a suburban parish and inhabited by the poor of the town, where smallpox mortality would be expected to be greatest because of their slow acceptance of inoculation.[554] Therefore it appears that smallpox mortality fell in the town from 15 deaths per 100 children born to 5 per 100.

This decline of smallpox in Chester was probably the result of Haygarth's influence, who started a society for inoculating the poor in 1780. Haygarth's society also adopted at his instigation a programme of isolating all smallpox cases, so as to stop the spread of the disease, and he later emphasized this aspect of his work because of the difficulty in getting the poor to accept inoculation in Chester. Haygarth felt there was so much resistance to inoculation that he became somewhat disillusioned with the proposal for general inoculations in the town, and even went on to argue, like Dimsdale, that partial inoculations could be damaging through spreading the natural form of the disease to unprotected people. This was in spite of his belief that inoculation was only a thirtieth to a fiftieth as infectious as natural smallpox. In 1793, he wrote:

"as far as my circle of observation extends, both in England and Wales, this improved method of communicating the distemper [inoculation] has manifestly appeared to be injurious to the poor, though eminently useful to the rich. It has become prejudicial to the community, though human art never bestowed so valuable a blessing as it confers on the few intelligent individuals who adopt it."[555]

Haygarth made it clear that he mainly had town areas in mind when he made this statement – we saw earlier how he had stated in 1785 that "whole villages in the neighbourhood (Chester), and many other parts of Britain, have been inoculated with one consent" – but the evidence considered on Chester itself casts considerable doubt on Haygarth's claim. He himself in his book of 1785 gave the following evidence on the recent history of

inoculation and smallpox in Chester, quoting from the Report of the Small-Pox Society of Chester, dated September 17, 1782:

"Last spring, 128 poor children were inoculated by the numbers of the Smallpox society; these, added to the 85 inoculated in the spring of 1780, made the whole number 213; during the last four years, 203 private patients have been inoculated: in all four hundred and sixteen . . . Taking the whole period of four years, ending March 30, 1782, the Smallpox has been fatal to 139, or 35 annually . . . whereas the annual average of deaths by the distemper for six years previous to the establishment of the society, was 63."[556]

Given what we know about case-fatality rates at this time, the reduction of smallpox deaths from 63 to 35 a year appears to have been entirely due to inoculation, and seems to have been a part of a long-term trend in the reduction of smallpox mortality in Chester. This is not the only evidence to make Haygarth's statement about the damage done by inoculation suspect; smallpox in Chester and other large towns was a young child's disease at this time, which means the disease was endemic. Under these conditions, it was impossible for inoculation to spread smallpox, as it already universally affected all (young) members of the population (we saw earlier that Dimsdale's argument suffered from the same fallacy in London). The reason for Haygarth's critical attitude towards inoculation appears to have resulted from his disappointment at the failure of the policy of general inoculation in Chester and other large towns, along with his belief in the efficacy of isolating smallpox cases so as to contain the disease.

This belief seems to have distorted Haygarth's perception and understanding of evidence, which when looked at carefully, goes against the case he was arguing. He gave in his writings examples of places which he considered had been able to avoid smallpox for very long periods by practising a policy of isolation; he quoted a letter from Howlett stating that the three parishes of Boughton, Hunton and Howlett's own native parish in Kent, had only had 10 smallpox deaths in the twenty-year period 1762-82,[557] but failed to mention Howlett's descriptions of general inoculations in the area.

Similarly, he quoted in 1793 the secretary of the local Chester infirmary on the absence of smallpox in Sussex as follows:

"Mr Connah, secretary of the infirmary, and formerly inspector of the smallpox society at Chester, informs me that both the casual and inoculated distemper are carefully avoided in Sussex. He was a practical surgeon at Seaford in that county . . . The town contains about seven hundred people. He was informed, that, about eleven years ago, one person had died of the smallpox, but could not learn when a like misfortune had happened in the place, antecendent to that period."[558]

We are in a fortunate position with which to evaluate this statement of Haygarth's, as East Sussex was one of the areas which was covered fairly intensively for the present book. We have seen earlier from the letters of Thomas Davies, bailiff to the Glynde estate, and from evidence coming out of general inoculation in places like Lewes and Brighton, that people in Sussex did indeed fear and avoid smallpox as much as possible – but once an epidemic had begun to establish itself, inoculation was rapidly resorted to. Haygarth singled out Seaford as a particular example of a town that had managed to avoid smallpox, yet Davies tells us in one of his letters, that Seaford "are inclined to our scheme" of general inoculation.[559] We do not know whether Seaford did actually carry out such a general inoculation (the relevant local historical records have disappeared), but given that everywhere else in East Sussex was doing so, it is likely that they did as well. Whatever happened at Seaford, it is clear that Haygarth gave his readers a very misleading impression when he wrote that "the casual and inoculated distemper are carefully avoided in Sussex."

It is not only statements of the kind made by Haygarth which have misled historians about the role of inoculation, but also the reliance of certain key statistics – those based on the London Bills of Mortality – which have been quoted, repeatedly, in various writings on the subject. The reliability of these statistics is questionable, with some evidence that the majority of vital events could escape registration.[560] Additionally, as with all statistics, they

can be arranged and interpreted to reach pre-conceived conclusions. Most nineteenth century writers on this subject were supporters of vaccination and opponents of the old inoculation, and used the London statistics to show that smallpox mortality had not declined through the use of inoculation, but on the contrary, they argued, the disease had been maintained through secondary contagion. The fallacy of this argument – that a disease cannot be disseminated (from the point of view of overall mortality) in a place like London, where smallpox was endemic and more-or-less confined to children "under the age of seven" – seems to have escaped all nineteenth century writers, both those for and against inoculation.

One later writer less hostile to inoculation – Dr George Gregory – argued that inoculation did reduce smallpox mortality in London, from 65,383 deaths in 1711-40 to 63,308 in 1741-70 and 57,268 in 1771-1800.[561] Gregory seems to have been unaware that the fairly rapid increase in London's population during this period would have increased the number of susceptibles (young children vulnerable to smallpox), and that to get a true measure of changing mortality it would be necessary to express the number of deaths as a proportion of the number of children. In the following Table, the numbers of smallpox burials are expressed as a proportion of the number of baptisms, attempting to allow for changes in the numbers at risk.

Smallpox Mortality In London, 1740-1829[562]

Period	*Smallpox Burials (Nearest 100)*	*Baptism (Nearest 1000)*	*Smallpox Burials Per 100 Baptisms*
1740-49	20,000	146,000	13.7%
1750-59	19,600	147,000	13.3%
1760-69	22,000	159,000	13.8%
1770-79	22,100	173,000	12.1%
1780-89	17,100	177,000	9.6%
1790-99	16,600	187,000	8.9%
1800-09	13,700	199,000	6.9%
1810-19	8,500	221,000	3.8%
1820-29	7,000	257,000	2.7%

This Table indicates that smallpox mortality fell in London after 1769 until the 1820s when it was a fifth of what it had been during the 1760s and before. The fall was relatively gradual, spread over an extended period of time, and is consistent with the chronology of the practice of inoculation and vaccination. These London statistics are only suggestive of the trend of mortality, given registration problems. Inoculation cannot be said to have nearly eliminated smallpox in London as it did in a place like Maidstone and other provincial towns and villages, although inoculation was practised very extensively in London up until at least 1830, by which time the disease was well under control. Therefore it would appear that inoculation did

significantly contribute to the reduction of smallpox mortality in London during the period 1770-1829 – no mean feat at a time when the disease was increasing in virulence.

In addition to the above statistical evidence, there is other somewhat piecemeal information to suggest that smallpox had all but disappeared in the country at large by the end of the eighteenth and beginning of the nineteenth centuries. In 1776 Dimsdale summarized the effect of inoculation in the town of Hertford as follows:

" . . . within these [last] ten years not six persons have died in Hertford of this disease [smallpox]; whereas before the practice [of inoculation] was so generally adopted, the Small Pox has frequently been epidemic and destroyed a great number of inhabitants . . ."[563]

The historian of Tamworth, Staffordshire, also noted the effect of inoculation on mortality and population in the town:

"Hence, it is evident that a very considerable increase took place in the population of the parish, particularly during the last ten years [of the eighteenth century] . . . The number of baptisms also became more disproportionate to the burials. This was attributed [by the Rev. F. Black] to the better mode adopted for preserving the lives of infants, when inoculation began generally to prevail."[564]

In the Milton Ernest, Bedfordshire parish register the cause of death is given for the years 1783-99, during which period smallpox accounted for only one of the 150 deaths,[565] an insignificant proportion at a time when the average case-fatality rate of the disease was probably twenty per cent and above. Similarly at Horton Kirbie, Kent, a village with a population of about 400 people, there were only six smallpox deaths between 1772 and 1810,[566] and at Whittington, Shropshire, with a population of about 1300, nineteen children died from smallpox in 1775-76, two in 1785, after which there were no more mentions of smallpox deaths.[567] At Selattyn, Shropshire, the number of smallpox burials between 1784-1812 – when the cause of all deaths was recorded – was nineteen and the number of baptisms 778, yielding a smallpox mortality rate of about 2.5

smallpox deaths per hundred births. This very low mortality was not due to the infrequency or absence of smallpox as sixteen of the nineteen deaths took place amongst children under the age of ten years and only one of them was that of an adult [568] Inoculation was probably responsible for the very low smallpox mortality in this village after 1784. At Luton, Bedfordshire, there were only 11 smallpox deaths out of a total of 1694 deaths during 1800-12 – this low mortality could have been due to the introduction of vaccination, but we have seen that there was a very successful general inoculation in the town in 1788, which it was intended to repeat annually.

The literary evidence provides some confirmation of the impact of inoculation on smallpox mortality. The decline of smallpox and the resulting increase in population was first commented upon by a contributor to the satirical periodical *The World* in 1755:

"The world, in general . . . is certainly much over-peopled . . . This inconvenience had in great measure been hitherto prevented, by the proper number of people who were daily removed by the smallpox in the natural way; one, at least, in seven dying, to the great ease and convenience of the survivors; whereas since inoculation has prevailed, all hopes of thinning out people that way are entirely at an end; not above one in three hundred being taken off, to the great encumbrance of society."[569]

The writer of this satirical piece appears to have mainly had "worthy country gentlemen" and their families in mind, for he goes on to describe how they were deserting the countryside for the metropolis where they no longer had anything to fear from smallpox because of inoculation. Nearly twenty years later, Goldsmith included another humorous reference to inoculation in his play *She Stoops to Conquer,* written in 1773. Mrs Hardcastle says to Hastings:

"I vow, since Inoculation began, there is no such thing to be seen as a plain woman. So one must dress a little particular, or one may escape in the crowd."[570]

Obviously this is not the most reliable form of evidence, but it does suggest that smallpox was beginning to disappear amongst the wealthy and fashionable classes. More reliable evidence of this change is to be found in reports sent to Haygarth which he discussed in his book published in 1793:

"Several respectable Correspondents have declined to give a decided opinion on this subject [of smallpox], from want of opportunity to make observations in their own practice. A physician of the greatest eminence both in rank and erudition gives the following very sufficient reason for his silence on this point. 'In London we have very few opportunities of seeing the smallpox. For the last five and twenty years, the number of variolous patients, who have fallen under my care, is very inconsiderable.' Another distinguished physician and author in a large city says, 'I have not seen six private patients in the smallpox In eighteen years . . .' "[571]

Clearly, this would only apply to the wealthy who were the employers of the "private" and "distinguished" physicians. Nevertheless it does suggest that smallpox had virtually disappeared as a disease amongst the wealthy by about 1770.

The first person to discuss at any length in print the effect of inoculation on smallpox mortality and general population was the Reverend John Howlett, who wrote on many economic and demographic subjects. Howlett was Vicar of Great Dunmow, Essex, for many years and was resident in Maidstone, Kent at different times. He was in a particularly good position to know about the effects of inoculation in the country as a whole as a result of his demographic studies. In September 1782 he described in the *Gentleman's Magazine* the nature of an enquiry he was engaged in:

" . . . during the last twelve months I have sent out between 3 and 4000 written letters and printed papers to the clergy in different parts of the kingdom, in which I have ventured to solicit not only register extracts for different periods in their respective parishes, but likewise, wherever conveniently attainable, actual surveys of the people, together with many curious, perhaps important information."[572]

Included in this "important information" were references to inoculation, an example of which Howlett quoted in a book by him published in 1781:

"A striking instance to the same purpose in the parish of Great Chart, near Ashford in the county of Kent, has been sent me. Its burials in a period of twenty years immediately subsequent to the revolution [1638-1708] were 192 – but almost 100 of them were occasioned by the smallpox; whereas in 20 years beginning with 1760, there appears to have been only 4 or 5 who died of that disorder. This diminution my ingenious correspondent imputes to inoculation, and adds 'that no register can, as yet, properly inform us of the thousands that have been preserved by this salutary practice for these 20 years past all over the kingdom . . .' "[573]

In the same publication, Howlett summarized the position on the effects of inoculation as he understood it:

" . . . the diminished mortality of . . . provincial towns and villages . . . appears to be chiefly owing to the salutary practice of inoculation . . . where two or three hundred used to be carried [off by smallpox] to their graves in the course of a few months, there are now perhaps not above 20 or 30."[574]

Howlett reached this conclusion before making his more extensive enquiry into population during 1781-82, some of the results of which were published anonymously in a pamphlet by him on changes in the population and health of the town of Maidstone, Kent. This was published in 1782 and Howlett summarized his conclusions vis-a-vis smallpox, inoculation and population in Maidstone as follows:

"Upon casting an eye over the annual lists of burials we see, that, before the modern improved practice of inoculation [the Suttonian method] was introduced, every five or six years the average number was almost doubled; and it was found upon enquiry, that at such intervals nearly the smallpox used to repeat its dreadful

periodical visits . . . In the short space of 30 years it deprived the town of between five and six hundred of its inhabitants; whereas in the 15 or 16 years that have elapsed since that general inoculation [in 1766] it has occasioned the deaths of only about 60. Ample and satisfactory evidence of the vast benefit the town has received from the salutary invention! And it appears, with a high degree of probability upon proofs similar to the above, that, from the same causes, in the kingdom at large not less than 4 or 500,000 lives were lost in the former of the periods now stated, and that nearly half that number had been saved in the latter . . . This [diminution of the death rate in Maidstone] may . . . be ascribed . . . principally and chiefly to that distinguished blessing of providence, inoculation."[575]

According to Howlett, the radical decline in smallpox mortality in Maidstone after 1766 due to the use of inoculation, was characteristic of many other parts of the country, in most "provincial towns and villages."[576]

During the last twenty years of the eighteenth century it was very common for non-medical writers to note the role of inoculation in reducing smallpox mortality and therefore leading to population expansion. For example, Arthur Young the agriculturalist wrote in 1781:

"In several of these parishes where population had for some periods been rather on the decrease, a great change has taken place lately, and the last ten years are found to be in a rapid state of progression; as considerable drains of men have been made from almost every parish in the kingdom for the public service in that period, I should not have expected this result, and know nothing to which it can be owing, unless the general prevalence of inoculation, which certainly has been attended with a very great effect."[577]

There are also references to the effects of inoculation on mortality in the reports on agriculture to the Board of Agriculture at the end of the eighteenth century. Plymley of Shropshire wrote before the end of the century:

"I may further add, that since the year 1782, when these observations were made, the population of this parish has been increasing: most certainly the inoculation for the smallpox . . . has been most essential to population throughout this kingdom."[578]

Similarly John Holt of Lancashire wrote in 1795:

"One reason why persons in large manufacturies in Lancashire do not frequently die in great numbers . . . is that they have (in general) been inoculated in their infancy. Inoculation is the most effectual of all expedients for preserving the short-lived race of men – many gentlemen pay for the inoculation of the children of the poor in their own neighbourhood."[579]

These observations on the effect of inoculation were made in passing, as most writers on agriculture did not consider the causes of population increase central to their subject. This makes this type of evidence all the more impressive, as it was unsolicited and cannot be explained as a function of partisan interest. Both Plymley and Holt agreed that inoculation was very important in their counties in diminishing smallpox mortality and increasing population.

Heysham's account of the impact of inoculation on smallpox mortality in the Carlisle Bills of Mortality has already been discussed in some detail. He summarized the effect of inoculation on mortality and population as follows:

"Inoculation, I am persuaded, has also greatly contributed to the increase of population, not only in Carlisle, but likewise in the whole county of Cumberland. In the year 1779, when the lower class of inhabitants [of Carlisle] were extremely averse to this salutary discovery, no fewer than ninety persons died of the natural smallpox; whereas only 151 have died during the eight succeeding years; which is, upon average, not quite nineteen in each year; and yet the disorder prevailed in every one of these years . . ."[580]

This summary is a useful overall picture of the scale of the effect of inoculation on smallpox mortality in Cumberland; the figures of

smallpox cannot be taken too literally as evidence for the decline of mortality, as the particular years discussed may have been untypical, and 1779 may have been a particularly fatal year. The statistics quoted are valuable however for illustrating Heysham's belief about the impact of inoculation on smallpox mortality in the Carlisle area, an effect which was general to the whole of Cumberland.

The impact of inoculation on smallpox mortality and population was noted not only at the local and county level, but also for the country as a whole. A contributor to the *Gentleman's Magazine* argued in the February 1796 edition:

"The increase of people within the last 25 years is visible to every observer . . . Inoculation is the mystic spell that has produced this wonder. Some time between 1738 and 1743 (I speak from memory), the smallpox was so severe at St Edmundsbury, that the assizes were twice, if not three times, held at Ipswich; which supposes a continuation of 13 months. During that term, it was said, that the town had been deprived of a sixth part of its inhabitants: there were no markets, and the town was avoided as the seat of death and terror. This was no more than a common calamity at that time . . . so that it may be safely asserted, that this malady [smallpox], added to the general laws of nature did at the least equipoise population . . . It is now 30 years since the Suttons, and others under their instructions, had practised their skill in inoculation upon half the kingdom, and had reduced the risk of death to the chance of one in 2000. Hence the great increase of people . . ."[581]

This general statement was based on personal experience and observation, although it suffers from being too impressionistic, particularly with reference to the effects of inoculation on population growth. A similar type of statement was made by a contributor to the *Gentleman's Magazine* in 1803, quoted at the very beginning of this book. A part of this statement – concerning the "saving of lives" through inoculation – was questioned by the Editor of the Magazine in a footnote: "On this head Doctors materially differ."[582] The Editor was referring to the criticisms levelled against inoculation by the medical supporters of vaccination,

based on abstract *a priori* medical arguments – that it gave rise to secondary contagion – and it is, these criticisms which have led medical historians subsequently to neglect the empirical study of the role of inoculation in the diminution of smallpox mortality. The contributor to the *Gentleman's Magazine* in 1803 was not to be misled by theoretical objections and replied to the Editor's footnote at length in the next edition:

"Of the proportion of deaths in the Natural Small Pox, I have had ocular demonstration, both in the North and West of England, more especially in country villages, the miseries of our large cities and towns . . . Of the great success of Inoculation with the matter of the Smallpox, I have read some accounts; but have had many more from various medical gentlemen; of whom, some have visited Ireland professedly for that purpose, and others have formed establishments in various parts of the kingdom. A gentleman, of but little medical knowledge, was, some years since, established in this peculiar branch of the profession, who was in the habit of inoculating whole parishes, at a very moderate stated price. It is scarce 20 years since I first became acquainted with some of the family; at which time, they had inoculated near 15,000 persons, mostly in villages and small towns, and in a few years afterwards the number was considerably more than 20,000. From persons well acquainted with the practice, it was agreed, that not one in a thousand of their patients miscarried. This was on the very boundry of Wiltshire and Hampshire, and is well known to every person then resident in its vicinity. . ."[583]

The substance of this correspondent's argument was that nearly everyone caught smallpox before the introduction of inoculation and of these between a fifth and a quarter died; due to inoculation, which became very widespread, only one of every 1000 persons died (after inoculation), leading to an enormous saving of lives, sufficiently great to explain the increase in population during the latter half of the eighteenth century. Again this argument has the advantage of being based on personal experience and observation, but also lacks any detailed statistical estimate of the significance of reduced smallpox mortality on population growth.

These contemporary writers were unaware of course that disease could indirectly effect fertility as well as mortality. The work of Phadke and his colleagues would lead us to expect smallpox to have had a significant impact on the history of fertility. Reliable figures on changes in fertility among the general population are not available for the relevant period, but T. H. Hollingsworth in his monograph on the demography of the British Peerage has calculated figures for fertility from the sixteenth century onwards. He has summarized the conclusions relevant to the present argument as follows:

" . . . fertility of the cohorts born between 1550 and 1724 apparently fell from nearly 5 children per married adult to only 3.5 . . . Hence it would appear that from about 1590 to about 1740 there was a fairly steady decline in fertility . . . the trend was arrested (say 1740) . . . [and] mean family size rose from 3.5 to almost 5 again between . . . 1740 and 1815."[584]

This historical pattern of fertility seems to coincide almost exactly with that of smallpox mortality; up until 1740 or thereabouts, smallpox mortality was increasing as a result of the growth in the virulence of the disease, and for the aristocracy who adopted inoculation earlier than the general population, smallpox mortality probably fell from the 1740s onwards. The increasing mortality of the disease up to 1740 would influence fertility as the more severe forms of smallpox would probably have created a greater number of focal lesions in the epidydimis. This would be analogous to severer types of smallpox producing larger crops of skin lesions. After 1740, with the practice of inoculation, the frequency of the focal lesions in the epididymis would decline, and fertility would gradually increase. We would expect to find from Phadke's findings that childlessness grew during the period of increasing smallpox mortality, and Hollingsworth's figures do show an increase for both men and women from about fourteen per cent in the middle of the seventeenth century to about twenty-three per cent by 1740, after which it declines to eighteen or nineteen per cent at the end of the century.[585]

There is a certain degree of uncertainty about the reliability of these figures on childlessness, and it appears from Phadke's work that the focal lesions produced by smallpox both reduce the degree of fertility through bringing about oligospermia (both severe and moderate), as well as creating infertility through azoospermia.[586] The extent to which this analysis of smallpox and fertility is applicable to the general population is unknown; at the present we lack sufficiently reliable figures to come to any firm conclusions.

Returning to the problem of the reduction of smallpox mortality, the first national figures for England and Wales only become available after 1837 with the introduction of civil registration. Given what we know about the case-fatality rate of smallpox at this time, it is possible to use the civil registration figures to reach firm conclusions about the effectiveness of prophylactic measures at this time. Both inoculation and vaccination were still being practised, and as they were probably both variants of the same operation – the inoculation of smallpox virus (with vaccination being the more attenuated form) – it is appropriate to evaluate their joint effectiveness. As about 75 per cent of all smallpox deaths in England and Wales in 1839 occurred in children under five (about 87 per cent under ten),[587] it is appropriate to express smallpox mortality as a proportion of births.

Smallpox deaths accounted for approximately 1.5 per cent of all children born in England and Wales during 1838-44,[588] and this was the highest smallpox mortality ratio recorded under civil registration (i.e. mortality ratios were even lower in subsequent periods).[589] This level of mortality was of course insignificant compared to some of the ratios for the pre-inoculation period. In Ireland during the 1830s, smallpox mortality was as low as 2.2 smallpox deaths per 100 births,[590] and this was almost certainly achieved primarily through inoculation rather than vaccination, because at this time "a large proportion of the peasantry in the country parts" were "in favour of inoculation,"[591] and it was in the large majority living in the countryside that smallpox was at its lowest, as can be seen in the following Table.

Smallpox MortalityIin Ireland, 1831-40[592]

	Smallpox Deaths (1831-1840)	*Population (1841)*	*Annual Average Smallpox Deaths Per Million Living*
Civic Districts	12,418	1,135,465	1093
Rural Districts	45,459	7,039,659	647

The lower mortality in the rural districts was not a function of the proportion of the population catching the disease, as smallpox was a disease of young children in Ireland at this time – about 49,000 of the total 58,000 smallpox deaths occurred under the age of five[593] – and in both rural and urban areas most children had either been inoculated or vaccinated, or caught the disease by the age of five. The total smallpox mortality rate for Ireland during 1831-40 was 710 annual deaths per million living in 1841, a very low mortality compared with that for the pre-inoculation period. For example, in Dublin during 1661-90 there were about 8600 smallpox deaths per million living.[594]

Smallpox mortality for both Ireland and England and Wales was insignificant when set alongside the case-fatality rate of the disease. We have already seen that this lay somewhere between the 23 per cent in the Norwich 1819 epidemic and the 49.5 per cent for the 1887-88 Sheffield one. Smallpox epidemics on the Continent during this period yielded similar fatality levels: 57.5 per cent of all unvaccinated cases in the 1828 Digne (France) epidemic died, and of the 10,246 unprotected people who caught smallpox in Milan during 1830-51, 38.3 per cent died.[595] This latter figure refers to the two decades for which the civil registration figure of smallpox mortality in England and Wales was calculated, and given the large numbers on which it is based, it is an appropriate statistic with which to evaluate that mortality (it is also what would be expected from the trend of British case-fatality figures). Thus only 1.5 per cent of all

children born in England and Wales died of smallpox in 1838-44, when the case-fatality rate was 38.3 per cent. Given that smallpox was a universal disease at this time, except for a five per cent minority with natural immunity, the saving of life revealed by these figures is highly significant.

Without prophylactic measures against smallpox, something like between a quarter and a third of the population would have died directly from smallpox in the post-civil registration period. In addition to this, many more people would have died from secondary broncho-pneumonia and other complicating diseases, and fertility would probably have been depressed to a significantly reduced level. It is no exaggeration to say that inoculation and vaccination prevented the decimation of the population of the kind that Europe suffered in the fourteenth century onwards, and instead of the rapidly expanding economy of the nineteenth century which we label the Industrial Revolution, there would have been a very prolonged period of decline and stagnation. Inoculation and vaccination were developed through folk medicine and accidental discovery, but they were medical measures unrivalled in their impact on health and mortality in the history of medicine.

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[P=Pamphlet]

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548. For the source of these statistics see *Maidstone All Saints Parish Register* lodged in the Church. After 1753 smallpox deaths are individually registered, but before this date the register simply states "small-pox now in this town" during epidemic years – thus the number of smallpox deaths for the period 1740-51 is an estimate based on changes in the total number of burials during epidemic years. The increase in the total number of burials during the whole period is a function of increasing population, e.g the population was 5755 in

1782 and 8027 in 1801.

549. See the *Calne Parish Register* in the Wiltshire Archaeological Society Library at Devizes.
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594. This is using Petty's population figure of 55,000 for Dublin; undoubtedly this is an underestimate, but some deaths were not registered so that the two under-enumerations appear to cancel each other out, giving a death rate of about 40 per 1000, a not unreasonable figure for a city the size of Dublin during this period. The decline of smallpox mortality in Dublin is indicated by the comparison of 1661-1745 when about twenty per cent of all deaths were due to smallpox with 1831-40 when it accounted for under three per cent.

595. See *Royal Commission on Vaccination,* 1st Report (1889), pp. 74, 215; 6th Report (1896), pp. 717, 718, 720; Edwardes, *op. cit.*, p. 55.

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